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EDITED BY

KARL M. DALLENBACH
University of Texas

AND

M. E. BITTERMAN
Bryn Mawr College

E. B. NEWMAN
Harvard University

WITH THE COÖPERATION OF

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# AN EDITORIAL ANNOUNCEMENT

We announce with deep regret the resignation of Professor Leo Postman from his co-editorship of this Journal, effective with the December, 1963, number.

His decision to reduce his extensive editorial responsibilities was prompted by the need to devote more time and energy to his own productive work.

His services will be grievously missed by his colleagues on the Journal and by the many contributors whose manuscripts he received and edited.

Karl M. Dallenbach
M. E. Bitterman
E. B. Newman

# SPATIAL LOCALIZATION BY THE BLIND

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By GERALD H. FISHER, University of Newcastle Upon Tyne, England

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This research is concerned with the ability of blind people to orientate themselves within, and to localize aspects of, their spatial environment. Since vision, the most accurate kind of spatial information, is not available to the blind, they must base their localization-judgments upon information which is relatively less reliable. Nevertheless, the ability of blind people to perceive objects often appears quite uncanny to the sighted observer and it has variously been supposed that they have either developed their remaining senses to a higher degree of acuity than sighted people or have developed an extra sense of some kind with which they are able to localize stimuli in space.[1] After vision, the most important of the spatial senses are tactile-kinesthesis (hereafter referred to as 'tactile') and audition.

There is some uncertainty regarding the relative accuracies with which auditory and tactile stimuli can be localized since, to our knowledge, no directly comparable threshold-estimates have been made. Stevens and Newman, however, estimated a value of approximately 12° of angle for the average error of localization of sound-sources in a situation in which the *Ss* were required to make absolute judgments of the apparent positions of sounds presented in the horizontal plane.[2] More recently, Mills has estimated a threshold-value of about 1° of angle for tones of 1000 c.p.s.[3] Fitts estimated an average error of about 4° of angle in a situation in which the *Ss* were required to reach out to touch targets with their index fingers.[4] In both the Stevens and Newman and the Fitts studied, the *Ss* made their judgments according to what might be called a 'remembered visual context,' and we suspect that a very wide range of threshold-values may be obtained when using such a method, depending upon how well the ordinates of the visual context have been learned or are recalled. In the Fitts experiment, moreover, strong visual anchoring stimuli in the form of two red lights placed in the frontal plane were present throughout. The presence of these stimuli insures that the judgment being made is not 'intrasensory' but 'intersensory,' the tactile stimulus being localized with respect to the visual.

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* Received for publication June 23, 1962.

[1] C. H. Amons, Philip Worchel, and K. M. Dallenbach, "Facial vision": The perception of obstacles out of doors by blindfolded and blindfolded-deafened subjects, this JOURNAL, 66, 1953, 519-553.

[2] S. S. Stevens and E. B. Newman, The localization of actual sources of sound, this JOURNAL, 48, 1936, 297-306.

[3] A. W. Mills, On the minimum audible angle, *J. acous. Soc. Amer.*, 30, 1958, 237-246.

[4] P. M. Fitts, A study of location discrimination ability, *Psychological Research on Equipment Design*, 1947, 202-217.

Mills used what we shall call the 'method of successive presentation,' in which a tone is first presented in a standard position and then again after being displaced by a small distance to the left or right, S's responses' being made according to the apparent relative positions of the two stimuli. Such a method inevitably introduces a 'time error' and as far as we know no research in audition has been conducted to estimate the extent of such errors for different time-intervals' intervening between presentation of standard and variable stimuli. The Mills estimate of the auditory threshold probably involves an 'anchoring' effect also, since he used the method of constant stimuli, but this would not be a major objection if it is borne in mind when making comparisons between this and other methods of estimation. It is intended to consider these and other problems involved in estimating thresholds in a subsequent paper. Furthermore, in none of these studies was any attempt made to eliminate the proprioceptive context in which the Ss were placed and it therefore would appear that the judgments made in each case were not truly 'intrasensory.' Finally, each of the methods used in these studies is sensitive to the interval or step-size between stimuli and if this is badly chosen, it is possible to obtain almost any value for the threshold.

Specifically, the purpose of the present investigation is to measure the ability of blind *Ss* to localize stimuli by sound and by touch, and to attempt to determine the nature of the information they utilize. The above considerations lead us to suggest that it is essential, in designing experiments for these purposes, (1) to use an appropriate stimulus-increment or step-size; (2) to control time-errors and 'anchoring' effects by using appropriate psychophysical procedures; and (3) to exclude 'spatial contexts' available in modalities other than the particular one under investigation.

## METHOD AND PROCEDURE

*Subjects.* Five blind *Ss* were used. Three of these had been blind from birth and were unable to distinguish between conditions of dark and bright sunlight. Two *Ss* had had some pattern-vision until the age of about 6 yr.; one of these was probably able to distinguish between darkness and very brightly lighted conditions, the other was almost certainly unable to do so. Two *Ss* were left-handed, the others right-handed; they had no obvious auditory defects. All of the blind *Ss* were employed in the workshops of The Blind Institute, Kingston-upon-Hull, England. Five sighted *Ss* were used as a comparison or control group; these were all university students who had no obvious visual or auditory defects.

*Apparatus.* The apparatus (see Fig. 1) has been described fully elsewhere.[8] Briefly, it consisted of a system of three bearings, each having the same center of rotation. Each of these bearings could be adjusted with an accuracy of ±10″ of angle in the horizontal plane. Mounted on arms attached to two of the bearings, for the present experiment, were a small hearing-aid earphone and a tactile-stimulus. The tactile-stimulus was effectively a soundless switch to which *S*'s index finger was guided by means of two brass rods fitted in the shape of a 'V' on either side of it. This switch was so connected into an electronic circuit that either the auditory stimulus could be presented immediately it was touched or it could be presented independently. Control-rods attached to the bearings enable the arms on which the stimuli were mounted to be moved through an angular distance of 90°. Since a turntable upon which the *S*'s chair was mounted could be rotated through 90° also, the total range of the apparatus was about 180°. The apparatus was mounted in an anechoic and partially sound-proofed room. Both blind and sighted *S*s wore dark goggles thrhughout the experiment. White noise was always used as the auditory-stimulus.

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[8] G. H. Fisher, Space perception in conditions of intersensory stimulation, 1962, Doctoral dissertation, University of Hull.

## EXPERIMENT I

Our first task was to determine the relative ability of the blind *S*s to localize both tactile and auditory stimuli. To do this, the 'method of successive presentation' was used in which a stimulus was presented first in a standard position and again after an interval of 2 sec. during which time it had been moved into another position. The *S*s were required to judge the apparent relative positions of the two stimuli. In an independent series of experiments, in which the extent of time-errors was estimated for intervals between 1–30 sec., it was found that these errors were not appreciable until some 4 or 5 sec. had elapsed between presentations.

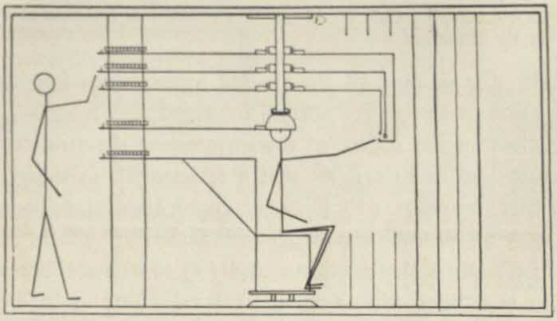

FIG. 1. SCHEMATIC DIAGRAM OF THE EXPERIMENTAL APPARATUS

*Procedure.* The *S*s were seated comfortably in the apparatus and instructed as follows:

*Instructions.* I am interested to know how well you are able to localize the positions of auditory and tactile stimuli. This is the auditory stimulus [sound switched on]. If you will now reach out the index finger of your preferred hand and run it down between these guide wires you will feel the tactile stimulus. I shall let you hear or ask you to touch the first stimulus, after this either wait or drop your hand back into your lap. I will then change the position of the stimulus and allow you either to hear it or ask you to reach out to touch it again. All you have to do is tell me which of the stimuli, first or second, appears to you to be farther to the left.

All the *Ss* were given 20 trials in each condition before the experiment proper was started. During these practice trials, it was found that all the *Ss* were able to reach out and to touch the tactile stimulus within a time-interval of 2 sec. The auditory stimuli were accordingly, presented at 2-sec. intervals also. To avoid building-up 'spatial-contexts' five positions of each *standard* stimulus were used; associated with each standard position were five positions of the *variable*. Selection of each standard and variable was made by entering two series of random numbers, one for scale and thus standard, the other for variable. The data were collected in the form of frequency-distributions of responses over the five positions of each variable; they were analysed by Spearman's distribution-method.

*Results.* The results of Experiment I are shown in Table I. In this table are entered the *variances* for both auditory and tactilelocalization judgments. Mean variances for blind and sighted *Ss* have been estimated and compared by calculating the *F*-ratio between groups. For neither auditory nor tactile localizations is this value significant and, in fact, from a satistical point of view, these data could be pooled and considered as having been drawn from a single population of *Ss* or responses.

TABLE I

VARIABLE ERRORS (VARIANCES) OF A GROUP OF BLIND AND SIGHTED *Ss* FOR BOTH AUDITORY AND TACTILE LOCALIZATIONS IN INCREMENTS OF 100 MINUTES OF ANGLE

| Auditory | | | | Tactile | | | |
|---|---|---|---|---|---|---|---|
| Blind | | Sighted | | Blind | | Sighted | |
| *S* | $\sigma^2$ | *S* | $\sigma^2$ | *S* | $\sigma^2$ | *S* | $\sigma^2$ |
| *JC* | 3.76 | *AC* | 2.66 | *JC* | 1.41 | *AC* | 1.02 |
| *DF* | 2.87 | *AL* | 3.42 | *DF* | 0.52 | *AL* | 0.85 |
| *AS* | 2.59 | *RW* | 3.22 | *AS* | 0.96 | *RW* | 0.79 |
| *CP* | 3.26 | *FG* | 2.86 | *CP* | 0.83 | *FG* | 1.36 |
| *PS* | 3.49 | *PC* | 2.93 | *PS* | 1.11 | *PC* | 1.42 |
| | 3.19 | $\bar{X}$ | 3.02 | $\bar{X}$ | 0.97 | $\bar{X}$ | 1.09 |
| *F* | | 1.06 | | | | 1.12 | |
| *p* | | >0.05 | | | | >0.05 | |

*Head-turning.* The results of Experiment I suggest that there is no difference in the ability of blind and sighted *Ss* to localize either auditory or tactile sources using these particular stimulus-conditions and this type of response. It should, however, be borne in mind that the response required was somewhat artificial. It is more usual, when making judgments of auditory localizations in particular, for people to turn their heads toward the apparent source of stimulation; this is also to some extent the case when making judgments of tactile localizations. It is, therefore, possible that if a more natural response could be used, such as allowing *Ss* to turn their heads toward the source of stimulation, blind *Ss* may prove to be more accurate than sighted *Ss*.

## Experiment II

Built into the apparatus described above was a helmet mounted on a very freely running ball-race bearing. By fixing *S*'s head firmly into this helmet and by fitting a light aluminum pointer onto the back of the helmet, it was possible to observe the angular position of his head in the horizontal plane on an additional scale built into the apparatus.

*Procedure.* *S* was seated comfortably in the apparatus with his head fitted firmly in the helmet by means of screws which tightened a caliper mounted inside the padding. He was then instructed as follows:

TABLE II

Variable Errors (Variances) of a Group of Blind and Sighted *Ss* for Both Auditory and Tactile Localizations in Increments of Degrees of Angle Using a 'Head-Turning' Response

| Auditory | | | | Tactile | | | |
|---|---|---|---|---|---|---|---|
| Blind | | Sighted | | Blind | | Sighted | |
| *S* | $\sigma^2$ | *S* | $\sigma^2$ | *S* | $\sigma^2$ | *S* | $\sigma^2$ |
| *JC* | 6.41 | *AC* | 8.21 | *JC* | 6.42 | *AC* | 7.11 |
| *DF* | 9.36 | *AL* | 6.79 | *DF* | 5.17 | *AL* | 8.93 |
| *AS* | 8.11 | *RW* | 9.84 | *AS* | 6.97 | *RW* | 4.62 |
| *CP* | 10.02 | *FG* | 11.76 | *CP* | 9.93 | *FG* | 6.17 |
| *PS* | 8.75 | *PC* | 7.44 | *PS* | 3.85 | *PC* | 6.29 |
| | 8.53 | | 8.81 | | 6.47 | $\overline{X}$ | 6.62 |
| *F* | | 1.04 | | | | 1.02 | |
| *p* | | >0.05 | | | | <0.05 | |

*Instructions.* I am interested to know how well you can localize both sound- and touch-stimuli, as before. This time all I wish you to do is either to listen to the sound or reach out to touch the tactile stimulus. As soon as you hear or touch the stimulus, simply turn your head directly toward it.

Twenty trials in each condition were given preceding the experiment proper. It was found convenient to present the auditory stimulus for 1 sec. and this period appeared to be sufficiently long for *S* to turn his head toward the tactile stimulus. The increment between stimuli presented was 5°; thus there were 17 positions of presentation. Each stimulus-position was presented 6 times in randomized order, with 102 localizations in all. In an attempt to break down any proprioceptive context, *S* was rotated very slowly on the turntable over the whole range of random positions before every stimulus was presented. The data were collected in the form of error-scores in degrees; errors for localizing to the right of the actual stimulus-position were scored as positive, errors to the left were scored as negative.

*Results and discussion.* The results of Experiment II are shown in Table II and III. In Table II the variances of error scores are entered for both auditory and tactile localizations. The mean variances for both blind and sighted groups have been estimated. Again the F-ratio indicates that there are no significant differences between the deaf and sighted *S*s in their ability to localize auditory and tactile stimuli. In each modality, however, the blind group is very slightly superior but whether this difference is real could only be considered by repeating the experiment with a much larger group of *S*s.

In Table III *constant errors* of the localizations are entered, the mean values having been calculated irrespective of sign. Here interesting differences between the two groups of *S*s are revealed that are very striking. After the *S*s are fitted into the apparatus, the sighted *S*s tend to keep their heads straightforward; the blind *S*s to turn their heads through a large angle and to keep them displaced throughout the experiment. The over-all effect of this is that constant errors for both auditory and tactile localizations are relatively larger for the blind *S*s.[6]

TABLE III

CONSTANT ERRORS OF A GROUP OF BLIND AND SIGHTED *S*S FOR BOTH AUDITORY AND TACTILE LOCALIZATIONS IN INCREMENTS OF DEGREES OF ANGLE USING A 'HEAD-TURNING' RESPONSE

| Auditory | | | | Tactile | | | |
|---|---|---|---|---|---|---|---|
| Blind | | Sighted | | Blind | | Sighted | |
| *S* | *CE* | *S* | *CE* | *S* | *CE* | *S* | *CE* |
| *JC* | −13.85 (0.25) | *AC* | +5.07 (0.29) | *JC* | − 9.58 (0.25) | *AC* | −3.62 (0.27) |
| *DF* | −18.72 (0.30) | *AL* | −1.26 (0.26) | *DF* | −12.47 (0.23) | *AL* | +2.42 (0.30) |
| *AS* | +20.33 (0.29) | *RW* | −3.58 (0.31) | *AS* | +10.08 (0.26) | *RW* | −0.23 (0.22) |
| *CP* | −10.06 (0.32) | *FG* | +1.93 (0.34) | *CP* | − 8.41 (0.32) | *FG* | −1.62 (0.25) |
| *PS* | +13.08 (0.30) | *PC* | +7.29 (0.27) | *PS* | + 6.00 (0.20) | *PC* | +4.67 (0.25) |
| X | 15.19 (0.29) | X | 3.83 (0.30) | X | 9.31 (0.25) | X | 2.51 (0.26) |

*Intersensory localizations.* Blind people appear to be able to orientate themselves in space using a combination of auditory and tactile cues; for them, audition seems to be something more than an 'attention' mechanism, which it is very largely with sighted people. Hunter has suggested that the spatial senses remaining to the blind are more *'tightly organized'* than the same senses of the sighted.[7] One way of considering the hypothesis relevant to such organization would be to consider the *'intersensory'* localization ability of blind *S*s in a situation in which stimuli in *different* spatial senses are localized mutually.

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[6] Although this phenomenon has not, to our knowledge, been described previously, it is one which is clearly evident when observing blind people in work-situations in which they tend to turn their heads in such a way that one ear is facing sources of sound more directly than the other. This same technique for assisting localization may occasionally be observed when sighted people are *straining* to hear a sound-source. These results appear to be evidence for 'ear-dominance' which, although not being a perceptual mechanism proper, arises from a technique which is used characteristically by blind people in localizing sounds and which appears to be similar to eye-dominance. We have some evidence that there are measurable differences between the acuity of the ears, but were unable to make accurate audiometric tests on this particular sample of blind *S*s.

## EXPERIMENT III

In Experiment III the *S*s were placed in the apparatus and instructed as follows:

*Instructions.* I am interested to know how well you can localize stimulus-cues of different kinds presented at the same time. If you reach out the index finger of your preferred hand you will feel the tactile stimulus, immediately you touch it you will also hear a noise. All you have to do is tell me which of the two stimuli is further to the left. All you need to say is 'touch' or 'sound.' You will hear the sound for 1 sec., when it stops, drop your hand back into your lap.

Twenty trials were given to insure that the *S*s understood the instructions before the experiment proper was started. The method used for presenting the stimuli was especially developed for investigating intersensory localizations. First, the technique used for breaking-down spatial context-effects which has been described above was used. In addition to this, it was necessary to measure and offset intersensory constant errors, since distributions of intersensory localizations are not normal for *S*s with appreciable constant errors. This was done by the 'staircase' method of limits that the extent of the constant error would be known.[8] The procedure was then continued by the method of constant stimuli, using tactile and auditory stimuli, in turn, as standard and variable to observe the effects of any residual spatial contexts. Since intersensory constant-errors fluctuate in time (*i.e.* are *not* constant), it is necessary to repeat this procedure at least every 50 trials. As far as we know, this is the only acceptable procedure that can be used for investigating intersensory localizations, since it insures that distributions of response judgments are essentially normal and remain so in spite of fluctuating constant-errors.[9] This method also allows consideration of sources of non-randomness in sequences of response-judgments, a problem which will be the subject of a subsequent paper.

*Results and discussion.* The results of Experiment III are shown in Tables IV and V. They appear to indicate an extra-chance difference between the abilities of blind and sighted *S*s in their ability to make intersensory localizations. If this difference, however, is real, it is in a direction

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[7] I. M. L. Hunter, Tactile-kinaesthetic perception of straightness in blind and sighted subjects, *Quart. J. exp. Psychol.*, 6, 1954, 149-154.

[8] T. N. Cornsweet, The staircase-method in psychophysics, this JOURNAL, 75, 1962, 485-491.

[9] Fisher, A suggested procedure for threshold estimation, *Bull. Brit. Psychol. Soc.*, 16, 1963, 5A-6A.

*opposite* to that which would be predicted on any hypothesis which implied that blind people are more able to localize spatial stimuli than sighted people. The results could probably be explained on the basis of the effects of the conditions of reduced sensory input in which these experiments were conducted, but in the absence of any detailed evidence relating to the effects of reduced ambient stimulation upon blind people, this does not appear to be very convincing. We reaffirm, however, that there is a lack of congruence between these two spatial senses; since there are statistically significant differences between the apparent positions of stimuli in the auditory and tactile senses which are physically in the same place, in the case of four blind and four sighted *S*s. For one blind *S* (*C P*), the difference is greater than 9° of angle. This implies that auditory and tactile stimuli which are actually presented in the same position appear to be displaced, sometimes by extremely large angular distances.

TABLE IV

VARIABLE ERRORS (VARIANCES) OF A GROUP OF BLIND AND SIGHTED *S*S FOR TACTILE-AUDITORY INTERSENSORY LOCALIZATIONS IN INCREMENTS OF 100 MINUTES OF ANGLE

| Blind | | | Sighted | |
|---|---|---|---|---|
| *S* | $\sigma^2$ | | *S* | $\sigma^2$ |
| *JC* | 2.44 | | *PW* | 11.60 |
| *DF* | 1.17 | | *CW* | 0.30 |
| *AS* | 0.62 | | *JF* | 3.31 |
| *CP* | 240.00 | | *AR* | 38.50 |
| *PS* | 510.00 | | *DH* | 0.85 |
| $\overline{X}$ | 150.85 | | $\overline{X}$ | 10.91 |
| *F* | | 13.8 | | |
| *p* | $>0.025$ | | | $<0.010$ |

TABLE V

CONSTANT ERRORS (MEANS) OF A GROUP OF BLIND AND SIGHTED *S*S FOR TACTILE-AUDITORY INTERSENSORY LOCALIZATIONS IN INCREMENTS OF 100 MINUTES OF ANGLE

Means represent mean *CE* irrespective of sign. Figures in brackets are the *SE* of estimate of the individual *CE*.

| Blind | | Sighted | |
|---|---|---|---|
| *S* | $\overline{X}_{Tk-A}$ | *S* | $\overline{X}_{Tk-A}$ |
| *JC* | −2.46 (0.15) | *PW* | +1.46 (0.34) |
| *DF* | +0.13 (0.11) | *CW* | +1.85 (0.05) |
| *AS* | −1.87 (0.08) | *JF* | +0.52 (0.18) |
| *CP* | −5.66 (1.55) | *AR* | −3.23 (0.62) |
| *PS* | −1.23 (2.26) | *DH* | −2.69 (0.09) |

These findings raise two questions. First, it seems unlikely that we are mistaken in assuming that blind people are able to utilise spatial information in some way superior to sighted people. We have, however, in the present experiments obtained no evidence in support of the view that auditory or tactile acuity is better than—or is in any way organized differently from—that of sighted people. Secondly, since there are constant-errors for localization of stimuli in different modalities, it must necessarily follow that there is conflict underlying the spatial information available to blind *S*s and this conflict may be resolved in some way.

*Spatial contexts.* In previous research into problems of perception in conditions of intersensory stimulation, it was observed that the *S*s were able to localize stimulus-cues with respect to 'spatial contexts' rather than with respect to comparison-stimuli. To eliminate the effects of these spatial contexts a special laboratory and apparatus were built and new psychophysical procedures were devised. The over-all result of these precautions was that the localizing situation was very different from the conditions in which blind people usually perceive spatial stimuli. It has been found possible in experiments with sighted *S*s to reintroduce an auditory context into the laboratory. This is achieved by placing a second hearing-aid earphone directly in front of the *S*s, slightly above and behind a position central with the arcs described by the arms bearing the stimuli. The earphone is energized with a 1000 c.p.s. tone. A proprioceptive context may be reintroduced also by using one set of scales only for presentation of standard and variable stimuli.

## EXPERIMENT IV

The previous experiments were conducted in conditions in which spatial contexts were not available. It is possible that blind people are able to utilize the information provided by such contexts more efficiently than sighted people and that it is because of this that they appear to be able to localize both auditory and tactile stimuli more accurately. The present experiment was designed to observe the effects of reintroducing spatial contexts into intersensory localization.

*Procedure.* The *S*s were fitted into the apparatus and instructed, as for Experiment III with the following additions:

*Instructions.* In one part of this experiment you will hear a sound like a whistle; this will be straight ahead of you and it will remain always in the same position. Again, all you have to tell me is whether the noise, not whistle, or the touch stimulus is further to the left.

One hundred trials in each of the stimulus-conditions were given in blocks of 20, according to the procedure described for Experiment III. The magnitudes of intersensory constant-errors were checked between blocks. The spatial context-conditions were randomized between blocks.

*Results and discussion.* The results of Experiment IV are entered in Table VI. To consider the influence of spatial contexts, the variances due to the three contextual conditions have been divided into those for the condition in which no context is introduced in order to estimate the relevant *F*-ratio. These results suggest that no spatial context remains available for localizing since none of the within-*S* variances in the 'no context' condition differ significantly. Introduction of an auditory context, however, in the condition in which the auditory stimulus is presented as the variable appears to improve accuracy in the case of all blind *S*s. Introduction of a proprioceptive context in the condition in which the tactile stimulus is presented as the variable, while being in each case in the right direction, is only significantly effective in the case of three blind *S*s. The results are similar for sighted *S*s in the tactile context-condition, the relevant variances being different in the case of three *S*s. Only one sighted *S*, however, appears to be able to utilize the auditory context although this appears to be used as a spatial context by two sighted *S*s when the tactile stimulus is presented as variable.

TABLE VI

VARIANCES OF TACTILE-AUDITORY INTERSENSORY LOCALIZATIONS OF A GROUP OF BLIND AND SIGHTED *S*S IN DIFFERENT SPATIAL CONTEXT CONDITIONS IN INCREMENTS OF 100 MINUTES OF ANGLE

| | Stimulus-condition | Spatial context condition | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | None | | Aud. | | | T-kin. | | | Aud.-T-kin. | | |
| | | *S* | $\sigma^2$ | $\sigma^2$ | *F* | *p* | $\sigma^2$ | *F* | *p* | $\sigma^2$ | *F* | *p* |
| Blind *S*s | Aud.+ T-kin. | *JC* | 2.96 | 1.82 | 1.63 | $>0.05$ | 1.21 | 2.43 | $\approx 0.05^*$ | 1.37 | 2.01 | $\approx 0.05^*$ |
| | | *DF* | 0.92 | 0.86 | 1.07 | $>0.05$ | 0.67 | 1.37 | $>0.05$ | 0.83 | 1.11 | $>0.05$ |
| | | *AS* | 0.69 | 0.50 | 1.38 | $>0.05$ | 0.42 | 1.64 | $>0.05$ | 0.59 | 1.17 | $>0.05$ |
| | | *CP* | 275.60 | 174.20 | 1.58 | $>0.05$ | 88.92 | 3.09 | $<0.05^*$ | 74.30 | 3.62 | $<0.01^*$ |
| | | *PS* | 453.76 | 408.12 | 1.12 | $>0.05$ | 131.60 | 3.45 | $<0.05^*$ | 110.10 | 4.12 | $<0.01^*$ |
| | T-kin. +Aud. | *JC* | 2.79 | 0.66 | 4.23 | $<0.01^*$ | 1.74 | 1.60 | $>0.05$ | 0.92 | 3.02 | $<0.05^*$ |
| | | *DF* | 1.06 | 0.52 | 2.04 | $\approx 0.05^*$ | 0.86 | 1.23 | $>0.05$ | 0.59 | 1.80 | $>0.05$ |
| | | *AS* | 0.85 | 0.42 | 2.02 | $\approx 0.05^*$ | 0.76 | 1.12 | $>0.05$ | 0.38 | 2.24 | $\approx 0.05^*$ |
| | | *CP* | 292.40 | 132.32 | 2.21 | $\approx 0.05^*$ | 229.64 | 1.27 | $>0.05$ | 151.86 | 1.93 | $>0.05$ |
| | | *PS* | 458.13 | 131.80 | 3.48 | $<0.01^*$ | 293.16 | 1.56 | $>0.05$ | 161.63 | 2.83 | $<0.05^*$ |
| | | *S* | $\sigma^2$ | $\sigma^2$ | *F* | *p* | $\sigma^2$ | *F* | *p* | $\sigma^2$ | *F* | *p* |
| Sighted *S*s | Aud.+ T-kin. | *PW* | 10.67 | 8.29 | 1.29 | $>0.05$ | 2.12 | 5.03 | $>0.001^*$ | 3.37 | 3.16 | $<0.01^*$ |
| | | *CW* | 0.41 | 0.72 | 1.76 | $>0.05$ | 0.16 | 2.56 | $<0.025^*$ | 0.12 | 3.42 | $<0.01^*$ |
| | | *JF* | 3.61 | 3.32 | 1.09 | $>0.05$ | 1.37 | 2.64 | $<0.025^*$ | 1.62 | 2.23 | $<0.05^*$ |
| | | *AR* | 36.29 | 30.91 | 1.18 | $>0.05$ | 10.76 | 3.38 | $<0.01^*$ | 26.47 | 1.37 | $>0.05$ |
| | | *DH* | 0.91 | 1.23 | 1.35 | $>0.05$ | 0.82 | 1.11 | $>0.05^*$ | 0.96 | 1.06 | $>0.05$ |
| | T-kin. +Aud. | *PW* | 10.93 | 11.62 | 1.06 | $>0.05$ | 9.63 | 1.14 | $>0.05$ | 8.76 | 1.25 | $>0.05$ |
| | | *CW* | 0.47 | 0.69 | 1.47 | $>0.05$ | 0.59 | 1.25 | $>0.05$ | 0.60 | 1.28 | $>0.05$ |
| | | *JF* | 2.93 | 3.21 | 1.10 | $>0.05$ | 3.22 | 1.10 | $>0.05$ | 3.02 | 1.03 | $>0.05$ |
| | | *AR* | 40.12 | 15.29 | 2.63 | $<0.025^*$ | 27.47 | 1.50 | $>0.05$ | 18.29 | 2.20 | $<0.05^*$ |
| | | *DH* | 1.11 | 1.17 | 1.06 | $>0.05$ | 1.06 | 1.05 | $>0.05$ | 0.71 | 1.57 | $>0.\ 5$ |

Introduction of both contexts at the same time to blind *S*s improves accuracy in only 6 of the 10 conditions and appears to be less effective than would be expected from the results of the two individual context conditions. This outcome seems somewhat surprising until we recall that there were significant constant-errors for the intersensory localizations. What is probably happening in this condition is that there is conflict between the information providing the two contexts. This problem again raises the question of the nature of the mechanism by which conflict of this kind may be resolved.

*Sensory dominance.* Evidence for constant-errors between the spatial senses observed both in intersensory localization and in spatial context experiments implies that there is conflict in localizing situations in which information of different kinds is available. We have suggested previously that this conflict is resolved by the operation of sensory dominant mechanisms.[10] To our knowledge perceptual mechanisms of this kind have not been studied previously with blind *S*s, but in view of their greater reliance upon auditory information it may be that the usual order of dominance is reversed and that this is the means by which apparent closer cooperation between the auditory and tactile senses is achieved.

## EXPERIMENT V

The method used previously in experiments on sensory dominance was to tell *S*s that two stimuli, say in vision and audition, were placed together when they were actually displaced by a small angular distance. The *S*s were then required to localize the 'paired' stimulus with respect to a tactile stimulus. Since this method could not be used with blind *S*s, it was decided to employ again the head-turning response.

*Procedure.* The *S*s were fitted into the apparatus with their heads clamped firmly into the helmet. They were instructed, as in Experiment II, with the following additional instructions:

> This time I will allow you to touch and to hear a stimulus in the *same* position, just turn your head towards it as before.

Auditory, tactile and 'paired' conditions were presented in randomized blocks. In the paired condition the tactile and auditory stimuli were displaced by 400 minutes of angle.

*Results and discussion.* The results of Experiment V are shown in Table VII. The *constant* errors for four blind and five sighted *S*s suggest that the tactile sense is dominant, since the tendency is to localize more toward the apparent position of the tactile stimulus. In no case, however, is dominance *complete* and for one *S* (*AS*) the apparent position of the 'paired' stimulus almost exactly bisects that of the individual stimuli. In the case of another *S* (*JC*) the position of the paired stimulus appears to be beyond the position of the auditory stimulus. This may be taken as an indication of auditory dominance, but it should be noted that the constant-error of this *S* also fluctuates very widely. All sighted *S*s show complete tactile dominance. Hence, it seems that in general the usual dominance order is *not* reversed for blind *S*s, the implication being that in conditions of the kind used in this experiment the perceptual mechanisms brought into operation are the same in each group.

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[10] Fisher, Resolution of spatial conflict, *ibid.*, 46, 1962, 3A.

TABLE VII

CONSTANT ERRORS (MEANS) FOR LOCALIZATIONS USING THE HEAD-TURNING RESPONSE (IN DEGREES OF ANGLE)

| | *S* | T-kin. | Aud. | T-kin.+Aud. | Dom. stimulus |
|---|---|---|---|---|---|
| Blind | *JC* | −7.72 (0.25) | −11.95 (0.25) | −13.02 (0.24) | Aud. |
| | *DF* | −11.32 (0.20) | −16.76 (0.28) | −12.37 (0.23) | T-kin. |
| | *AS* | +9.13 (0.29) | +22.26 (0.28) | +15.11 (0.30) | T-kin. |
| | *CP* | −8.20 (0.29) | −10.29 (0.30) | −8.74 (0.30) | T-kin. |
| | *PS* | +8.42 (0.22) | +14.49 (0.30) | +9.96 (0.26) | T-kin. |
| Sighted | *AC* | −5.01 (0.25) | +6.04 (0.30) | −5.37 (0.27) | T-kin. |
| | *AL* | +1.67 (0.25) | −0.92 (0.31) | +1.47 (0.25) | T-kin |
| | *RW* | +0.22 (0.30) | −3.17 (0.27) | −0.13 (0.25) | T-kin, |
| | *FG* | −1.84 (0.34) | +2.04 (0.28) | −1.72 (0.26) | T-kin. |
| | *PC* | +5.16 (0.27) | +6.73 (0.28) | +5.21 (0.30) | T-kin, |

## SUMMARY

To blind people, vision, the most accurate source of spatial information is not available; nevertheless, their ability to localize and to orientate themselves in relation to objects is often remarkably accurate. Two hypotheses have been advanced. The first is that the acuity of hearing is markedly improved; and the second that an extra sense of some kind is developed. The present investigation was designed to compare the auditory and tactile-kinesthetic localizing abilities of groups of blind and sighted *S*s. That the results gave no support for either of the hypotheses mentioned may be due to the fact that 'spatial contexts' were not available to the *S*s. In experiments in which these contexts were reintroduced, it was observed that blind *S*s appear to be able to utilize the additional information provided—particularly in audition and rather less so in tactile-kinesthesis. This is, to some extent, the reverse of the tendency of sighted *S*s under similar conditions.

In a series of 'intersensory' localizations, it was observed that both blind and sighted *Ss* make significant constant-errors in localizing auditory and tactile stimuli when presented in the same place. This suggests that for blind *Ss*, also, there must be conflict underlying these two spatial senses. Experiments on 'sensory dominance' were conducted to establish which of these two modalities was dominant. Tactile-kinesthesis was generally found to be dominant for both blind and sighted, but for one *S*, who had been blind from birth, audition appeared to be the dominant modality.

# AVOIDANCE-CONDITIONING IN THE FISH: FURTHER STUDIES OF THE *CS-US* INTERVAL

By Erika R. Behrend and M. E. Bitterman, Bryn Mawr College

In a recent experiment on avoidance-conditioning in the fish, the role of the *CS-US* interval in acquisition was studied by a method which equated opportunity for response.[1] Various groups of goldfish were trained in a shuttlebox with intervals of 0, 1.25, 2.5, 7.5, 10, 15, 20, 30, or 60 sec., after which each group was given 10 test-trials with a 20-sec. interval. Probability of avoidance on test-trials was relatively low for animals trained at 0 sec.—no higher than the probability for sensitization-controls—which suggested that some precedence of the *CS* was necessary for conditioning. At 1.25 sec., the probability-function rose to a high level which did not change significantly with further increases in the interval, although latency of avoidance on test-trials increased progressively as the training interval increased from 1.25-60 sec. How are these findings to be interpreted?

## I. Warner vs. Hunter

A possibility to be considered is that performance was good at the longer *CS-US* intervals because all the animals were in fact receiving some training at a short interval. Warner's method was employed for the work—at the end of the *CS-US* interval, shock came on and 'stayed on' until the shuttling response was made, or until 15 sec. had elapsed.[2] Since the shock was an intermittent one (a 0.25-sec. pulse every 1.5 sec.)—continuous shock could not be used because it tended to produce immobilization—one might argue that all animals were in effect subjected to many series of massed conditioning trials at a *CS-US* interval of 1.5 sec. To test this interpretation, certain aspects of the earlier experiment were repeated by Hunter's method—at the end of the *CS-US* interval, the trial was terminated with a single 0.5-sec. pulse of shock.

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* Received for publication June 30, 1962. This research was supported by Grant No. M-2857 from the National Institute of Mental Health.

[1] E. R. Behrend and M. E. Bitterman, Avoidance-conditioning in the goldfish: Exploratory studies of the *CS-US* interval, this Journal, 75, 1962, 30-34.

[2] J. L. Horner, Nicholas Longo, and M. E. Bitterman, A shuttlebox for the fish and a control circuit of general applicability, this Journal, 74, 1961, 114-120.

*Experiment 1.* The *Ss* were 49 goldfish drawn from the same population as those of previous experiments. They were trained in the same apparatus and under the same general conditions as in the previous experiment: 10 trials per day with a mean intertrial interval of 3 min. One group of 13 animals was trained by Warner's method with a *CS-US* interval of 2.5 sec.; a second group of 10 animals was trained by Warner's method with an interval of 20 sec.; two other groups of 13 animals each were trained by Hunter's method with intervals of 2.5 and 20 sec., respectively. There were 10 days of training under these conditions. On Day 11, which was the final day of the experiment, all groups were trained with a *CS-US* interval of 20 sec.

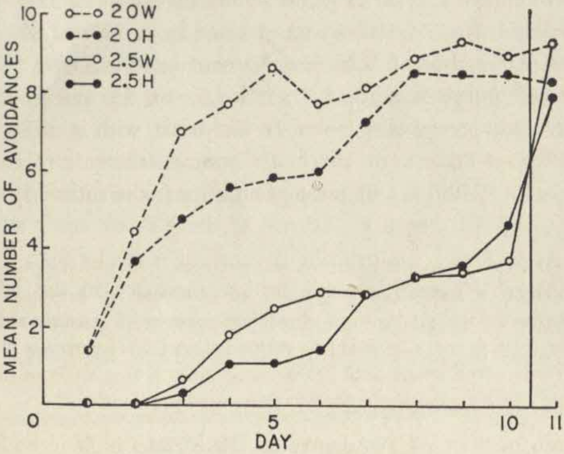

FIG. 1. THE COURSE OF LEARNING IN THE FOUR GROUPS OF EXPERIMENT 1
(On Day 11, all groups were trained with a 20-sec. interval.)

In Fig. 1, the course of learning in the four groups is plotted in terms of mean number of avoidances. Inspection of the curves suggests that performance during the first 10 days varied with *CS-US* interval but not with method of training, and this impression is confirmed by analysis of variance (Lindquist's Type III). The effects of Days and of Intervals are significant beyond the 1% level (for Days, $F = 48.55$, with 9 and 405 *df;* for ntervals, $F = 96.98$, with 1 and 48 *df*), but the effect of Conditions is not ($F = 2.75$, with 1 and 48 *df*, $p > 0.05$). The interaction of Days and Intervals is significant ($F = 5.54$, with 9 and 405 *df*, $p < 0.01$), as is the interaction of Days and Conditions ($F = 2.16$ with 9 and 405 *df*, $p < 0.05$). Neither the double interaction of Intervals and Conditions, nor the triple interaction, is significant. On Day 11, with opportunity for response equated, all four groups performed at essentially the same level. Analysis of variance shows no significant differences among them.

Since the 2.5-sec. groups made so few avoidances in the first 10 days, the latency of avoidance in Warner and Hunter groups trained at that interval could not be compared. Latency of avoidance in the two 20-sec. groups declined progressively in essentially similar fashion. Analysis of variance (Lindquist's Type I) yields significant effects of Days ($F = 3.59$, with 9 and 171 *df*, $p < 0.01$) and of Conditions ($F = 4.67$, with 1 and 21 *df*, $p < 0.05$), but no significant interaction. On Day 11, the latency of the 20-sec. groups was about 3 sec. longer on the average than that of the 2.5-sec. groups—about 7 sec. as compared with 4 sec. Analysis of variance shows a significant effect of Training Intervals ($F = 9.04$, with 1 and 44 *df*, $p < 0.01$), but not of Conditions; the interaction is negligible.

*Experiment 2.* A second experiment was performed to provide some further results with Hunter's method which could be compared to previous results with Warner's method. Two new groups of goldfish, one of 11 animals and one of 12, were trained for four days by Hunter's method with *CS-US* intervals of 2.5 and 20 sec., respectively, after which both groups were given a single day of training at 20 sec. All other conditions were the same as those used previously.

The mean number of avoidances on Day 5 was high in both groups—9.2 after training at 2.5 sec. and 8.6 after training at 20 sec. The mean latency of avoidance on Day 5 was 4.1 sec. in the 2.5-sec. group and 5.8-sec. for the 20-sec. group, significantly different at the 5% level ($F = 4.85$, with 1 and 21 *df*). The values for probability and for latency are quite similar to those obtained in an analogous experiment with Warner's method.[3]

In general, then, the *CS-US* functions obtained with prolonged intermittent shock and with a single brief shock seem to be very much the same. The present results provide no foundation for the suspicion that differences due to *CS-US* interval may have been masked in previous work by the use of intermittent shock.

## II. Some Determinants of Latency

With opportunity for response controlled, probability of avoidance does not change appreciably as the *CS-US* interval used in training increases from 1-60 sec., but latency of avoidance increases progressively with interval. If latency and probability both are considered to be measures of the 'strength' of conditioning, with latency a more sensitive measure than probability, the results may be taken to mean that conditioning is better at the shorter intervals—either because the shorter intervals are more favorable *per se* or because animals trained at the shorter intervals, which provide less opportunity for avoidance, receive more shock than animals trained at the longer intervals. Another possibility is that the latency-function is a product of temporal discrimination. Longer intervals may produce, not 'poorer' conditioning, but conditioning to a 'different' stimulus—*e.g.* a light which has been on for a time as distinct from one which has just come on.

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[3] Behrend and Bitterman, *op. cit.*, 31-32.

*Experiment 3.* The purpose of the third experiment was to study the effect on latency of variation in the *CS-US* interval while controlling the number of shocks received. To achieve this purpose it was necessary to prevent avoidance, and each of the shuttle-boxes therefore was equipped with a removable Plexiglas barrier which made changing compartments impossible. In the first stage of training, four groups of goldfish were given eight days of classical conditioning at *CS-US* intervals of 0, 2, 8, and 32 sec., respectively—there were 11 *S*s in the 2-sec. group and 12 in each of the others. Ten trials a day were given with a mean intertrial interval of 3 min. The *US* was 5 sec. of intermittent shock, during which time the *CS* remained on. Each animal was trained on half the days in one of the two compartments of its shuttle-box and on the remaining days in the other compartment. In the second stage of the experiment, which lasted but a single day, the barriers to shuttling were removed, and all groups had 10 trials of pure escape-training at the usual intertrial interval; the intermittent shock alone was turned on, and remained on until the animal changed compartments, or until 15 sec. elapsed. In the third stage of training, again a single day, all groups had 10 test-trials with a *CS-US* interval of 20 sec.

In Fig. 2, mean latency of avoidance on the test-trials is plotted against *CS-US* interval. The function is very much like that produced by training under conditions which permit avoidance, falling sharply from a high at 0 sec., and then rising gradually. Analysis of variance shows the latencies of the groups trained at 2, 8, and 32 sec. to be significantly different at the 1% level ($F_1 = 7.21$, with 2 and 32 *df*). Again, as in avoidance-training, probability of avoidance was high in all three groups, which did not differ significantly in this respect. We conclude, therefore, that differences in *CS-US* interval produce differences in latency of avoidance which cannot be attributed entirely to differences in number of shocks received. As the next experiment will show, however, the avoidance-contingency may indeed have some effect on latency.

*Experiment 4.* Two groups of 14 goldfish each were trained for 20 days under the usual avoidance-conditions. For one group (constant-avoidance), the *CS-US* interval was 15 sec. For the other group (variable-avoidance), the interval varied unpredictably from trial to trial, although the average interval was 15 sec. On three of each day's trials, the interval was 2.5 sec., on three it was 27.5 sec., and on the remaining four trials it was 15 sec. The performance of the variable group on the four 15-sec. trials given each day was compared with the performance of the constant group on the corresponding trials. Two additional groups of 13 animals each were trained for 10 days under the classical conditions (barrier in place to prevent shuttling) with a single 0.5-sec. pulse of shock as the *US*. For one group (constant-classical), the *CS-US* interval was 15 sec., while for the other (variable-classical), it varied as in the variable-avoidance group. Then the classical groups were given 10 trials of pure escape-training on an unnumbered day which intervened between the last day of classical training (which was designated Day 10) and what was designated the eleventh day of the experiment. On Day 11, the two classical groups were shifted to the avoidance-procedure with a constant *CS-US* interval of 15 sec. for both. On Days 12–20, avoidance-training was continued for both groups, with a constant 15-sec. interval for the constant group and the variable interval which averaged 15 sec. for the variable group.

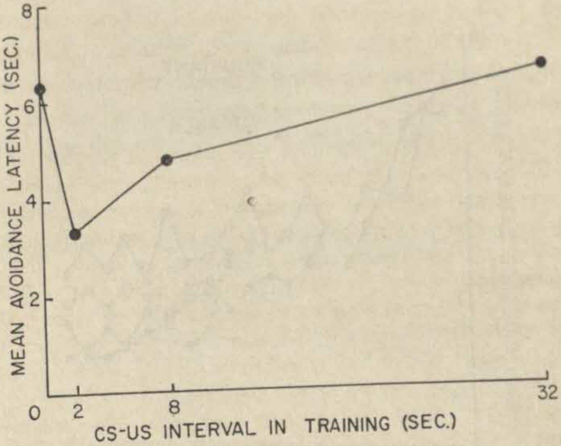

FIG. 2. LATENCY OF AVOIDANCE AS A FUNCTION OF *CS-US* INTERVAL WITH NUMBER OF SHOCKS CONTROLLED (EXPERIMENT 2)

In Fig. 3, the results for the four groups are plotted in terms of mean avoidance-latency on the four 15-sec. trials typically given the variable groups on each day of avoidance-training and on corresponding trials for the constant groups, except that the Day-11 points for the two classical groups are based on all 10 of the 15-sec. trials which both groups had that day. Analysis of variance (Lindquist's Type III) for the data of Days

12-20 yields a significant effect of Days ($F = 3.50$, with 8 and 396 *df*, $p < 0.01$) and of Variability ($F = 13.17$, with 1 and 50 *df*, $p < 0.01$); a significant interaction of Days and Previous Training ($F = 2.05$, with 8 and 396 *df*, $p < 0.05$); and a significant triple interaction ($F = 2.10$, with 8 and 396 *df*, $p < 0.05$).

These results show clearly that the determinants of latency in avoidance-conditioning are different, at least to a certain extent, from those in classical conditioning. The identical latencies of the two classical groups on Day 11 suggest an averaging effect in classical conditioning. The difference in the latencies of the two avoidance-groups suggests that the shortest interval is weighted more heavily than the longer ones. It should be noted that the variable-avoidance group received more shocks than the constant-avoidance group, but it is doubtful that the lower latency of avoidance in the variable group can be explained in these terms. The classical groups had received substantially more shocks by Day 11 than had the variable-avoidance animals, and yet their latencies were higher. A more likely explanation of the lower avoidance-latencies which develop in variable-avoidance training is to be found in the fact that, as training proceeds,

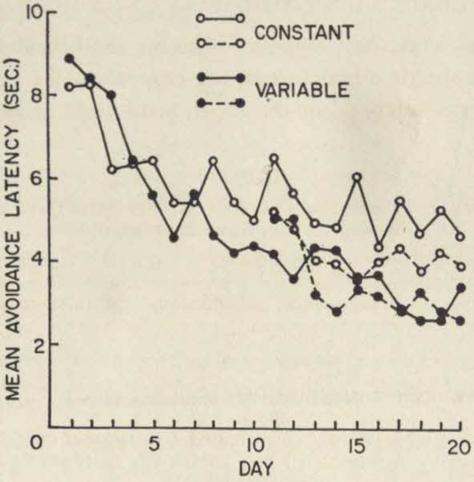

FIG. 3. LATENCY OF AVOIDANCE IN TRAINING WITH CONSTANT AND VARIABLE *CS-US* INTERVAL (EXPERIMENT 4)
(The broken lines are for the groups trained for 10 days under conditions in which avoidance was prevented.)

an increasing proportion of the shocks received come after the shortest of the three intervals; at asymptote, almost all failures of avoidance occur on 2.5-sec. trials. From this point of view, the effect of the avoidance-contingency on latency of response is simply to reduce the likelihood of exposure to the longer *CS-US* intervals. There remains the question of why the shorter intervals produce shorter latencies, which will be considered after the presentation of some data on still shorter intervals.

## III. CONDITIONING WITH SHORT *CS-US* INTERVALS

Our previous work has suggested that no conditioning occurs when *CS* and *US* are strictly simultaneous, but only when the *CS* precedes the *US* by some short interval. In the experiments now to be reported, the possibility of simultaneous conditioning is investigated further, and the 0-1 sec. range of intervals is explored in some detail.

TABLE I

PERFORMANCE OF THE SIMULTANEOUS AND SENSITIZATION-CONTROL GROUPS ON DAY 6 IN EXPERIMENT 5

| | Shock 0.5-sec. | | Shock 5-sec. | |
|---|---|---|---|---|
| | Simultaneous | Sensitization | Simultaneous | Sensitization |
| Mean number of avoidances | 6.0 | 5.8 | 6.0 | 4.8 |
| Mean latency of avoidance (sec.) | 9.65 | 10.17 | 8.49 | 9.00 |

*Experiment* 5. Four groups of goldfish were studied in a 2 × 2 design. One variable was *US*-duration—a 0.5-sec. steady shock was used for two groups, and a 5-sec. intermittent shock for the other two. The second variable was pairing. For two (simultaneous) groups, the presentation of the *CS* coincided exactly with the presentation of the *US;* these animals had 10 trials per day at the usual 3-min. intertrial interval. For the other two (sensitization) groups, there were 20 stimulus-presentations per day at a mean interpresentation interval of 1.5 min.—10 of the *US* alone, and 10 of the *CS* alone, in random order—the duration of the *CS* being 0.5 sec. for the shorter-shock group and 5 sec. for the longer-shock group. During this first stage of the experiment, which lasted four days, the barrier was used to prevent shuttling. On Day 5, the barrier was removed, and all groups were given 10 trials of pure escape-training. On Day 6, all groups were given 10 trials of avoidance-training with a 20-sec. *CS-US* interval. There were 12 goldfish in each of the groups except the 0.5-sec. simultaneous group, which contained only 11 fish.

In Table I, the four groups are compared in terms of mean number of avoidances and mean avoidance-latency on Day 6. Analysis of number of

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avoidances shows neither a significant effect of *US*-duration, nor a significant effect of Pairing, nor a significant interaction (all *F*-values $< 1$). Analysis of avoidance-latencies shows a significant effect of *US*-duration ($F = 2.30$, with 1 and 42 *df*, $p < 0.05$), but not of Pairing ($F < 1$), and a negligible interaction. The data provide no evidence, then, of any conditioning with strictly simultaneous presentation of *CS* and *US*.

*Experiment* 6. In this experiment—really a series of experiments—238 goldfish were studied at 11 different *CS*-*US* intervals ranging from 0-1 sec. There were 32 animals at 0 sec.; 16 at 0.10 sec.; 32 at 0.12 sec.; 15 at 0.18 sec.; 48 at 0.25 sec.; 16 at 0.32 sec.; 15 at 0.38 sec.; and 15 each at 0.40, 0.50, 0.75, and 1.00 sec. The reason for this peculiar array of numbers lies in the fact that our first set of determinations (at 0, 0.25, 0.50, and 1.00 sec.) gave some indication of better performance at 0 sec. than at 0.25 sec., which led us to a more detailed exploration of the 0-0.50 sec. region. Each of the 11 groups had four days of training at its given interval by our standard Warner method. Then each group had an additional day of training with a 20-sec. interval.

In Fig. 4, the results of the experiment are plotted in terms of mean number of avoidances and mean avoidance-latency on Day 5. The curves indicate a progressive increase in number of avoidances and a progressive decrease in latency of avoidance as the *CS*-*US* interval increased from 0-1.00 sec. Analysis of variance shows both trends to be significant at a level of confidence well beyond 1% (for a number of avoidances, $F = 3.90$, with 10 and 277 *df;* for latency, $F = 3.32$ with 10 and 224 *df*). Over this range, apparently, there is a substantial (inverse) correlation between the two measures. Beyond this range, the correlation disappears, with latency of avoidance increasing progressively while probability of avoidance remains essentially constant, at least for the next 30-60 sec.

Before considering the meaning of the 0-1 sec. results, let us consider the Pavlovian assumption that the long avoidance-latencies found in animals trained with long *CS*-*US* intervals may be traced to temporal discrimination. If what is conditioned is the afferent state of affairs existing when the *US* is introduced, and if the state existing at any moment varies with time since onset of the *CS*, then, if what is necessary to elicit avoidance is an afferent state similar in some degree to that which has been conditioned, the observed relation between latency of avoidance and *CS*-*US* interval follows. On any given test-trial, an animal trained with a long interval must wait longer for such a state than an animal trained with a short interval, although the latencies of both animals will be considerably shorter than the intervals at which they were trained because of generalization. The observed relation between probability of avoidance and

*CS-US* interval also is easy to understand in these terms. With long test-intervals, probability does not decline with training interval; an animal trained with a longer interval must wait longer for the requisite afferent state to develop, but eventually it does, and the animal responds. With short test-intervals, probability of avoidance may decline in animals trained at long intervals because insufficient time is provided for the requisite state to develop. We have begun to map the relation between training and test-interval in a previous experiment on resistance to extinction.[4]

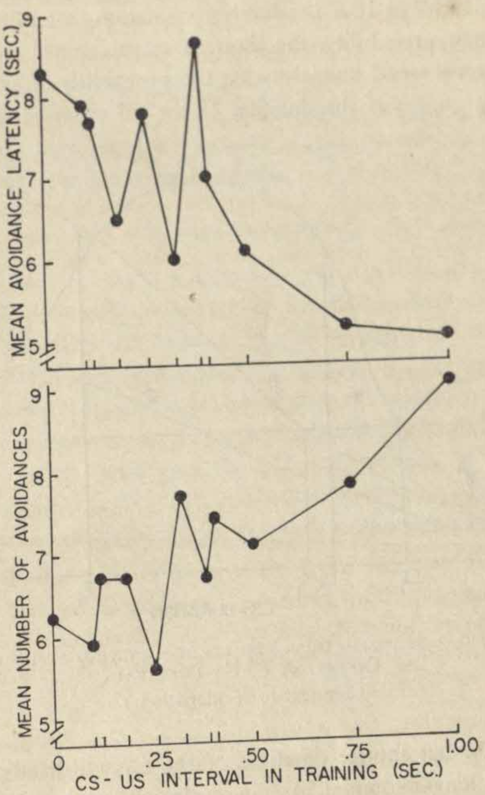

FIG. 4. MEAN PROBABILITY OF AVOIDANCE AND MEAN LATENCY OF AVOIDANCE AS A FUNCTION OF *CS-US* INTERVAL OVER THE 0-1-SEC. RANGE

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[4] Behrend and Bitterman, *op. cit.*, 27-30.

The conception outlined here is schematized in Fig. 5. The abscissa represents *CS*-duration, and the ordinate represents the strength of some excitatory tendency established by conditioning. Curves *A* and *B* represent temporal gradients of excitation generated by training at two *CS-US* intervals, $T_2$ and $T_4$.[5] If *R* is a level of excitation that will produce avoidance with high probability, the shorter-interval animal will avoid by $T_1$ and the longer-interval animal by $T_3$; the probability of avoidance will be high in both groups if the duration of the *CS* on test-trials is somewhat longer than $T_3$, but shorter durations of the *CS* will handicap an animal trained at the longer interval. As Fig. 4 also shows, however, the scheme makes a place for the notion that both latency and probability of avoidance somehow reflect the 'strength' of conditioning. Curve *C* represents a gradient of excitation produced by a smaller amount of training at $T_4$ than that which produced the gradient of Curve *B;* it shows the *R*-level of excitation being reached at a time after $T_3$. The maximum of a curve drawn to represent a still smaller amount of training would fall below *A*, which would mean a lower probability of avoidance, and, where avoidance did occur, a still longer latency. The curves of learning plotted

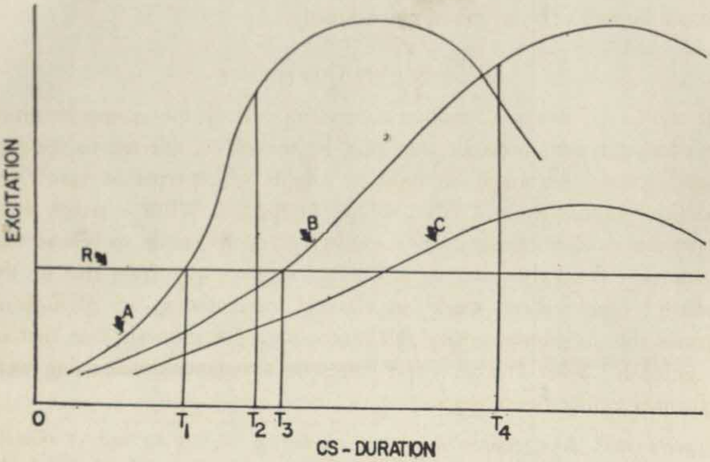

FIG. 5. HYPOTHETICAL RESULTS OF TRAINING AT TWO DIFFERENT *CS-US* INTERVALS
(See text for explanation.)

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[5] The choice of curve-shape is, of course, rather arbitrary.

in terms both of latency and of probability, which we have presented in an earlier paper, may be understood in these terms.[6] It is possible, too, that the results obtained for *CS-US* intervals of 0-1 sec. are to be explained in the same terms; on the assumption that conditioning is "an increasing function of the frequency of the associated receptor discharge,"[7] the various short intervals may be thought of as generating excitatory functions of different height at essentially the same locus on the temporal continuum. It seems clear, in any event, that the measures available to us in the shuttlebox make only rather peripheral contact with a process which it might be well to try to get at more directly.

## IV. A Species-Difference

If the *CS-US* interval function depends in part on the manner in which the afferent system brought into play by the *CS* is affected by its prolonged action, the shape of function might be expected to vary from modality to modality and from species to species. While a recent study of avoidance-conditioning in *Pomacentrus* gives no reason to believe that its function for light-onset as *CS* differs substantially from that of the goldfish,[8] other recent work on classical conditioning in *Mollienesia* suggests that its function may fall much more precipitously than that of the goldfish.[9] Some further experiments on avoidance-conditioning were performed to investigate the matter.

*Experiment* 7. Four groups of mollies, numbering 16, 18, 19, and 17 animals, were trained with *CS-US* intervals of 1.25, 2.5, 5.0, and 10.0 sec., respectively, by the standard (Warner) procedure. The shocking voltage was higher than that used for the goldfish, but the functional criterion for the choice of voltage was the same: the voltage used was the lowest which would produce consistent, low-latency escape in pilot animals. After four days of training at one of the four intervals, each group was given one day of training at an interval of 20 sec.

The results of this experiment showed primarily that the mollies improve less rapidly than the goldfish; the mean number of avoidances at each interval on Day 5 (4.2 at 1.25 sec., 3.4 at 2.5 sec., 3.3 at 5.0 sec., and 2.2 at 10 sec.) was lower even than in goldfish of a previous experiment that were given only a single day of training.[10] Even so, however, the mollies gave evidence of a progressive decline in performance as *CS-US* interval increased, while there was no tendency of this sort in the one-day goldfish. Mean latency of avoidance increased with interval in the mollies as well as in the goldfish.

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[6] Behrend and Bitterman, *op. cit.*, 20, 22.

[7] C. L. Hull, *Principles of Behavior*, 168.

[8] Jerome Wodinsky, E. R. Behrend, and M. E. Bitterman, Avoidance-conditioning in two species of fish, *Anim. Behav.*, 10, 1962, 76-78.

[9] Merrill Noble, Anne Gruender, and D. R. Meyer, Conditioning in fish (*Mollienesia sp.*) as a function of the interval between *CS* and *US*, *J. comp. physiol. Psychol.*, 52, 1959, 236-239; C. S. Klinman and M. E. Bitterman, Classical conditioning in the fish: The *CS-US* interval, *ibid.*, 56, 1963, 578-583.

*Experiment 8.* The design of this experiment was somewhat different from those of previous experiments, and it was necessary, therefore, to do some analogous work with goldfish. Five groups of mollies were given seven (training) trials per day at a *CS-US* interval of 0, 0.5, 1.25, 2.5, or 10 sec.; on the remaining three (test-) trials of each day (whose positions within the day varied in quasi-random fashion), the *CS-US* interval for all groups was 20 sec. There were nine animals in each of the groups except for the 10-sec. group, which contained eight animals. Other features of the training were standard. Five additional groups of mollies (nine animals per group) were treated in the same way as the first five, except that the mean intertrial interval was reduced from 3 min. to 1.5 min. The spacing variable was introduced because the classical conditioning results of Klinman and Bitterman had suggested an interaction in mollies between *CS-US* interval and intertrial interval in the determination of probability of response.[11] When, however, an analysis of variance (Lindquist's Type III) showed no significant effect of Spacing ($F < 1$) and no interaction of *CS-US* Interval with Spacing ($F < 1$) in the present data for mollies, the results for the two intertrial intervals were pooled for comparison with the results for five groups of goldfish (11 at 0 sec., 9 at 0.5 sec., 10 at 1.25 sec., 10 at 2.5 sec., and 11 at 10 sec.) trained in like manner with an intertrial interval of 3 min.

The results for probability of avoidance on training and on test-trials are plotted in Fig. 6. The plots are based on data for Days 6-10, during which time all groups of the two species appeared to be at asymptote. The training-trial curves for both species rise in negatively accelerated fashion as the *CS-US* interval increases—a now familiar effect of increasing opportunity for response—although level of performance is higher in the goldfish than in the mollies. The test-trial curves for the two species differ in a manner which might have been expected from the data on classical conditioning; the curve for the goldfish rises rapidly to a high level which is maintained over the range of intervals tested, while that for the mollies falls progressively from a maximum at 0.5 sec. Analysis of variance for the total number of avoidances on the test-trials of Days 6-10 yields significant effects of *CS-US* Interval ($F = 14.4$, with 4 and 130 $df$, $p < 0.01$) and of Species ($F = 145.0$, with 1 and 130 $df$, $p < 0.01$), as well as a significant interaction ($F = 5.9$, with 4 and 130 $df$, $p < 0.01$).

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[10] Behrend and Bitterman, *op. cit.,* 31.

[11] Klinman and Bitterman, *op. cit.,* 578-583.

The asymptotic latency-functions for the test-trials with mollies prove to have very much the same shape as those for goldfish—the curve for mollies falling from 0 to a minimum at 0.5 sec. and then rising progressively, the curve for goldfish falling to a minimum at 1.5 sec. and then rising progressively—although the latencies of the mollies are about 1 sec. higher on the average than those of the goldfish at the upper three *CS-US* intervals. Analysis of variance yields a significant effect of *CS-US* Interval ($F = 21.02$, with 4 and 130 *df*, $p < 0.01$) and of Species ($F = 7.82$, with 1 and 130 *df*, $p < 0.01$), but the interaction is negligible ($F < 1$).

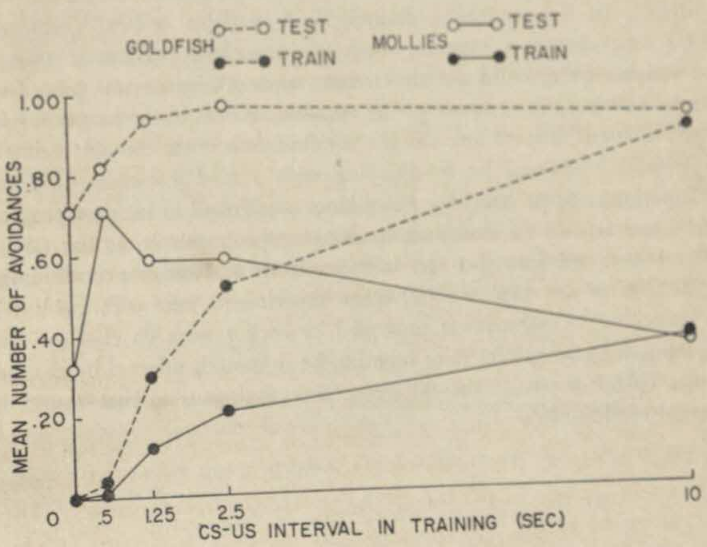

FIG. 6. ASYMPTOTIC PERFORMANCE OF GOLDFISH AND MOLLIES ON TRAINING AND TEST-TRIALS OF EXPERIMENT 8

The basis of the species-difference which appears in this experiment, remains, of course, to be determined. It may prove to be a perfectly trivial one. For example, a change in shock-intensity or in *CS*-intensity might produce in the goldfish a declining probability-function like that of the mollies. The similarity between the present results and those obtained in so diverse a situation as that of Klinman and Bitterman suggests, however, that some more far-reaching difference may be involved.

## SUMMARY

The role of the *CS-US* interval in the avoidance-conditioning of fish was studied in a series of experiments with the shuttlebox. In Experiments 1 and 2, the relation of probability of avoidance to *CS-US* interval was found to be unaffected by the nature of the shock employed (a brief, continuous shock *versus* a prolonged, intermittent one). In Experiment 3, longer *CS-US* intervals were found to produce longer avoidance-latencies than short *CS-US* intervals even when the number of shocks received was controlled. In Experiment 4, training with variable and with constant *CS-US* intervals was compared; when avoidance was prevented, constant and variable groups did not differ, but, when avoidance was permitted, thereby limiting the exposure of the variable animals to the longer *CS-US* intervals, latencies were lower in the variable than in the constant animals. Experiment 5 showed no conditioning with a *CS-US* interval of 0 sec. In Experiment 6, probability of avoidance was found to increase progressively, and latency of avoidance to decrease progressively, as the *CS-US* interval increased from 0-1 sec. In Experiment 7, avoidance-conditioning in *Mollienesia* was explored (all other experiments were with goldfish). In Experiment 8, which was prompted by earlier work on classical conditioning, the two species were found to be differently affected by changes in the *CS-US* interval; the difference was analogous to that found in classical conditioning.

# PROBABILITY-LEARNING AND HABIT-REVERSAL IN THE COCKROACH

By NICHOLAS LONGO, Colgate University

Comparative studies of learning in vertebrates have begun to yield an interesting pattern of results which suggests the need for similar studies of invertebrate forms.[1] In a series of experiments designed to investigate the phyletic generality of certain well-established phenomena of mammalian learning, some striking differences have appeared in the behavior of fish and rat: Experiments on probability-learning in the fish have produced matching under conditions in which the rat maximizes, and experiments on habit-reversal in the fish have failed to produce evidence of the progressive improvement which is so easily found in the rat.[2] Furthermore, subsequent experiments with intermediate forms, such as the pigeon, have given results which are in some respects like those for the rat and in other respects like those for the fish. For example, the pigeon seems to function like the rat in experiments on habit-reversal,[3] but like the fish in experiments on probability-learning.[4] The purpose of the present work was to provide some comparable data for a representative arthropod.

An analysis of the sparse literature of invertebrate learning indicated that the cockroach might be appropriate for this purpose. Studies by Szymanski,[5] Eldering,[6] and Turner[7] suggested that the animal would live well in the laboratory, would

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[1] M. E. Bitterman, Toward a comparative psychology of learning, *Amer. Psychologist*, 15, 1960, 704-712.

[2] M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this JOURNAL, 71, 1958, 94-110.

[3] D. H. Bullock and M. E. Bitterman, Habit reversal in the pigeon, *J. comp. physiol. Psychol.*, 55, 1962, 958-962.

[4] Bullock and Bitterman, Probability-matching in the pigeon, this JOURNAL, 75, 1962, 634-639.

[5] J. S. Szymanski, Modification of the innate behavior of cockroaches, *J. anim. Behav.*, 2, 1912, 81-90.

[6] F. J. Eldering, Acquisition d'habitudes par les insects, *Arch. neerland. Physiol.*, 3, 1919, 469-490.

[7] C. H. Turner, Behavior of the common roach (*Periplaneta orientalis*) on an open maze, *Biol. Bull.*, 25, 1913, 348-361.

adjust to a variety of learning situations, and would be susceptible to standard techniques of motivational control. The more recent work of Minami and Dallenbach suggested that enough precision was possible to permit the kinds of functional comparison the program required,[8] although their technique—an adaptation of Szymanski's—was itself not well-suited to that program. Accordingly, efforts were made to develop a more suitable technique. After several failures with food as reward, success was finally achieved with a *Y*-maze in which correct choice was rewarded with avoidance of or escape from shock—an $A_d$-$2_g$ situation according to Bitterman's classification.[9] The efficacy of the technique is demonstrated by two experiments designed to study the cockroach under conditions analogous to those which have differentiated among vertebrate forms.

## EXPERIMENT I: PROBABILITY-LEARNING

Trained in a choice-situation, such as a *T*-maze, with one alternative rewarded at random on a certain proportion of trials and the other alternative rewarded on the remaining trials, the fish and the pigeon show what has been called 'matching'—that is, their choice-ratios correspond to the reinforcement-ratio.[10] Thus, in a 70:30 problem, the 70% alternative is chosen on about 70% of trials and the 30% alternative on remaining trials; in a 50:50 problem, each alternative is chosen on about half the trials. Furthermore, the matching seems perfectly random; the appropriate model is given by the selection of a single marble (with replacement after selection) from a sack of black and white marbles, the ratio of the two colors in the sack being the reinforcement-ratio. Under conditions analogous to those in which the fish and the pigeon match, the rat and the monkey typically 'maximize'—that is, they choose the more frequently rewarded alternative on about 100% of trials.[11] When, on occasion, some correspondence between choice-ratio and reinforcement-ratio has appeared in one of these animals, it has been traced to a non-random pattern of choice. In a study of the rat by Hickson,[12] the correspondence was the outcome of reinforcement-following, the rats tending to go on each trial to the side on which they were last fed. In a study of the monkey by

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[8] Hiroshi Minami and K. M. Dallenbach, The effects of activity upon learning and retention in the cockroach, this JOURNAL, 59, 1946, 2-12.

[9] Bitterman, Techniques for the study of learning in animals: Analysis and classification, *Psychol. Bull.*, 55, 1962, 81-93.

[10] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110; Erika Behrend and M. E. Bitterman, Probability-matching in the fish, this JOURNAL, 74, 1961, 542-551; Bullock and Bitterman, *op. cit.*, this JOURNAL, 75, 1962, 634-639.

[11] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110; W. A. Wilson, Jr., and A. R. Rollin, Probability learning in the monkey, *J. exp. Psychol.*, 58, 1959, 174-179; Wilson, Two-choice behavior of monkey, *ibid.*, 59, 1960, 207-208.

[12] R. H. Hickson, Response probability in a two-choice learning situation with varying probability of reinforcement, *J. exp. Psychol.*, 62, 1961, 138-144.

Wilson, Oscar, and Bitterman,[13] quite the opposite strategy could be detected, the animals tending to shift sides after reinforcement, particularly after minority reinforcement. In the experiment now to be reported, probability-learning in the cockroach was studied under conditions like those which have produced random matching in fish and pigeon.

*Subjects.* The *S*s were 12 mature female cockroaches of the species *Nauphoeta cincera*, selected from the laboratory colony. A short length of nylon thread was attached to the thorax of each animal with melted paraffin. (Properly applied, the wax and thread do not impede movement of wings or limbs, but greatly facilitate handling.) Each *S* was kept in a covered compartment of black plastic (5 in. long, 4 in. wide, and 2 in. high) fitted with a guillotine door, which served both as a living cage and a goal-box. The animals were fed the usual colony-diet of apples and hamburger twice each week.[14]

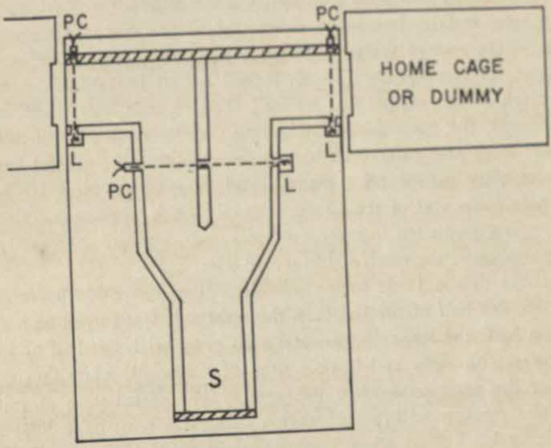

FIG. 1. THE *Y*-MAZE

(*S*, starting box; *PC*, photocell; *L*, photocell lamp. The hatched areas are coated with silver circuit-paint and connected to the voltage-source. The area in between is coated with a high-resistance paint.)

*Apparatus.* The *Y*-maze is diagrammed in Fig. 1. Its walls, floor, and cover were made of ¼-in. Plexiglas. The walls were ½ in. high, which permitted free running but prevented climbing on walls and cover. The starting box was 1½ in. long, and the two alleys were 4 in. long; the path-width was 1½ in. throughout.

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[13] W. A. Wilson, Jr., Marlene Oscar, and M. E. Bitterman, Probability learning in the monkey, *Quart. J. exp. Psychol.*, (in press).

[14] For information on the maintenance of cockroaches in the laboratory, see P. G. Piquett and J. H. Fales, Rearing cockroaches for experimental purposes, U. S. Dept. Agric., ET-301, 1952, 1-12.

The shocking technique employed was that of Longo, Holland, and Bitterman.[15] The floor and walls of the maze served as a shocking surface, its resistance measured between front and back walls being 8000 ohms. A potential-difference of 100 v. A.C. applied between these two walls was sufficient to shock animals standing anywhere on the surface with high reliability. The advantages of this technique lies in the fact that the surface is not shorted by watery exudates from the cockroach. It is, in fact, impossible to shock these animals *reliably* with a conventional grid.

Photoelectric circuits were used to provide an objective index of choice, to monitor errors, and to measure running times. The breaking of a light-beam 1 in. beyond the choice-point triggered a relay which served as an objective indicator of error for both alleys. The roach was picked up by its nylon thread (with forceps), placed in the starting box, and its home-cage positioned at the end of the correct alley. A dummy-cage at the end of the incorrect alley prevented escape and provided exteroceptive balance. A foot-operated switch locked on an Industrial Timer and started an electric clock as the animal was released. Ten seconds later, a pulsed shock was delivered through the maze-floor, terminating only when the roach ran to the end of the correct alley and entered its home-cage. Breaking the photo-beam at the end of the correct alley turned off the shock and stopped the clock. If *S* entered its home-cage before the end of the 10-sec. interval, shock was avoided altogether. The experiments were conducted in a room illuminated only by a 40-w. red lamp which permitted *E* to observe the roach and to record data.

*Procedure.* Six of the 12 *S*s were randomly assigned to a 100:0 group and six to a 70:30 group. For half of the 100:0 *S*s, the right side was correct on every trial; for the remaining half, the left side was correct on every trial. For half of the 70:30 *S*s, right was correct on 70% and left on 30% of the trials, while the opposite ratio was used for the remaining *S*s in this group. The distribution of minority reinforcements was random with the restriction that there must be at least one in each block of five trials. Twenty trials a day were given for 13 days, after which all *S*s were shifted to the 0:100 problem; that is, the previous minority side (0% or 30%) now was always correct. When all *S*s had reached a stable level of performance on this problem (7 days), they were reassigned to two groups matched on the basis of the 0:100 performance. (Since one *S* died after this problem, the total *N* was reduced to 11.) One group ($N = 5$) was assigned to the 40:60 problem, and a second group ($N = 6$) to the 20:80 problem, for 8 days, after which (a stable level of performance having been attained) all *S*s were shifted to the 50:50 problem. In the 50:50 case, the correct side on each trial was chosen in accordance with Gellermann orders.

*Results.* The course of learning in the 100:0 and 70:30 problems is shown in Fig. 2. The 100:0 animals began slightly above the chance-level and quickly developed a strong preference for the 100% side—about 90% by Day 8. The 70:30 group began at about the same level and, with somewhat more daily fluctuation, reached the 70% level on about Day 5; that level was maintained on subsequent days. The mean percentage of majority-responses for the 100:0 animals on the last 6 days (8-13) was 91.3 with a range of 85-95. The mean percentage to the majority-side for the 70:30 group was 70.1 (with a range of 61-81). There was no overlapping at all between the two groups.

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[15] Nicholas Longo, L. R. Holland, and M. E. Bitterman, The resistive sheet: A gridless and wireless shocking technique, this JOURNAL, 74, 1961, 612-618.

Performance on the 0:100 problem also is shown in the upper portion of Fig. 2. Both groups adjusted to the change rather rapidly, reaching a low level of error (about 10%) by Day 20. It is interesting to note that the decline was more rapid in the animals previously trained at 100:0 than in those previously trained at 70:30. The two curves cross between the first and second day on the new problem, demonstrating an effect like that of partial reinforcement on resistance to extinction. An analysis of variance (Lindquist's Type I) for transformed (arcsin) error-scores showed the simple interaction of the two distributions to be significant beyond

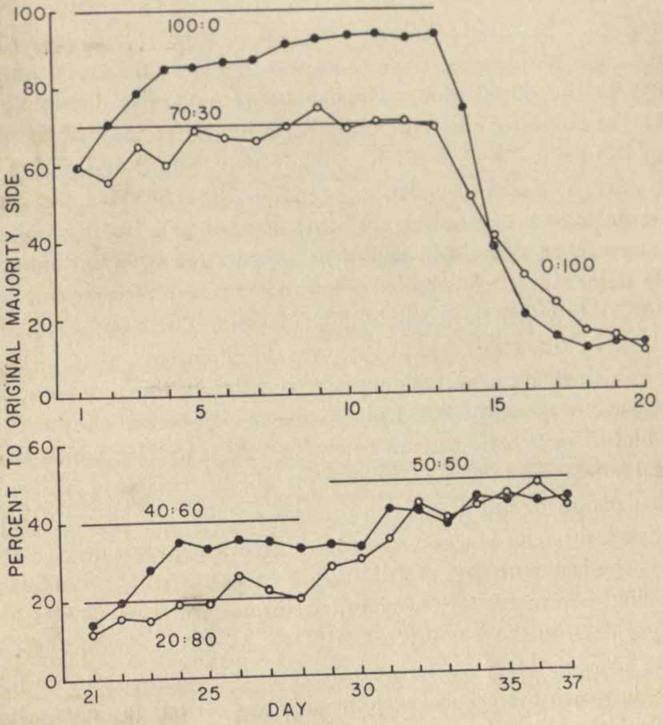

FIG. 2. PROBABILITY-MATCHING IN EXPERIMENT I

the 1% level. The analysis was based only on the post-shift data (Days 14-20), thus providing the severest test of the reliability of the effect. Earlier experiments with fish and pigeon yielded no such cross-over; the 70:30 and 100:0 groups did not reverse at significantly different rates.

The lower portion of Fig. 2 shows performance on the 20:80 and 40:60 problems, and on the final 50:50 problem. After four days on the 20:80 and 40:60 problems, both groups reached what seemed to be stable levels of performance. The mean number of responses to the original majority-side (now minority-side) for Days 24-28 was 22.8% in the 20:80 group and 34.8% in the 40:60 group. Overlap between the two distributions was slight, the range for the 20:80 problem being 16-32 and that for the 40:60 problem being 29-42. It is interesting to note that fish and pigeon as well as cockroach tended to undershoot the 40% level by about the same amount in analogous experiments. When shifted to the 50:50 problem, both groups reached a level of about 45% in just a few days. All animals previously trained at 40:60 showed some increase in preference for the former 40% side when shifted to 50:50, indicating that a change in the reinforcement-ratio as small even as 10% was discriminated.

In general, then, the cockroach appears to match in these problems in much the same way as the fish and the pigeon. Inspection of the data gives no hint of systematic patterns of choice similar to those which have been found in rats and monkeys. For example, the percentages of majority-choice after majority- and after minority-reinforcement are almost identical. After majority-reinforcement, roaches chose the majority-side on 72% of trials in the last four days of 70:30 training (Days 10-13), while the corresponding percentage after minority-reinforcement was 71%. For the last four days on the 50:50 problem (Days 34-37), a similar analysis shows that 42% of the responses were made to the original majority-side after trials in which the original majority-side was correct, and 45% after trials in which the original minority-side was correct.

*Discussion.* The implications of these results are fairly evident. Random matching has now been found in each of the three submammalian forms in which it has been sought (cockroach, fish, and pigeon), but not in rat and monkey. There exists, then, the possibility of a mammalian-submammalian behavioral dichotomy, which is an exciting prospect indeed. The only comparable dichotomy known is that for the ability to learn in synaptic vs. presynaptic forms (with ability to learn defined in terms of the standard Thorndikian and Pavlovian paradigms). Both dichotomies require further substantiation, of course, since a null outcome in a series of experiments with a given form may mean only an inadequacy of technique. The question of probability-learning obviously should be studied further, not only at the mammalian level, but also in the submammalian vertebrates which have not thus far been examined—the amphibians and the reptiles.

The high level of accuracy shown by the animals of this experiment in the 100:0 and 0:100 problems, and the sensitivity which they displayed to small changes in the ratio of reinforcement, testify to the effectiveness of the technique employed. It should be noted in this connection that extensive work with the cockroach in a situation patterned after the mammalian shuttle-box yielded no indication of learning except a decline in escape-time.[16] Why the maze-technique is so much more effective than the shuttle-box, it is impossible to say, but it is clear that the avoidance-feature is extremely advantageous. Earlier work with the *Y*-maze under pure escape-conditions (that is, with shock turned on as soon as the trial began) produced only disorganized behavior, with many repetitive errors and a high rate of mortality.

## EXPERIMENT II: HABIT-REVERSAL

Progressive improvement in habit-reversal, a phenomenon easily demonstrated in the rat,[17] has been difficult to find in the fish.[18] The pigeon, which in experiments on probability-learning functions like the fish rather than like the rat, functions like the rat in reversal-learning experiments—that is, it shows progressive improvement,[19] and some indication of progressive improvement has appeared in the turtle.[20]

There has been some study of this phenomenon also in invertebrate forms. A recent experiment with the earthworm has yielded a reversal-function of much the same form as that which is found in the rat, but there is evidence that the basis of improvement is different at the two levels; Datta's results suggested that the improvement shown by her worms was independent of reversal-experience and a function only of general experience in the experimental situation.[21] Of four previous studies with arthropods, two—one by Yerkes[22] and another by Schneirla[23]—are too fragmentary to permit any sort of conclusion. A third, by Thompson, showed no significant improvement in the sowbug, although there was some indication that it might have appeared if the training had been carried further.[24] A fourth, by Datta, Milstein, and Bitterman, gave no indication of progressive improvement in the crab.[25] In the experiment to be reported here, evidence of progressive improvement in habit-reversal was sought in the cockroach.

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[16] Longo, Some comparative studies of learning in the cockroach. Unpublished Doctoral dissertation, Bryn Mawr College, 1963, 1-60.

[17] A. J. North, Improvement in successive discrimination reversals, *J. comp. physiol. Psychol.*, 43, 1950, 442-460; R. H. Dufort, Norman Guttman, and G. A. Kimble, One-trial discrimination reversal in the white rat, *J. comp. physiol. Psychol.*, 47, 1954, 248-249.

[18] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110.

[19] Bullock and Bitterman, *op. cit.*, *J. comp. physiol. Psychol.*, 55, 1962, 958-962; work with the chicken has also given evidence of reversal learning (J. M. Warren, K. H. Brookshire, G. G. Ball, and D. V. Reynolds, Reversal learning by White Leghorn chicks, *J. comp. physiol. Psychol.*, 53, 1960, 371-375).

[20] K. L. Kirk and M. E. Bitterman, Habit reversal in the turtle, *Quart. J. exp. Psychol.*, 15, 1963, 52-57.

*Subjects.* Eighteen cockroaches of the species *Nauphoeta cincera* were harnessed and maintained under the conditions of the previous experiment.

*Procedure.* The *Ss* were randomly assigned to three equal groups. A Control Group was consistently reinforced on the same side for 44 days, three animals on the left and three on the right; for a Daily Reversal Group, the positive side was shifted each day for 44 days; and for a Four-Day Reversal Group it was shifted every four days for a total of 10 reversals beyond the original problem (Reversal 0). At the end of this period, the Control and Four-Day groups were shifted to the daily-reversal procedure, and the training of all three groups was continued for 12 more days. Ten trials per day were given, and all other procedures were the same as in the probability-learning experiment.

*Results.* The performance of the three groups in the first stage of the experiment is shown in Fig. 3, which is plotted in terms of the mean number of initial errors per day for blocks of four days. The Control Group, which had the same problem for 44 days, showed the rapid improvement found in the 100:0 group of the probability-learning experiment. By Day 9, its mean error-level had fallen to about 10%, and, during the next 35 days, all control animals had long periods of error-free performance. The Four-Day Reversal Group also showed some improvement over the series of reversals, but an analysis of variance based on transformed (arcsin) error-scores showed no significant change in performance.

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[21] L. G. Datta, Learning in the earthworm, *Lumbricus terrestris*, this JOURNAL, 75, 1962, 531-553.

[22] R. M. Yerkes and G. E. Huggins, Habit formation in the crawfish, *Cambarus affinis*, *Harvard Psychol. Stud.*, 1, 1903, 567-577.

[23] T. C. Schneirla, Learning and orientation in ants, *Comp. Psychol. Monogr.*, 6, 1929, 1-143.

[24] Robert Thompson, Successive reversals of a position habit in an invertebrate, *Science*, 126, 1953, 163-164.

[25] L. G. Datta, Sandra Milstein, and M. E. Bitterman, Habit reversal in the crab, *J. comp. physiol. Psychol.*, 53, 1960, 275-278.

The improvement of the Daily Reversal Group was more dramatic than that of the Four-Day Reversal Group and did prove to be statistically significant at the 1% level. Plots of daily performance for the Daily Reversal and Control groups (not here presented) are essentially parallel functions which look very much like those of Fig. 3. The daily plot for the Four-Day Reversal Group shows a chain of negatively accelerated learning curves, each of which begins at a level slightly above that of the Daily Reversal Group and falls in four days to that of the Control Group.

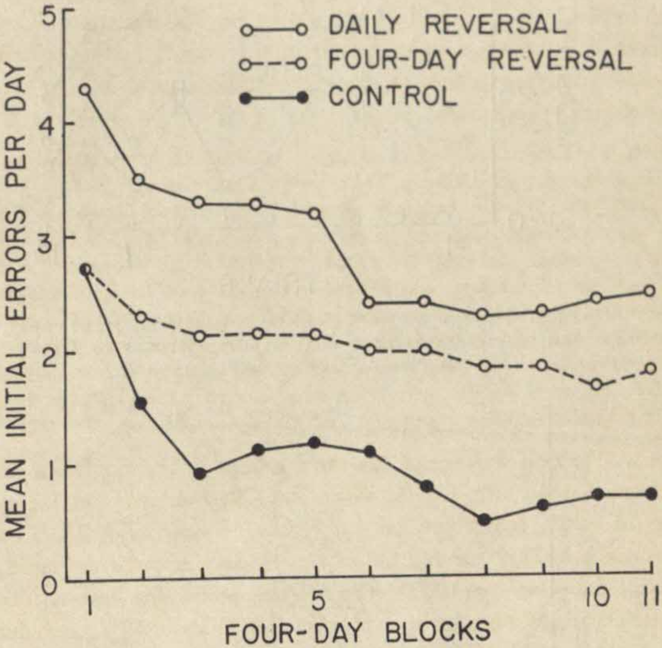

FIG. 3. MEAN INITIAL ERRORS IN THE FIRST STAGE OF EXPERIMENT II

Fig. 4 shows the course of within-days learning in the Daily Reversal Group. The curve for the early days (1-4) is relatively flat, beginning at about the chance-level and falling in an essentially linear fashion. The curves for the later days are steeper, beginning at a higher level than the first and falling to a lower one. Analysis of the data gave no indication that any further change in curve-shape might be expected.

Fig. 5 shows the performance of the three groups in terms of the mean

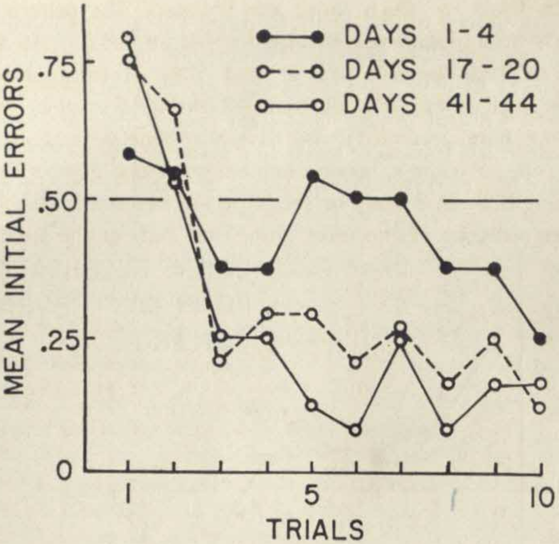

FIG. 4. THE COURSE OF WITHIN-DAYS IMPROVEMENT IN THE DAILY REVERSAL GROUP FOR SELECTED BLOCKS OF DAYS IN THE FIRST STAGE OF EXPERIMENT II

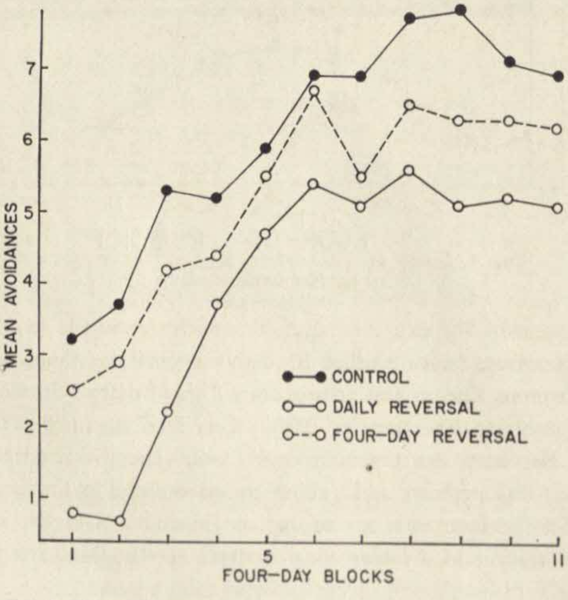

FIG. 5. MEAN NUMBER OF AVOIDANCES IN THE FIRST STAGE OF EXPERIMENT II

number of trials in which shock was avoided. The pattern of improvement bears an obvious resemblance to that in Fig. 3. At asymptote all groups were avoiding shock on at least 50% of trials, and the Control Group on well over 70% of trials. Running-time plots for the three groups (not here presented) also have the same general shape, which is not surprising, of course, because number of errors, number of avoidances, and running-time in a maze of this type are necessarily correlated.

The performance of the three groups on each of the last four days of

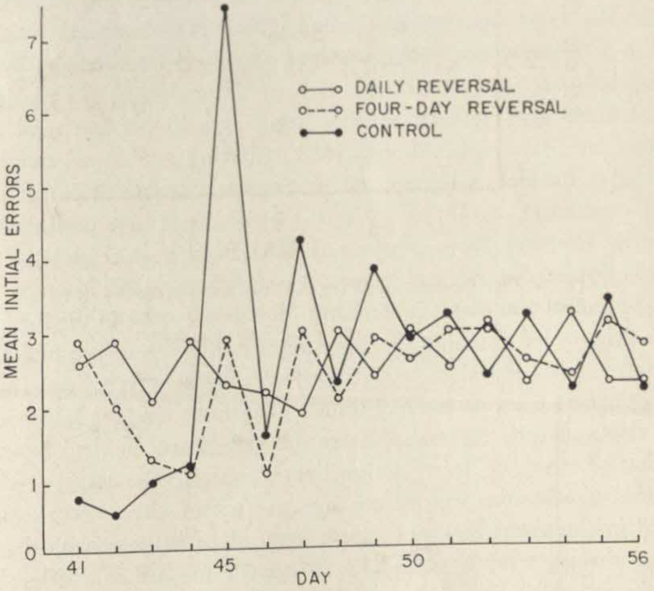

FIG. 6. MEAN INITIAL ERRORS IN THE SECOND STAGE OF EXPERIMENT II

the first stage of the experiment, and on each day of the second stage (in which all groups were shifted to daily reversal), is shown in Fig. 6. The precipitous rise in the error-score of the Control Group on Day 45 shows strong negative transfer from 44 days of training on the previous problem. Recovery was rapid, however, and, after five reversals, the Control Group was performing at about the same level as the Daily Reversal Group. The performance of the shifted Four-Day Reversal Group shows equal flexibility, and, by Day 50, the curves for the three groups reach the same level.

*Discussion.* In the second stage of the experiment, the Control Group reached in about five days a level of performance which the Daily Reversal Group had taken about 24 days to reach in the first stage of the experiment. This suggests that much of the improvement in the Daily Reversal Group which appeared in the first stage may be attributed to experimental experience quite apart from reversal-experience. One might even hazard a guess that if the spatial biases acquired by the cockroach were as temporally unstable as they are in the earthworm (where they disappear almost entirely in the 24-hr. intersession interval), the performance of the Control Group would have been as good as that of the Daily Reversal Group from the very outset of the second phase. Fig. 4 certainly need not be taken as evidence of anything beyond a general improvement in learning efficiency—an increase in the rate of improvement within-days. The error-curves for the later sessions begin at a higher level and fall to a lower one, the higher initial level reflecting the lower terminal one. Negative transfer only *increases* progressively from session to session in this experiment, as a plot of Trial-1 errors (not here presented) shows clearly. *Recovery* from negative transfer, which is characteristic of reversal-learning in rat and pigeon,[26] is nowhere in evidence. It is interesting that of the many experiments on habit-reversal in the fish, the only one that yielded anything like progressive improvement also failed to show recovery from initial negative transfer.[27] While reversal-training in the rat generates a set of parallel learning functions showing improvement at all stages of each reversal, the one fish experiment to yield improvement generated a set of learning functions progressively steeper in slope—a pattern of improvement which suggests that of the present experiment. Such improvement may not appear more often in work with the fish because the pretraining procedures in current use are so effective that all of the general learning is complete before experimental training is begun. It should be noted that the Wodinsky-Bitterman experiment in question was the first of the series and done at a time when little was known about experimental work with fish.

Much more remains to be done before these matters are understood. The analysis of reversal-learning, even at the mammalian level, has, in fact, barely been begun. Comparative studies already indicate, however, that improvement is far from a unitary affair and provide the framework for a more detailed analysis of the phenomenon.

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[26] Bitterman, Wodinsky, and Candland, *op. cit.*, 94-110; Bullock and Bitterman, *J. comp. physiol. Psychol.*, 55, 1962, 958-962.

[27] Wodinsky and Bitterman, Discrimination-reversal in the fish, this JOURNAL, 70, 1957, 569-576.

## SUMMARY

Two experiments are reported in which the behavior of the cockroach was studied under conditions analogous to those which are used for the study of learning in higher animals. The apparatus employed was a *Y*-maze, with correct choice rewarded by avoidance of or escape from shock. The first experiment, on probability-learning, yielded clear evidence of the random matching which has been found in fish and pigeon but which has not thus far been found in mammals. The second experiment, on habit-reversal, gave some indication of progressive improvement, but analysis of the data suggested that the underlying process in the cockroach might be more like that in earthworm and fish than like that in pigeon and rat.

# EFFECTS OF DOUBLE SIMULTANEOUS STIMULATION OF THE SKIN

By CARL E. SHERRICK, JR., Princeton University

When two transient pressures of equal intensity are presented to the skin simultaneously, *O*'s phenomenal report of the sensation will vary depending on the relative loci of the two stimuli. If the stimuli are a few centimeters apart, *O* will often report a single pressure, a 'phantom' lying between the two actual stimuli. If the stimuli are as far apart as on opposite sides of the body, both may be felt, though displaced from the position occupied by either stimulus when it is presented alone. Still other effects may be noted if the two stimuli are varied in their relative intensities and time relations.

Von Békésy has recently reported a series of experiments that included manipulations of intensity and phase-relations between vibrotactile stimuli.[1] He has suggested that a functional characteristic of the nervous system, which he calls funneling, explains many of the effects produced by multiple simultaneous stimulation of all sensory surfaces commonly studied, *i.e.* the eye, the ear, and the skin. According to Von Békésy's hypothesis, a single supraliminal stimulus to the surface produces a central core of excitation surrounded by an annular zone of refractoriness. When two stimuli are presented to the surface, the areas of excitation and refractoriness may overlap and summate algebraically to produce a resultant pattern of excitation that will vary according to the degree of temporal and spatial overlap, and according to the intensities of the two stimuli. The major advantage of this hypothesis lies in the possibility for the explanation of the clarity of stimuli to *O* when the physical conditions of stimulation would seem to induce only diffuse or weak neural effects.

Recently, in related experiments, Uttal and Schmid have reported on the effects of multiple stimulation by electric currents to the fingers.[2] Uttal demonstrated a kind of 'masking' effect produced by stimulating a finger on one hand with a supraliminal pulse of electrical current and measuring the threshold shift on other fingers of the same hand to a signal pulse simultaneously presented. Threshold-

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* Received for publication August 7, 1961; revision received April 29, 1963 This work was done at the Central Institute for the Deaf in St. Louis, Missouri, with support from grants (G-4457 and G-9769) from the National Science Foundation.

[1] George von Békésy, Similarities between hearing and skin sensations, *Psychol. Rev.*, 66, 1959, 1-22.

[2] W. R. Uttal, Inhibitory interaction of responses to electrical stimuli in fingers, *J. comp. physiol. Psychol.*, 53, 1960, 47-51; Ethel Schmid, Temporal aspects of cutaneous interaction with two-point electrical stimulation, *J. exp. Psychol.*, 61, 1961, 400-409.

shifts ranged from about 2 to 10 db. depending on the masking-finger, the intensity of the masking-pulse, and the test-finger.

Schmid's experiments extended Uttal's results by examining the variation of threshold-shifts to electrical pulses when time between onset of the masker and the test-signal was varied systematically. She found that the threshold-shift rose steeply from a value near zero when test-signal preceded masker by 10 m.sec. to a maximum of about 4 db. when the masker preceded the test-signal by about 5 m.sec. The curve then fell slowly as the temporal lead of the masker increased, and threshold shift approached zero as the lead approached 40 m.sec.

An analogous experiment in audition was performed by Raab.[3] Supraliminal clicks were presented monaurally at various time-intervals preceding and following test-clicks. The threshold-shift of the test-clicks varied according to the intensity of the masking-clicks and the time-interval between masker and test-click. The function bore some resemblance to that obtained by Schmid in that the rate of rise to the maximal value of threshold-shift was more rapid when test-signal preceded masker than when masker preceded test-signal. The maximal threshold-shift observed by Raab amounted, however, to 50 db. A study by Elliott, in which brief tones and noises were used as signal and masker, yielded results very similar to those of Raab.[4]

Raab's recent review of the literature on backward masking points up the current interest in both research and theory relating to temporal factors in multiple stimulation of the eye, the ear, and the skin.[5] A second point of significance in Raab's paper is the relative paucity of research reports on the effects of manipulation of time, locus, and intensity of stimulation of the skin.

The experiments to be reported here were suggested by an observation in a study by Hirsh and Sherrick.[6] When perceived temporal order of two subjectively equal stimuli in the same modality was studied, it was found that occasionally *O* would report no stimulation by one of the stimulus-sources. This occurred in the visual modality and the tactual modality sufficiently often to convince us that failure of stimulation in the physical sense was not the basis for the effect. The experiments reported above serve to suggest the probable basis for the phenomenon. Experiments were designed, therefore, that would delineate the phenomenon in the perception of pressure and vibratory forces.

## EXPERIMENT I

An adequate demonstration of the effect of double simultaneous stimulation of the skin may be accomplished by shifting the relative intensities of two supraliminal stimuli until the weaker disappears. The first experiment illustrates this phenomenon and its relation to the loci of the stimuli.

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[3] D. H. Raab, Forward and backward masking between acoustic clicks, *J. acoust. Soc. Amer.*, 33, 1961, 137-139.

[4] L. L. Elliott, Backward masking: Monotic and dichotic conditions. *J. acoust. Soc. Amer.*, 34, 1962, 1108-1115.

[5] Raab, Backward masking, *Psychol. Bull.*, 60, 1963, 118-129.

[6] I. J. Hirsh and C. E. Sherrick, Jr., Perceived order in different sense modalities, *J. exp. Psychol.*, 62, 1961, 423-432.

*Apparatus.* A diagram of the apparatus is shown in Fig. 1. The outputs of two Tektronix Type 161 pulse-generators were amplified, controlled in their intensity by attenuators, and impressed on Goodmans Type V-47 vibrators through matching transformers. The systems were electrically and mechanically independent of one another, except that a Tektronix Type 162 wave-form generator triggered both pulse generators. The onset times of the pulses produced could be varied independently by *E*. In this experiment *O*'s attenuator (OBS ATTEN) shown was not used. The mechanical pulse produced by the vibrator was recorded by an MB Type M-3 vibration meter and M-115 pickup. The displacement amplitude of the pulse as a function of time can be seen in Fig. 2, and this may be taken as the form of the pulse that stimulated the skin.

The vibrators were so mounted on platforms that *O* could be stimulated at any desired point on the body with contactors made of short polystyrene rods, 6.5 mm. in diameter. A Berkeley Model 7360K counter was used to time the interval between the pulses to the vibrators and the pulses were presented in pairs at the rate of 1 pair per 1.5 sec.

The *O*s, three men, were carefully trained to observe the transient pressure-stimuli on various skin-areas: the left index fingertip (LIF); the left palm at the base of the thumb (LP); and the right little finger (RLF). Each of these areas was paired with the right index finger (RIF) in order to present the required double stimulation. Other areas were examined, but for one reason or another these proved to be unsuitable for the present study. For any given pair of areas chosen, absolute thresholds were obtained. Then the stimulus-intensity was raised to a sensation-level of 20 db. on the right index finger. (Sensation-level is used in the acoustic sense, as a level in decibels defined relative to the absolute threshold.) The intensity required to equate the test-areas (LIF, LP, RLF) to the reference area (RIF) was not necessarily 20 db. sensation-level, since the 'loudness' on the test-area often grew more rapidly than it did on the reference area. Therefore, the intensity of the test-area was changed until *O* reliably reported that the perceived intensities of the two sensations presented successively were the same.

*Results.* When this adjustment was made, and the stimuli were presented simultaneously, *O* perceived either sensations at both areas, or a phantom sensation between them, or a combination. The test-area was always maintained at the intensity-level thus obtained.

Masking of the test-area by the reference-area was obtained in the following manner. The intensity of the pulse on RIF was raised until *O* perceived at the test-area only one pulse out of every three presented. This value of intensity on the reference-area (RIF) was taken as the threshold for masking on the test-area of a stimulus having a 'loudness' equal to 20 db. sensation-level on RIF. Depending on the areas chosen, an intensity-ratio of 1:2 to 1:8 will produce masking of the less intense pulse for these conditions.

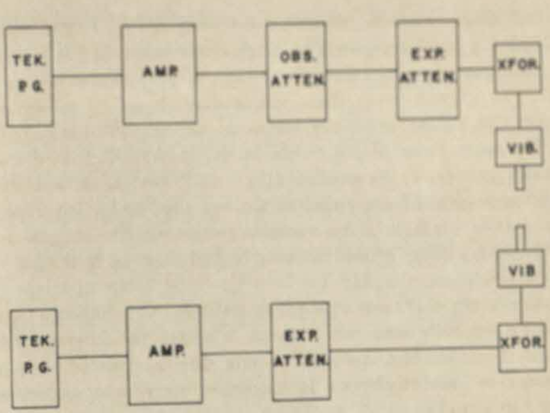

FIG. 1. BLOCK-DIAGRAM OF THE APPARATUS USED IN EXPERIMENTS I AND II

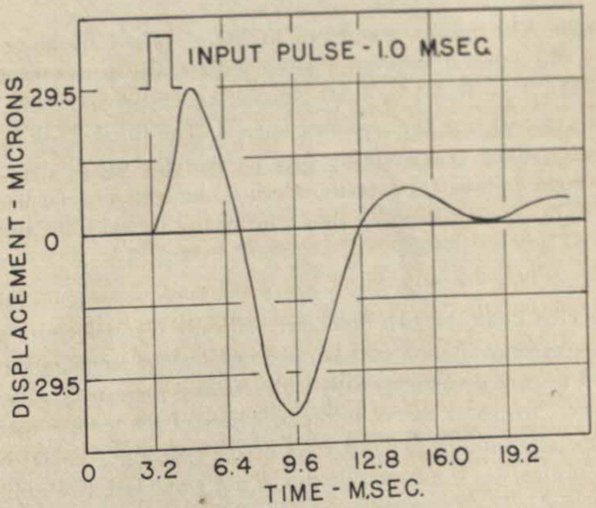

FIG. 2. RESPONSE OF THE VIBRATOR TO A 1-M.SEC. ELECTRICALLY RECTANGULAR D.C. PULSE
(Natural frequency of the vibrator is about 110 ∼.)

## EXPERIMENT II

The functional relation between time of onset of masker and test-signal and threshold-shift, as studied by Raab, Schmid, and Elliott, is the subject of the second experiment.[7]

*Apparatus.* The same apparatus was used as in Experiment I, except that a recording attenuator was placed as shown (OBS ATTEN) in the circuit of one system shown in Fig. 1. By manipulating a foot switch, *O* could increase or decrease the attenuation of the signal to the test-area, and in so doing draw a permanent record, on a graph, of his variation in threshold to the pulses as a function of time. Three *O*s were trained to observe and respond with this system. Three test-areas were chosen, the RIF (as both test- and reference-area), the RLF, and the LIF. When the RIF was studied as test-area, the outputs of the attenuators were mixed electrically and sent through a single matching transformer to one vibrator. In this condition, pulse-pairs were never presented at intervals of less than 15 m.sec. to prevent physical interaction in the vibrator system. Time-intervals of 100, 80, 40, 20, 15, 10, 5, 2.5 and 0 m.sec. were set between pulse-pairs with test-signals both preceding and following the masker for every time-interval. For every interval, *O* would first get a quiet threshold at the test-area, then a masked threshold, with the masker set always at 20 db. sensation-level on the reference area.

*Results.* The results are shown in Fig. 3 where masking is plotted against the interval between pressure-pulses. The curves generated are not unlike those found by Raab, Schmid, and Elliott. The RIF condition has as a special case the zero time-interval. The threshold-shift measured in that condition is actually the base level of the masking stimulus plus the difference limen for pressure, which at the level presented amounts to about 3 db. The data are also shown for one *O* in which the center of the upper lip was the test-area.

It is clear from the two experiments already reported that a great deal of masking or inhibition may be produced by one supraliminal transient pressure presented simultaneously with a second pressure. The locus of the interaction, moreover, is not in the periphery of the sensory system alone, since the mechanically isolated areas of the two index fingers have been shown to interact. If vibrotactile stimuli are employed, will similar effects be observed? The experiments of Von Békésy indicate that they should be, provided that the duration of the stimuli is not too great.[8]

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[7] Raab, *op. cit.*, 138; Schmid, *op. cit.*, 403; Elliott, *op. cit.*, 1110.

[8] Von Békésy, Funneling in the nervous system and its role in loudness and sensation intensity on the skin, *J. acoust. Soc. Amer.*, 30, 1958, 399-412.

## EXPERIMENT III

The tactual analogue of auditory masking effects with tones and noise is closer when the stimulus-patterns on the skin are vibratory. The third experiment was designed to study the masking of vibrotactile signals by a second vibratory stimulus presented simultaneously, as a function of the characteristics of the masker, the frequency of the test-signal, and the loci of test-signal and masker.

*Apparatus.* A diagram of the apparatus is shown in Fig. 4. The output of a General Radio Model 1304-B oscillator was electronically switched, amplified, attenuated, and set to the Goodmans V-47 vibrator through an impedance-matching transformer. *O*'s attenuator was the recording attenuator already described. In this experiment, the frequency dial of the oscillator was so connected mechanically to the chart drive of the attenuator that the threshold-level of the test-signal could be derived, after calibration, as a function of its frequency. The vibratory analogue of the Von Békésy audiogram was thus produced.[9] The vibrator used as masker was energized either by the output of a random-noise generator filtered by a 100 ~ low-pass filter or by a pure-tone oscillator. The signals of these generators were switched, amplified, and attenuated as shown. The two electronic switches

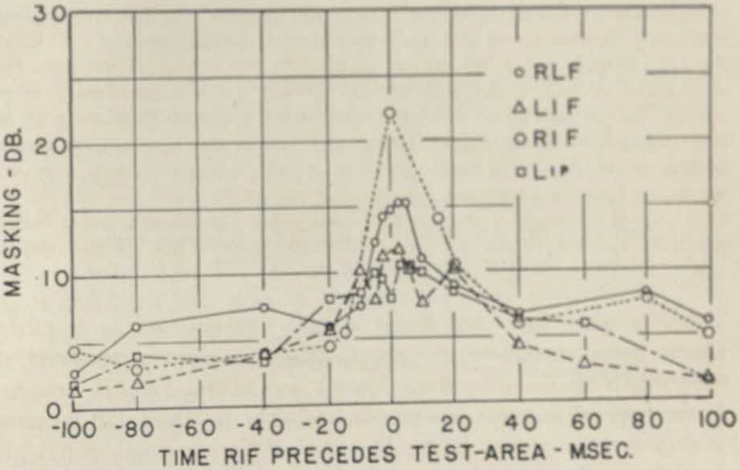

FIG. 3. THRESHOLD SHIFT, OR MASKING, OF ONE MECHANICAL PULSE BY A SECOND PULSE AT A SENSATION-LEVEL OF 20 DB AS A FUNCTION OF THE TEMPORAL ORDERING OF THE PULSES

(The masking stimulus was on the right index fingertip (RIF); the curves represent areas receiving the test-signal: RIF, left index fingertip (LIF), right little fingertip (RLF), and lip.)

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[9] Von Békésy, A new audiometer, *Acta Otolaryngol.,* 35, 1947, 411-422.

were triggered simultaneously at a rate of once per record. The stimulus was on for half the time and the rise- and fall-times were 60 m.sec. *O* then perceived a pulsing test-signal and a masker appearing at a rate of one vibrotactile burst per sec., and lasting for about one-half sec.

Each of four *O*s was tested separately as follows: A quiet absolute threshold recording was made for certain test-frequencies from 25 to 700 ~ at the test-area (LIF). The noise or tone was presented to the reference area (RIF or LIF), and *O* then obtained the masked threshold over the same frequency-range. When the LIF was studied as both test- and reference-area, the outputs of the attenuators were mixed electrically and sent through a single matching transformer to one vibrator. Masking is defined as the threshold-shift in decibels as before. The noise was set at a level of 40 db. *re* 0.1 micron (r.m.s.) displacement. The masking frequencies used were 50, 100, and 400 ~, and were set at 25 db. sensation-level.

*Results.* The results of this experiment are shown in Fig. 5. Since variation in frequency of the sinusoidal maskers did not produce a significant change in threshold-shift over the frequency-range tested, median values were taken and these are plotted. The greater masking effect of the noise is accounted for by the fact that the sensation-level of the noise was higher than for the pure vibrations. The difference between the two loci is obvious; the masking effect for the LIF-LIF condition is correlated with the difference limen for vibratory intensity, and the LIF-RIF condition is more like contralateral masking. Despite this difference in psychophysical conditions, the two frequency-functions are very similar. The greater amount of masking in the middle frequency-range, which is the range of maximal sensitivity for humans, may be explained by the variation in the growth of 'loudness' of different vibratory frequencies. Low- and high-frequency vibrations appear to increase more rapidly in perceived intensity than the middle frequencies. This was verified by having 2 *O*s match the 'loudness' of various frequencies to a standard 100 ~ intensity. The sensation-level at 30 ~ required to match the standard set at 25 db. sensation-level was 17 db. At 600 ~, the sensation-level required was 17 db. At frequencies between 70 and 300 ~ the sensation-level required was 23 to 25 db. The more rapid increase in perceived intensity at the lower and higher frequencies would necessarily lead to smaller threshold shifts.

## EXPERIMENT IV

Most studies of auditory masking have employed a steady masker and either a steady or pulsing test-signal.[10] The comparison of the effectiveness

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[10] C. E. Sherrick, Jr., and P. L. M. Albernaz, Auditory threshold shifts produced by simultaneously pulsed contralateral stimuli, *J. acoust. Soc. Amer.*, 33, 1961, 1381-1385.

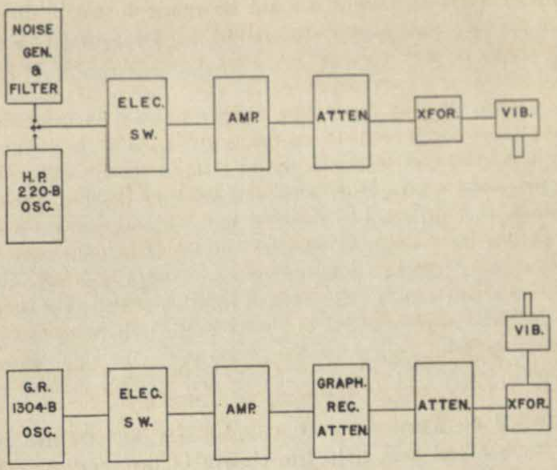

FIG. 4. BLOCK-DIAGRAM OF THE APPARATUS USED IN EXPERIMENTS III AND IV (NOT ALL APPARATUS WAS USED FOR BOTH EXPERIMENTS.)

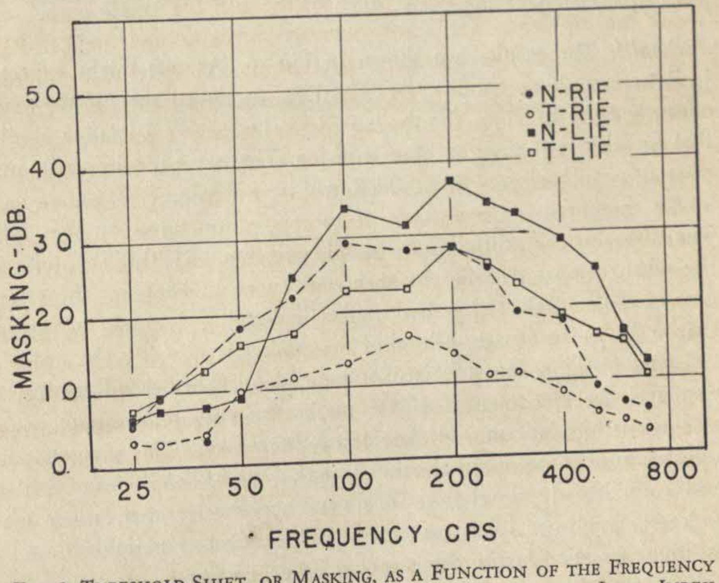

FIG. 5. THRESHOLD-SHIFT, OR MASKING, AS A FUNCTION OF THE FREQUENCY OF THE TEST-SIGNAL, WHICH WAS PRESENTED TO THE LEFT INDEX FINGERTIP (LIF)

(The masker was either low-frequency random noise (N) at 45 db. above 0.1 micron r.m.s. displacement, or sinusoidal vibrations of 50, 100, or 400 ~, (T), at 25 db. sensation-level. Masker was on LIF or right index fingertip (RIF). Median values for the frequencies used are shown.)

of steady *vs.* pulsing stimuli should be made to establish whatever difference there may be between the conditions. Experiment IV was designed to assess this difference as a function of the sensation-level of the masker.

*Apparatus.* The apparatus was the same as that used in Experiment III, except that the noise-generator and Hewlett-Packard oscillator were eliminated and a phase-shifting device was inserted after the General Radio oscillator. The two output circuits of the phase-shifter were connected to the two electronic switches. Both vibrators were activated by the same 150 ~-signal, and the phase-shifter was so adjusted that the mechanical vibrations of the transducers were in phase. The chart-drive of the recording attenuator was disconnected from the oscillator, and the graphic record of attenuation was made as a function of time. The electronic switches could be independently controlled to yield a pulsing signal of 350-m.sec. duration at both vibrators, with a silent interval of 650 m.sec., or a pulsing test-signal with these temporal characteristics at one vibrator and a steady masking-stimulus at the other.

The LIF and the RLF were used as masker-areas and the RIF as the test-area on three *O*s who had been trained to observe in the situation. Thresholds were obtained on the reference-area, and the masker-stimulus was set at either 0, 10, 20, 30, 40, or 50 db. sensation-level. For each masker-level and each *O*, three threshold-values were obtained: a quiet threshold, a masked threshold with a steady masker, and a masked threshold with a pulsed masker.

*Results.* The results are shown in Fig. 6. As one might expect, the ipsilateral masking condition (RIF-RLF) produced the greater masking effect, and the function for the pulsed masker shows a relation similar to that found in monaural auditory masking. The curve for the steady masker rises at a much slower rate, which may be a result of adaptation as well as the disappearance over time of the funneling effect of Von Békésy. The curve for the pulsed contralateral condition (RIF-LIF) is quite striking when compared with the analogous curve in hearing. The effectiveness of the contralateral pulsed masker in hearing does not increase more than 6 db. as the sensation-level of the masker is raised.[11] The steady contralateral masking function is of interest also, since beyond 40 db. sensation-level the effectiveness of the masker appears to decrease, probably as a result of adaptation at the site of the masker. This suggests further research to examine differences in adaptation under conditions of ipsilateral and contralateral stimulation. We may hypothesize that, when masker and test-stimuli are ipsilateral, the masking effect is peripheral, and that at high sensation-levels the steady masker produces adaptation that is peripheral. When masker and test-stimuli are contralateral the masking effect is central, and at high sensation-levels the steady masker produces adaptation that is both peripheral and central.[12] If the course of the two adaptation-processes may be quantified over time, the difference between them should be the central adaptation-process alone.

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[11] Sherrick and Albernaz, *op. cit.*, 1385.

## DISCUSSION

The effects described do not belong to that class of phenomena that require elaborate statistical design nor ultrasensitive psychophysical procedures for their detection. Control of the time of onset and relative intensities of the two stimuli is important, and reasonably well-trained *O*s can quickly show reliable responses. Completely naïve persons will perceive the gross effect of masking immediately. We are led to wonder, then, why the great amount of research on the two-point limen did not expose the phenomenon in normal *O*s much earlier. The present author suggests that the reason lies in the apparatus and techniques commonly used. Time- and intensity-control of punctate stimuli was not precise,

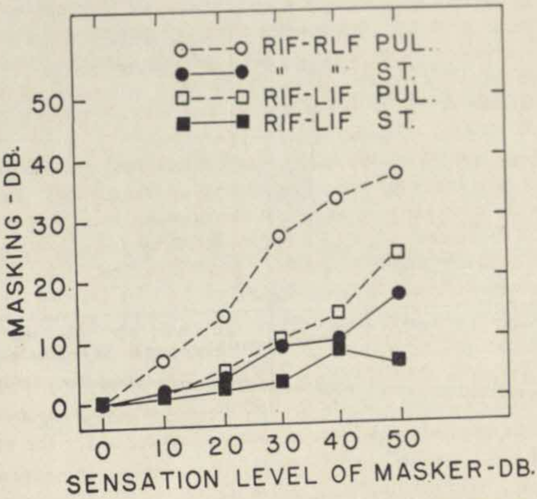

FIG. 6. THRESHOLD-SHIFT, OR MASKING, AS A FUNCTION OF THE SENSATION-LEVEL OF THE MASKER.
(The curves indicate the variation with locus of stimulation and whether the masker (RLF or LIF) was pulsed (PUL) or steady (ST).)

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[12] By central is meant the processes that accompany contralateral stimulation. The ipsilateral masking condition may produce central effects, but these would be present in the contralateral condition in equal measure and may be called peripheral for present purposes.

and acceptable procedure dictated against rocking the compass points to find the limen. The duration of point-pressures was, furthermore, great enough that the initial effects, which best produce masking, were themselves masked by the long after-stimulation that commonly occurred. The study by Klemm of the effect of short-time differences of pressures contains a qualitative description of many of the phenomena observed by the present writer.[13] Klemm did not, however, manipulate intensity relations between the stimuli. Von Frey and Pauli reported the effects of intensity-imbalance between stimuli in the determination of the two-point limen for pressure, but they did not manipulate the time-relations.[14]

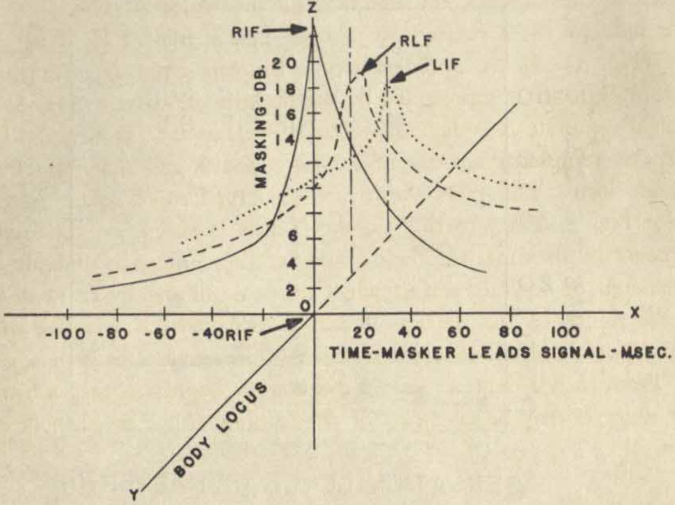

FIG. 7. THRESHOLD-SHIFT OF A TEST-SIGNAL AS A FUNCTION OF TEMPORAL ORDER OF MECHANICAL PULSES AND BODY LOCUS OF THE TEST SIGNAL.

The neural funneling hypothesis proposed by Von Békésy is in no sense tested by the present research. One may be led to speculate on the exact locus of the funneling process, however, in considering that contralateral and ipsilateral masking both exist, and that the effects of such masking form part of the basis for the hypothesis. Is funneling a nervous process that occurs in the periphery, at the site of the physical stimulation, or does it occur in the central nervous system only? Obviously, if the nerves in the periphery are zoned as excited or refractory by the process, such zoning will project centrally if a one-to-one relation obtains through synapses. If the latter case exists, we must assume that lateral, peripheral interconnections of the system provide for the zoning, or that effector systems from the center are activated to modify selectively the excitability of receptors or their nerves. In addition to the ambiguity of site of action, the temporal characteristics of funneling (which might provide some information about the site of activity) are not clear. From the present experiments, the duration of the temporal zone of relative silence induced by a masker was shown in Experiment II to be ± 20 m.sec. (Fig. 3). If we assume that the masking effect as a function of time for any locus is represented by the appropriate curve in Fig. 3, then we might generate a 3-dimensional plot such as that shown in Fig. 7. The Z-axis represents amount of masking, the X-axis time, and the Y-axis body locus. The plots shown are smoothed curves taken from the data for Fig. 3. The ordering on the Y-axis is simply on the basis of the amount of maximal threshold-shift, but the correlation of body locus with amount of shift is perfect when the scale of the Y-axis is distance from the reference area (RIF). It will be instructive to examine the intervening areas with the same technique, to determine the ordering that exists. From such a diagram should come a psychophysical map which the writer suggests may be an aspect of the solution to the problem of space in man.

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[13] Otto Klemm, Über die Wirksamkeit kleinster Zeitunterschiedes im Gebiet des Tastsinns, *Arch. ges. Psychol.*, 50, 1925, 205-220.

[14] Max von Frey and Richard Pauli, Die Stärke und Deutlichkeit einer Druck empfindung unter der Wirkung eines begleitenden Reizes, *Z. Biol.*, 59, 1913, 497-515.

# THE EFFECT OF GLOSS ON PERCEIVED LIGHTNESS

By JACOB BECK, Harvard University

The present experiments deal with the relation between the perceptions of lightness and of gloss. Judd has observed that surface-gloss affects perceived lightness.[1] He notes that if a spot on a black polished surface is finely ground, the spot will no longer appear black, but gray. The effect of surface-gloss on perceived lightness has not, however, been studied systematically. Undoubtedly the relationship between lightness and gloss stems from the fact that the directional reflectance of a glossy surface is not constant,[2] but the interpretation must be more specific. It is of interest, in any case, to study the processes entering into the perception of surface-lightness when the luminance of the surface is not uniform.

Four experiments were conducted. Experiment I compares the judgments of lightness of a smooth matte and a smooth glossy surface as a function of the contrast-ratio of the luminances of the surround and of the target. Experiment II compares the judgments of lightness of the same surfaces when the contrast-ratio of the target to the surround is constant as the illumination is varied. Experiments III and IV test the proposition that lightness-perception of a glossy surface can be analyzed as a two-step process: (1) contrast acts to determine a pattern of lightness in accordance with the distribution of luminances reflected and (2) as a result of the cue-properties of the stimulus, the lightness-pattern is organized in terms of a single lightness with deviations due to highlights and light-spots.

## EXPERIMENT I

In Experiment I, a smooth black paper and a glazed black tile both were illuminated directionally. The luminance of the paper was uniform because it diffusely reflected the light, while the luminance of the tile was non-uniform because of its specular reflection. The average luminances of the paper and tile were equated. Photographs of the paper and tile are

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* Received for prior publication October 7, 1963. This study was supported by Grant No. MH 05120-02 from the Public Health Service to Harvard University Center for Cognitive Studies.

[1] D. B. Judd, *Color in Business and Industry*, 1952, 305.

[2] R. S. Hunter, Methods of determining gloss, *J. Research Natl. Bur. Standards*, 18, 1937, 19-39. For a phenomenological study, see F. L. Bixby, A phenomenological study of luster, *J. gen. Psychol.*, 1, 1928, 136-174.

shown as *A* and *B* in Fig. 1.[3] The luminance-gradients on the tile appear in the photograph as varying shades of gray.

*Method.* A large rectangular box, from whose front side a rectangular window had been cut, was divided into a front and a back chamber by means of a screen placed crosswise. A 1.5-in. square aperture was cut in the center of the screen. The target-surfaces were set on a shelf 32 in. behind this aperture. A chin-rest was placed 110 in. from the target and 43 in. from the front side of the box. When *O* placed his chin in the chin-rest, he could look through the window and see a target 3.25 × 2.125 in. in the center of a 15 × 12-in. surround. (Fig. 1).

Illumination of the target and surround was provided by separate 150-w. flood lamps placed out of *O*s' line of sight in each chamber. The average luminances of the matte and the glossy targets were set at 0.3 ft.-L. The luminances of the surround were set at 0.3, 0.6, 1.2, 1.8, 2.4, 3.6, and 4.8 ft.-L. Checks assured that changing the luminances of the surround did not alter the luminance of the target. The luminances of the target and the surround were controlled by metered variacs. All measurements of luminance were made with a Spectra brightness-spot meter.

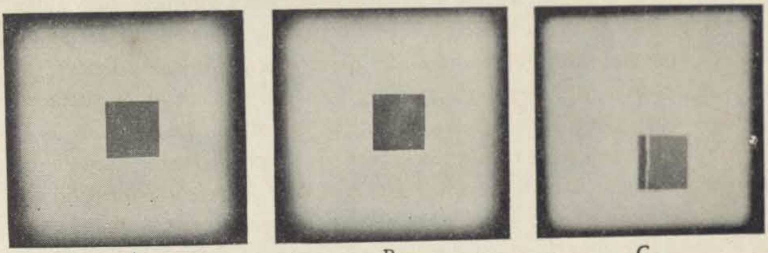
A B C

FIG. 1. PHOTOGRAPHS OF TARGETS

*A* shows the matte target illuminated directionally; *B* the glossy target illuminated directionally, and *C* the glossy target illuminated uniformly reflecting a metallic rod. In *A* and *B*, the camera was placed in the same position as *O*'s head. In *C*, the camera was raised.

Each *O* was seated in the dark room before the chin-rest and told that when the shutter was raised he would see a rectangular target in the presence of a surround. He was told that his task would be to judge the lightness of the target by matching it with one of the grays on a Munsell neutral-value chart which was attached to the front of the box. *O* was asked to check his match by viewing the target a second time, and he was allowed a third examination if still uncertain. The chart was illuminated by a 25-w. lamp which was turned on after *O* viewed the target. Following each lightness-judgment, *O* was shown the target again and asked to judge the gloss of the surface on a 4-point scale. He was told to assign to the surface a scale-value of 0 if it appeared matte, a scale-value of 3 if it appeared glossy, and a scale value of 1 or 2 if it appeared closer to matte or glossy, respectively, but not fully either. To standardize the *O*s' judgments, they were shown examples of matte and glossy surfaces. The luminance-levels of the surround were presented in a different random order for each *O*. No time-limitations were imposed on their observations.

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[3] The photograph of the matte paper was printed to match a gray on the Munsell scale which was equal to the median of *O*s' lightness-judgments at the contrast-ratio photographed. The photograph of the glazed tile was then printed under the same conditions and its lightness allowed to fall where it would. A time-exposure was necessary in taking these photographs because of the low light-levels. This introduced distortion into the photograph of the glazed tile due to the failure of reciprocity.

*Observers.* The 21 *O*s made judgments on both the paper and the tile. A balanced order was employed, with half the *O*s viewing the paper first and half the tile first. All the *O*s were undergraduates, naïve regarding the experiment, who were paid to participate.

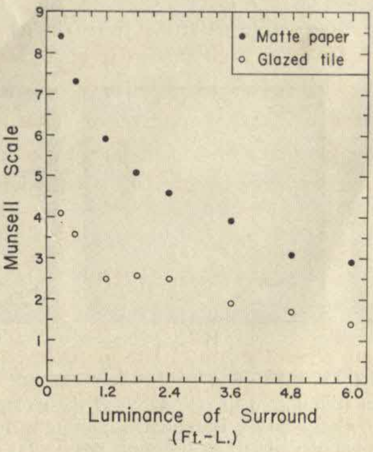

FIG. 2. MEAN LIGHTNESS-JUDGMENTS OF MATTE PAPER AND GLAZED TILE IN EXPERIMENT I

The paper and tile were illuminated directionally, and the points represent judgments at different contrast-ratios.

*Results.* Fig. 2 presents the mean lightness-judgments for paper and tile at the different luminance-levels of the surround. As would be expected on the basis of contrast, increasing the luminance of the surround darkens both the surfaces of paper and tile in a similar fashion. There is, however, a marked difference in the mean lightness-matches at equal contrast-ratios. The tile surface was consistently judged darker than the matte surface. With increasing luminance of the surround, the mean of the *O*s' lightness-judgments of the matte surface went from a Munsell value of

8.4 to a value of 2.9, while that of the tile went from a Munsell value of 4.1 to a value of 1.4.

Correlated with the differences in the perceived-lightness of the paper and tile was a difference in their surface-appearance. The luminance-gradients on the tile surface produced the impression not of a surface of varying shades of gray (as shown in *B* of Fig. 1), but of a shiny insistent surface of uniform lightness. The mean of the *O*s' gloss-judgments at the different luminance-levels of the surround was 2 for the tile and 0.8 for the paper surface ($p < .004$ by the Sign Test). The luminance-gradients on the tile surface caused it to differ in yet another way. When the luminance of the surround was extinguished, the matte target lost its surface-appearance and was perceived as an expanse of yellowish light, while the glossy surface retained its surface-appearance. Thus, in the presence of similar surrounds, the gradients on the tile surface altered both its surface-appearance and lightness when compared to a paper with the same average target-to-surround ratio.

## EXPERIMENT II

Experiment II was designed to study the perceived lightness of the paper and tile surfaces when the contrast-ratio of the target to the surround was kept constant while the illumination on both was varied. The target-surfaces were illuminated directionally.

*Method.* The center partition of the box was removed and the target and surround were set flush with the back wall of the box. The target and surround were simultaneously illuminated by two projectors. The ratios of the surround-luminances to the average luminances of the paper and tile were set at 6:1. Since it was necessary to illuminate the tile more strongly to make its average luminance equal to that of the paper, separate surrounds were prepared for the paper and tile surfaces. In each surround, a circular aperture 2.5 in. in diameter was cut. The *O*s therefore saw a circular target in the presence of a surround. The average luminances of the paper and tile surfaces were set at 0.1, 0.5, 1, 1.5, 2, 2.5, 3, 4, and 5 ft.-L. Different groups of *O*s were used with the paper and with the tile targets, 13 with paper and 15 with tile. In other respects the procedure was similar to that of Experiment I.

*Results.* Fig. 3 presents the mean lightness-judgments for the tile and the paper. The results indicate that the glossy surface tends to exhibit greater constancy than the matte surface, and that, as in Experiment I, for equal contrast-ratios, the tile surface is seen as darker. The mean of the gloss-judgments at the different levels of illumination was 1.9 for the tile surface and 0.9 for the paper surface ($p < 0.002$ by the Sign Test).

The means of the lightness-matches of the matte surface at the different levels of illumination ranged Munsell values of 3.1–4.5. A Friedman analysis of variance rejects the hypothesis that lightness-matches of the matte surface at the different levels of illumination differed only by chance ($p < 0.001$). The means of the lightness-matches of the glossy surface ranged from 2.1–2.5 and did not vary significantly.

The results of Experiments I and II show that when the average contrast-ratio for a target and surround is the same for a matte and a glossy surface illuminated directionally, the glossy surface is seen as darker and maintains greater constancy with increasing illumination than the matte surface. The differences between the tile and paper surfaces are undoubtedly related to the fact that, due to its specular reflection, the tile surface reflects light non-uniformly, while the paper reflects light uniformly. The question which must be considered is how variations in the luminance of a glossy surface affect perceived lightness.

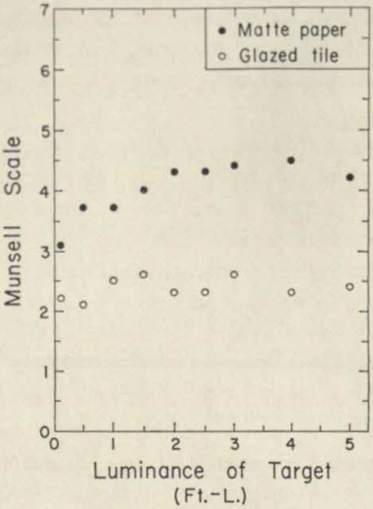

FIG. 3. MEAN LIGHTNESS-JUDGMENTS OF MATTE PAPER AND GLAZED TILE IN EXPERIMENT II

The paper and tile were illuminated directionally, and the points represent judgments at different illuminations.

In Experiments I and II, the perception of gloss is a result of the contrasts in the intensities of light scattered by the tile. Glossiness is not, however, a simple response, but involves the organization of the luminance-distribution in terms of a specific surface-lightness and deviation due to highlights. An important aspect of both Experiments I and II is that the black tile was seen as a shiny black surface and not as a surface of varying shades of gray. This suggests that the lightness-perception of a glossy surface can be analyzed as a two-step process: (1) contrast acts to determine a pattern of lightnesses in accordance with the distribution of luminances reflected and (2) an organization of the lightness-pattern in terms of the character and distribution of the lightness-variations. What is supposed is that a judgment of surface-lightness is arrived at by fixing upon that lightness which is most consistent with the distribution of luminances. Due to the shape, size, edge-gradients, and localization of luminance-variations, these are seen as highlights or reflections rather than as surface-lightness differences.

Differences in the perceived lightnesses of the paper and tile are consequently the result of two factors. The luminance-variations of the tile introduce internal contrasts in addition to the external contrast of the surround. This accounts for the fact that the tile retains its surface-appearance with the surround extinguished. Furthermore, for a given surround, the perceived lightness of the tile is determined, not by the average luminance reflected, but by a particular luminance within the array of luminances reflected. It is hypothesized that the uneven distribution of light reflected by the tile is organized in terms of a given lightness with deviations from this lightness seen as differences in illumination due to highlights and light spots. Experiments III and IV test this explanation.

## EXPERIMENT III

From the foregoing discussion it is clear that the character of the luminance-variations would be expected to play an important role in determining perceived lightness of a glossy surface. The contrasts in the intensities of light reflected by the tile in Experiments I and II could not be easily specified. In Experiment III, the luminance-pattern reflected by the tile was so simplified that its effect could be analyzed. Gloss was revealed by a localized mirror-image of a metallic rod. A photograph of the tile and metallic reflection is shown as *C* in Fig. 1. Both the paper and the tile were diffusely illuminated. Except for the image of the rod, its shadow, and associated edge-gradients, the light reflected from the tile was uniform, and similar to that reflected by the paper. The question posed by this experiment was: would the *Os*' lightness-judgments of the paper and tile be similar when the ratio of the surround-luminances to the paper and tile was equal except for the localized mirror-image which revealed the gloss of the tile?

*Method.* The experimental arrangement was the same as in Experiment I. The interior portions of the box were lined with white cardboard which acted as a large diffuse secondary light-source illuminating the tile and paper uniformly. The gloss of the tile was revealed by the reflection of a metallic rod placed out of the line of sight at the side of the chamber. The average luminances of the paper and tile (without the rod-reflection) were set at 1 ft.-L. The luminance of the surround was set at 0.75, 1.5, 2, 3, 6, 9, 12, and 15 ft.-L. The procedure was similar to that of Experiment I except that the chin-rest was removed, and the *O*s were instructed to move their heads slowly from side to side while viewing the target. At the conclusion of the experiment, the *O*s were questioned on how they made their judgments. The 16 *O*s were undergraduates, naïve regarding the purpose of the experiment, who were paid to participate.

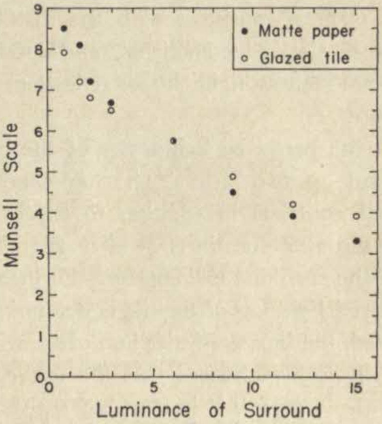

Fig. 4. Mean Lightness-Judgments of Matte Paper and Glazed Tile in Experiment III

The paper and tile were illuminated uniformly, and the points represent judgments at different contrast-ratios.

*Results.* The results indicate that when the ratio of the surround-luminances to the luminances of the paper and tile are equal, except for the mirror-image, lightness-judgments of the paper and tile were similar. As shown in Fig. 4, the tile was judged darker only at the lower surround-luminances, which would be expected as a result of the internal contrast due to the mirror reflection. The comments of the *O*s indicated that the mirror-reflection and associated dark shadow was avoided in making lightness-judgments. The localization, edge-gradients, and changes of the image and shadow with head-movements caused the light reflected by these areas to be seen as deviations due to surface-gloss. The mean of the *O*s' judgments of gloss was 2 for the tile surface and 1 for the paper surface ($p < 0.004$ by the Sign Test). Thus, the gloss-judgments of the paper and tile surfaces were similar to those obtained in Experiment I, while the lightness-judgments were not. This result indicates that differences in the perceived lightness of a matte and glossy surface are not simply the result of the perception of gloss. That is, the *O*s cannot be thought of as evaluating the reflected luminance in terms of whether a surface is matte or glossy and drawing the inference that a glossy surface would have to have a lower albedo than a matte surface. The differences between lightness-judgments of the tile in Experiments I and III lend support, instead, to the hypothesis that the perceived lightness of a glossy surface depends upon the contrasts within and the organization of the array of light reflected by the surface.

## EXPERIMENT IV

The previous discussion suggested that lightness-judgments of the glossy surface in Experiments I and II differed from judgments of the matte surface because of the internal contrast introduced by variations in reflected luminance. Experiment IV was designed to study lightness-judgments of a matte paper when the luminance of the paper was made nonuniform by painting white dots on it. To compare lightness-judgments of the spotted paper with those of the title in Experiment I, the effect of the internal contrast on the tile and paper had to be equated. To this end, the luminance of the surround was set at 0.3 ft.-L., and the size of the dots on the paper was varied until the median of the *O*s' lightness-judgments was equal to the median of their lightness-judgments of the glazed tile in Experiment I. The question posed was: When the matte target is so darkened by internal contrast that it is equal in lightness to the tile at the lowest luminance-level of the surround, will the *O*s' lightness-judgments of both targets be similar with increasing luminance of the surround?

*Method.* The procedure and experimental arrangement were identical with those of Experiment I. The *O*s judged the lightness of a uniform black paper and of a black paper on which white dots 3/10 in. in diameter were equally spaced. The average luminances of the uniform-matte and spotted-matte targets were set at 0.3 ft.-L. With the spotted target, the *O*s were instructed to judge the lightness of the target between the dots. The 17 *O*s were undergraduates, naïve regarding the purpose of the experiment, and were paid to participate.

*Results.* Fig. 5 presents the mean lightness-judgments of the uniform- and spotted-matte targets at the different luminance-levels of the surround.

The lightness-matches of the uniform-matte target were close to the matches obtained for it in Experiment I except when the surround-luminance was set at 6 ft.-L. The lower match obtained in the present study appears closer to what would be expected on the basis of contrast. The reason for the higher value obtained in Experiment I is unclear. The *O*s' lightness-judgments of the spotted target at the different luminance-levels of the surround were similar to their judgments of the glazed tile of Experiment I. Though not conclusive, the similarity of the two curves supports the hypothesis that the darkening of the glazed tile in Experiment I was due to the presence of internal contrast. The tile and matte targets differed, however, in their surface appearance. The tile appeared as a shiny surface of uniform lightness, while the paper appeared more as a matte surface with white spots. The mean gloss-judgments of the tile was 2, and, of the spotted paper, 1.1 ($p < 0.004$ by the Sign Test). The mean of the *O*s' gloss-judgments of the uniform-matte target was 0.74. Comments made by the *O*s indicated that the greater impression of gloss on the spotted target was the result of the glare due to the white spots, but a Sign Test of the difference missed significance ($p = 0.06$).

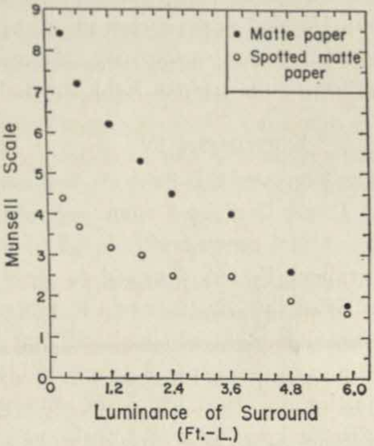

FIG. 5. MEAN LIGHTNESS-JUDGMENTS OF UNIFORM- AND SPOTTED-MATTE PAPER IN EXPERIMENT IV

The points represent judgments at different contrast-ratios.

The results of these experiments suggest, then, that the relationship between perceived lightness and glossiness is a consequence of the processes entering into the perception of lightness when the luminance of a surface is non-uniform. They lend support to the hypothesis that the perception of lightness can be analyzed as a two-step process. If a surface reflects light uniformly, perceived lightness is determined by the relationship of luminances in adjacent regions, *i.e.* contrast; however, when a surface reflects light non-uniformly, lightness-perception involves certain processes of pattern-organization. That is, the pattern of lightness resulting from variations in the luminance reflected by a surface may be seen either as variations in lightness (spotted paper in Experiment IV) or as variations in illumination (highlights on the glazed tile in Experiment I). The organization of a lightness-pattern in terms either of differences in surface-lightness or differences in illumination depends upon the cue-characteristics of the stimulus; that is, upon the information provided by the luminance-variations over the surface, the apparent orientation of the surface, and the perceived direction of illumination.

## SUMMARY

The relation of lightness-perception to the perception of glossiness was studied in four experiments. Experiments I and II show that when the average contrast-ratio for a target and surround is the same for a matte and a glossy surface, the glossy surface is seen as darker and maintains constancy better with increasing illumination. The hypothesis is proposed that the lightness-perception of a glossy surface can be analyzed as a two-step process: (1) contrast acts to determine a pattern of lightness in accordance with the distribution of luminances reflected and (2) as a result of the cue-properties of the stimulus, the lightness-pattern is organized as a single lightness with deviations due to highlights and light-spots. Experiments III and IV lend support to this hypothesis.

# THE EFFECTS OF VARIABLE CONTEXTS ON THE ACQUISITION AND RETENTION OF PAIRED ASSOCIATES

By LEO POSTMAN and LAURA W. PHILLIPS
University of California, Berkeley

The effective stimuli in paired-associate learning are not necessarily limited to the verbal items designated as the specific cues to the responses. Contextual features may serve as additional cues or may, indeed, supersede the nominal verbal stimuli. When there is a one-to-one relationship between the two sets of cues, well-differentiated and familiar contextual features, such as colors, are considerably more effective as stimuli than verbal items of low meaningfulness. Thus, when a compound stimulus consisting of a nonsense-item against a colored background is used in training, performance on a test of retention is higher when the colors alone are used as stimuli than when only the verbal items are presented.[1] Since transfer from compounds to contextual elements may be as high as 100%, there is no reason to assume that configurational learning or summation of habits occurs in acquisition.[2] On the other hand, since both the nominal stimulus and the contextual features usually prove functional on a test of transfer, it is plausible to conclude that independent associations can be developed to each set of cues. With stimuli composed of separate distinctive elements, selective association of the responses to the most effective components of the compounds become possible. Thus, compound stimuli may produce faster learning and better recall than either the nominal or the contextual cues by themselves.[3] The same analysis should apply to situations in which the nominal verbal stimulus is presented in the context of other verbal items. In Pan's classical study, contextual verbal cues "logically

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[1] Walter Weiss and Garry Margolius, The effect of context stimuli on learning and retention. *J. exp. Psychol.*, 48, 1954, 318-322.

[2] B. J. Underwood, Stimulus selection in verbal learning, in C. N. Cofer and B. S. Musgrave (eds.) *Verbal Behavior and Learning: Problems and Processes*, 1963, 33-48. In this paper, Underwood introduces the distinction between the nominal and functional stimulus in verbal learning, *i.e.* the stimulus manipulated and presented by *E*, on the one hand, and the stimulus selected as a cue by *S*, on the other.

[3] F. A. Hill and D. D. Wickens, The effect of stimulus compounding in paired-associate learning, *J. verb. Learng. and verb. Behav.*, 1, 1962, in press.

related" to the *responses* facilitated learning, and the removal of such cues reduced the amount recalled. On the other hand, contextual items related to the *stimuli* had a detrimental effect on learning and did not influence recall.[4] Contextual items facilitate learning and recall only to the extent that they acquire independent associative strength to the prescribed responses.

The introduction of contextual features not consistenly related to the nominal stimuli should be conducive to intralist interference. For example, if the contextual features surrounding a set of nominal stimuli are interchanged on successive trials, the functional similarity of the stimulus-terms should be increased and the associative stage of learning retarded. The higher the probability that the responses become associated with the contextual items, the greater should be the intra-list interference. Thus, an inverse relationship is predicted between the meaningfulness of variable contextual features and speed of learning. The effects of a variable context on recall are less readily predictable. On the assumption that errors of generalization elicited by contextual features recover over time, recall should be inversely related to the meaningfulness of variable contextual features during acquisition. It is a fact, however, that the hypothesis of differential recovery of generalization has not been supported in experimental studies of the effects of intra-list similarity, *i.e.* the expected inverse relationship between intra-list similarity and recall has not been found.[5] Thus, the available evidence runs counter to the prediction of an inverse relationship between the meaningfulness of variable contextual features and recall. There are some reasons, on the other hand, to expect a positive relationship. The higher the meaningfulness of the contextual items, the more likely it is that some associative strength will develop between each of them and the prescribed responses in the list. These converging associations should increase the availability of the responses and thus enhance the amount recalled, provided this advantage is not offset by differential recovery of errors of generalization. The present study compares the effects of variable contexts of high and low meaningfulness on the acquisition and retention of lists of paired associates. For purposes of this experiment, meaningfulness was defined in terms of the frequency of usage of English words which correlates highly with scaled measures of meaningfulness.[6]

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[4] Shuh Pan, The influence of context upon learning and recall, *J. exp. Psychol.*, 9, 1926, 468-491.

[5] Underwood, An evaluation of the Gibson theory of verbal learning, in C. N. Cofer (ed.), *Verbal Learning and Verbal Behavior*, 1961, 204 f.

[6] B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning*, 1960, 44-64; Leo Postman, Extra-experimental interference and the retention of words, *J. exp. Psychol.*, 61, 1961, 99.

## METHOD

*Materials.* The learning materials consisted of three sets of 20 two-syllable nouns. One set consisted of words of high frequency of usage classified as A or AA in the General Count of Thorndike and Lorge,[7] the second set of words of low frequency (median frequency in the General Count 2.5; range 1-6), and the third set of words of intermediate frequency (median frequency 22; range 10-39). The words of intermediate frequency were used to construct lists of 10 paired associates which constituted the learning task for all *S*s. The words in the other two lists were used as contextual items of high frequency (HC) and low frequency (LC), respectively. Thus there were two arrangements of the materials—Lists HC and LC—which included the same basic lists of paired associates but contextual items of different word-frequency. The materials were chosen so as to minimize preëxperimental associations among the members of a set, and between members of the paired-associate list and the contextual items. Norms of free association were used for this purpose.[8] The frequency with which relevant words elicit each other as associates in the norms is zero in the vast majority of cases and in no case exceeds 0.1%.

In a given list, the 20 contextual items were grouped into 10 sub-sets of two, and one such sub-set was presented with each of the stimulus-members of the paired associates. The paired associates were presented in six different random orders, and the contextual items accompanying the stimulus-members were interchanged for each order of presentation. No sub-set of contextual items was assigned to the same stimulus-members more than once in the cycle of six orders of presentation. Hence, each sub-set was presented with six different stimulus-members.

Two different pairings of the stimuli and responses were used in the construction of the list of paired associates, and there were also two different groupings of the contextual items into sub-sets. The resulting four combinations of materials at each level of frequency were used equally often.

*Procedure.* The list of paired associates was presented on a memory-drum at a 4:2 rate, *i.e.* stimulus alone for 4 sec., and stimulus and response together for 2 sec. The stimulus-member and the two contextual items were arranged vertically one above the other and hence were always presented simultaneously. The spatial position of the stimulus-member varied from unit to unit within a given trial, and from trial to trial for a given pair. The response-member was always presented in the position directly opposite to the stimulus-member. The *S* was required to read aloud the three words on the left-hand side (the stimulus and the two contextual items) and to call out the response during the 4-sec. period of anticipation. The interval between successive trials was 6 sec. In the instructions, the *S* was given full information about the occurrence of variable contextual items and about the spatial relation between the appropriate stimulus and the response. Learning was carried to a criterion of 8/10.

Retention was tested in five trials of relearning either 30 sec. or 7 days after the end of original learning. The tests of retention always started with the order of presentation following the last one used in original learning.

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[7] E. L. Thorndike and Irving Lorge, *The Teacher's Wordbook of 30,000 Words*, 1944.

[8] The norms for words of different frequency of usage are based on the responses of 1000 students at the University of California. These norms were supplemented by those of W. A. Russell and J. J. Jenkins, *The Complete Minnesota Norms for Responses to 100 Words from the Kent-Rosanoff Word Association Test*, 1954.

*Subjects.* With two types of context and two retention-intervals, there were four groups of 24 *Ss*. The *Ss* were undergraduate students whose native language was English. They were not necessarily naïve to experiments on verbal learning. The *Ss* were assigned to conditions in blocks of four, with one *S* per block from each of the four experimental groups. The running order within blocks was determined by means of a table of random numbers, as was the assignment to specific lists and running orders. No *Ss* were lost because of failure to learn.

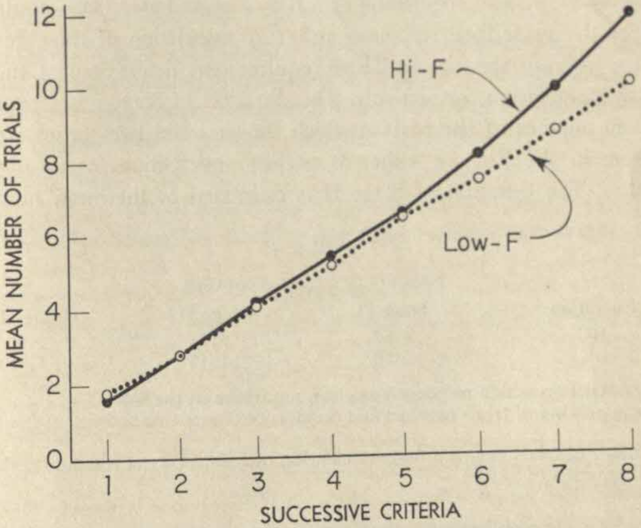

FIG. 1. MEAN NUMBERS OF TRIALS TO SUCCESSIVE CRITERIA UNDER THE TWO CONDITIONS OF LEARNING

## RESULTS

*Learning to criterion.* The mean number of trials to a criterion of 8/10 was 12.02 ($SD = 4.92$) for Condition HC and 10.14 ($SD = 6.43$) for Condition LC.[9] The difference in speed of learning is significant. Following a log transformation (in view of the skewness of the distributions) $t$ is 2.24 ($df = 94$, $0.01 < p < 0.02$). Fig. 1 shows the number of trials required to reach successive criteria of learning. There is no difference between the conditions in the early stages of acquisition. The rates of learning do not diverge until the higher criteria of performance—five or more correct responses—are reached. The late occurrence of the separation between the groups suggests that the disadvantage of Condition HC reflects the cumulative effects of intraserial interference. The results of a stage-analysis of the period of acquisition are consistent with this interpretation.

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[9] Under a given condition, there were no significant differences in original learning between the groups tested after 30 sec. and after 7 days. The results of the two groups were, therefore, combined.

*Stage-analysis.* The acquisition of a list of paired associates requires (1) recall of the prescribed responses and (2) association of these responses with the appropriate stimuli. These requirements define Stage I and Stage II of the acquisition, respectively. The duration of Stage I is estimated by the mean number of the trial on which the prescribed responses are given anywhere in the list, *i.e.* either as correct anticipations or as misplaced responses. The duration of Stage II is estimated by the mean number of trials which elapses between the first occurrence of a response and its correct placement in the list.[10] As Table I shows, the durations of both Stage I and Stage II are comparable for Conditions HC and LC, and for neither measure does the difference between the groups approach statistical significance. The conditions are, however, clearly separated during the third stage of acquisition which may be designated as the period of oscillation. The delimitation of this stage is based on the following considerations. Suppose that the criterion of learning is *n* correct responses on a single trial. The earliest point at which this criterion can be attained is when each of the *n* items has been anticipated correctly once, *i.e.* upon the completion of Stages I and II for all items. The attainment of the criterion does not, however, usually coincide with the first correct anticipation of the *n*th item. Instead, a period of oscillation typically intervenes until all *n* correct responses are given on the same trial. The duration of Stage III

TABLE I

RESULTS OF STAGE-ANALYSES

| Condition | Stage I* | Stage II† | Stage III‡ |
|---|---|---|---|
| HC | 4.82 | .92 | 2.83 |
| LC | 4.30 | 1.15 | 1.88 |

* Mean trial on which response was given anywhere in the list.

† Mean number of trials between first occurrence of response and first correct antici-tion.

‡ Number of trials between first anticipation of eighth correct response and attainment of criterion of 8/10.

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[10] For a discussion of these two stages of associative learning, see Underwood and Schulz, *op. cit.*, 92-94.

was estimated, therefore, by the number of trials which elapsed between the point at which each of eight items had been anticipated correctly at least once and the attainment of the criterion (Table I). This period of oscillation is significantly longer for Condition HC than for Condition LC ($t = 2.06$, $df = 94$, $0.02 < p < 0.05$ following a Freeman-Tukey square-root transformation). Thus, Stage III appears to be the locus of the observed difference between the conditions in speed of learning to criterion.

*Errors during learning.* Table II presents the mean percentages of misplaced responses given during the acquisition of the lists. The percentages are based on opportunities for occurrence of such errors (total number of presentations minus number of correct responses). Percentages of anticipatory and perseverative errors are indicated separately. The rates of both types of misplaced responses are quite similar for the two conditions, and there are no significant differences between the groups. The relatively high frequency of perseverative errors is worthy of note. Such errors account on the average for 39.3% and 41.7% of the misplaced responses under Conditions HC and LC, respectively. The presence of variable contextual items reduces the discriminability of individual pairs and hence short-term memory for the identity of specific units presented on a given trial. Under these circumstances *S*s frequently fail to reject intruding responses which had been correct earlier during a trial.

TABLE II

MEAN PERCENTAGES OF MISPLACED RESPONSES DURING ORIGINAL LEARNING

| Condition | Anticipatory | Perseverative | Total |
|---|---|---|---|
| HC | 10.5 | 6.8 | 17.3 |
| LC | 11.2 | 8.0 | 19.2 |

While the average rates of learning and of misplaced responses do not vary together, these two measures are substantially correlated for individuals within the groups. The product-moment correlation between the percentage of misplaced responses and the number of trials to criterion is 0.56 for Condition HC, and 0.50 for Condition LC. Both coefficients are significant beyond the 1% level. It appears that overt errors are eliminated less readily than failures to respond. Thus, the individual *S*'s proneness toward errors of generalization is significantly related to speed of learning.[11]

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[11] This pattern of correlations corresponds to that found in a study of paired-associate learning as a function of the meaningfulness of the stimuli and responses. See Postman, The effects of language habits on the acquisition and retention of verbal associations, *J. exp. Psychol.*, 64, 1962, 7-19.

*Recall.* The mean numbers of responses recalled after 30 sec. and 7 days are shown in Table III. The amount recalled is higher under Condition HC than LC on the immediate as well as the delayed test. Absence of the usual inverse relationship between speed of learning and amount of post-criterial drop suggests a difference in the rate of forgetting immediately after the end of learning. To assess the amounts of forgetting on both tests, successive-probability analyses were carried out on the results of original learning.[12] On the basis of these analyses, an expected recall-score was determined for each *S*. The difference between the expected and observed score provides a measure of forgetting which takes account of associative strength at criterion. The mean loss-scores are presented in Table III. After 30 sec., the loss for Condition LC is very close to the post-criterial drop predicted on the basis of the probability-analysis. On the other hand, the loss for Condition HC clearly falls below the predicted amount. A positive factor appears to have come into play after the end of practice which counteracted the expected loss in retention. After 7 days, both groups show considerable amounts of forgetting, but the amount of loss is again greater for Condition LC. Analysis of variance shows the difference between Conditions to be significant ($F = 4.49$, $df = 1/92$, $0.02 < p < 0.05$). With Condition HC superior after both intervals, the interaction of Tests with Conditions is not significant ($F < 1$)[13].

TABLE III

AMOUNTS RECALLED AFTER 30 SEC. AND 7 DAYS

| Condition | 30-sec. recall | | | 7-day recall | | |
|---|---|---|---|---|---|---|
| | Mean | *SD* | Loss* | Mean | *SD* | Loss* |
| HC | 7.79 | 1.54 | −.60 | 4.62 | 2.28 | 2.68 |
| LC | 7.38 | 0.91 | −.10 | 3.75 | 1.74 | 3.65 |

* Difference between score expected on the basis of probability-analysis and obtained score.

*Retention as a function of performance during original learning.* An increase in the meaningfulness of variable contextual items has opposite effects on acquisition and retention: it reduces speed of learning but favors recall. As was noted earlier, converging associations between contextual items and responses may be responsible for both effects. In acquisition such associations are conducive to intra-list interference. Once the appropriate stimuli have been differentiated, however, converging associations serve to 'prime' the prescribed responses at recall. If this analysis is correct, amount of recall should be positively related to the number of converging associations, and hence to degree of intra-list interference during acquisition. This relationship should be more pronounced under Condition HC than LC. The duration of Stage III, which reflects the amount of oscillation during approach to criterion, was used as a measure of *S*s' susceptibility to interference during acquisition. The product-moment correlation between the duration of Stage III and amount recalled after 7 days is 0.42 ($p < 0.05$) for Condition HC, and 0.07 for Condition LC. The correlation between the number of trials to criterion and recall is near zero under both conditions, so that the relationship between Stage III and recall is not reducible to an effect of the sheer number of trials in original learning. The correlation between the percentage of misplaced responses in original learning and amount recalled is 0.29 for Condition HC and 0.01 for LC. Although neither coefficient is significant, the difference between conditions is congruent with that found for the duration of Stage III. By contrast, the correlation between the percentage of errors and speed of learning was significantly negative. When both indices of interference—duration of Stage III and percentage of misplaced responses—are considered together, the multiple correlation with amount recalled is 0.51 ($p < 0.05$) for Condition HC, and 0.07 for Condition LC.

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[12] For a description of this method, see Underwood, Speed of learning and amount retained: A consideration of methodology, *Psychol. Bull.*, 51, 1954, 276-282.

[13] The results of this analysis must be interpreted with caution. The correlations between the predicted and obtained recall-scores of individuals within groups are low (the average $r$ is 0.20), probably because the expected scores of fast *Ss* are underestimated and those of slow *Ss* are overestimated. It may be assumed, however, that the mean predictions are accurate and that the errors in the estimation of individual scores are comparable under the two conditions.

*Errors at recall.* The total numbers of misplaced responses on the 30-sec. test of recall were 16 for Condition LC, and 11 for Condition HC. The corresponding numbers on the 7-day test were 20 and 13. As in original learning, the frequencies of misplaced responses are similar for the two conditions. The changes over time are minor and not significant.

*Relearning.* Performance under the two conditions converged during the five trials of relearning. The median numbers of trials required for relearning to the original criterion of 8/10 after 30 sec. were 1.6 and 1.5 under Conditions HC and LC, respectively. After 7 days the median number of trials for both groups was 3.1.[14] On the immediate test the mean number of correct responses given in five trials was 42.92 for Condition

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[14] Since a few *S*s failed to reach a criterion of 8/10 during relearning, medians rather than means are reported.

HC and 41.59 for Condition LC. The corresponding means on the delayed test were 36.21 and 35.38. A convergence of the groups in relearning was to be expected since recall was higher for Condition HC whereas speed of learning had been found to be greater for Condition LC.

## DISCUSSION

This experiment helps to bring out some of the continuities between the operations and processes of rote-learning and concept-learning.[15] In both types of experimental situations, *S* must learn to respond to relevant features of successive stimuli and to disregard irrelevant variations in the contexts of the critical cues. Changes in environmental and internal conditions are likely to produce some contextual variation from trial to trial even when the manipulated conditions of learning are held constant. Normally, however, the nature and magnitude of such changes are not known. In the present study, the potential influence of variable contexts is maximized by the use of verbal items as both relevant and irrelevant stimuli.

The results of the experiment show that speed of learning is significantly influenced by the meaningfulness of variable contextual items. The higher the meaningfulness the more likely it is that associations will be formed between the irrelevant stimuli and the prescribed responses. Such irrelevant associations compete with responses to the relevant cues for which *S* is consistently reinforced. In agreement with this interpretation, criterion was reached more slowly under Condition HC than LC, and this difference appears to reflect the amount of oscillation in the later stages of acquisition. The rate of misplaced responses does not, however, increase as a function of the meaningfulness of the contextual items. The failure of mean trials to criterion and mean rate of intra-list errors to covary has also been observed in a number of experiments in which the similarity of the stimuli in paired-associate learning was manipulated.[16] In the present case it is, moreover, likely that the use of a relatively long interval of anticipation (4 sec.) permitted *Ss* to achieve some success in discriminating between responses elicited by appropriate and inappropriate stimuli and in rejecting the latter. Such differentiation and rejection of errors was probably favored by the distinctiveness of highly meaningful contextual stimuli and may have served to equalize the rates of intra-list errors under the two conditions.

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[15] The similarities between concept-learning and rote-learning are discussed by G. A. Miller, Free recall of redundant strings of letters, *J. exp. Psychol.*, 56, 1958, 485-491.

[16] Underwood, *op. cit.*, 1961, 203.

In contrast to the effects on acquisition, meaningfulness of the contextual items favors retention. We have suggested that the higher the meaningfulness of the contextual items, the more likely it is that converging associations between these items and the prescribed responses will facilitate recall. To speak of converging associations, *i.e.* connections between several contextual stimuli and a response-term, does not necessarily imply a summation of habit-strengths. It is sufficient to assume that the probability of a response being recalled and available to *S* is a function of the number of stimuli which can severally elicit it. It is clear that converging associations can facilitate performance on the test of recall only to the extent that *S* can discriminate between appropriate and inappropriate stimuli once the responses are available to him. The fact that retention on the immediate test exceeds the expected value suggests that converging associations begin to enhance recall as soon as their effectiveness as sources of interference has been reduced sufficiently to permit attainment of the criterion. There is no evidence in the present data for recovery of errors of generalization over time: the frequency of misplaced responses at recall shows only small and insignificant changes between the 30-sec. and the 7-day test. Moreover, performance on the delayed test of recall appears to be positively related to *S*'s susceptibility to intra-list interference during acquisition.

The results of this experiment leave open the question of whether the introduction of the contextual items facilitated or interfered with learning and recall in an absolute sense, *i.e.* as compared with a condition in which no contextual items are used.[17] Our conclusions are necessarily limited to the differences between items of high and low meaningfulness. The present findings are also specific to a situation in which the relationship between contextual items and appropriate stimuli varies randomly from trial to trial. When the relationship between the two classes of stimuli is consistent, associations between contextual stimuli and responses are necessarily correct and hence can facilitate learning but cannot become sources of interference.

## SUMMARY

This experiment compares the effects of variable contexts of high and low meaningfulness on the acquisition and retention of paired associates.

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[17] Such a comparison presents some methodological problems. It would be necessary to equate the duration of exposure to the relevant stimuli, but this could be done without changing the rate of presentation of the pairs.

The learning task consisted of a list of 10 paired associates composed of words of medium frequency of usage. The stimulus-members were presented in the context of two irrelevant items of either high or low frequency of usage. The irrelevant stimuli accompanying each revelant stimulus varied from trial to trial. Learning to a criterion of 8/10 was faster when the meaningfulness (as defined by word-frequency) of the contextual items was low than when it was high. This difference appears to reflect the amount of interference from associations between irrelevant stimuli and prescribed responses under the two conditions. Retention both 30 sec. after the end of original learning and after 7 days varied directly with the meaningfulness of the contextual items. The beneficial effects of meaningfulness are attributed to increased availability of the responses as a result of multiple associations with the contextual stimuli. There was no evidence for differential recovery of errors of generalization as a function of the meaningfulness of the contextual stimuli.

# THE LATENCY OF THE CONDITIONED FEAR-RESPONSE

By R. A. Champion, University of Sydney

The concept of emotional activity figures prominently in both abnormal psychology and in the theory of behavior, and appears to provide a promising avenue by which the experimental psychologist might contribute to the understanding of clinical problems. The possibility of a link of this kind may be increased by the clearer specification of and distinction between two types of emotional activity, in the form of anxiety or emotionality, on the one hand, and the hypothetical fear-response, on the other hand. Both of these types of emotional activity are said to be derivable from painful or noxious stimulation, but the former may best be regarded as a persisting state of the organism, while the latter is thought of as a specific response.

Spence, for example, has offered a theory of emotionally-based drive which is studied through the manipulation of airpuffs administered between conditioning trials or the selection of high and low scorers on a self-inventory test of manifest anxiety,[1] but it seems that he is dealing with an hypothetical 'state' variable, even though he describes it as an emotional response and uses the designation $r_e$. Miller, on the other hand, represents a group of workers who have given rather more attention to the specific, discrete response of fear, originating as a component or concomitant of pain, but learnable in the sense that it is capable of being elicited by some neutral stimulus consistently presented with the painful stimulus.[2] While the distinction between emotionality and fear in these terms may be important, the two processes should still be conceived of as having common properties and as being in no way opposed. Motivational properties are attributed to fear, and the fear-response may be thought of as occurring with such frequency as to equal emotionality in persistence. The distinction must be made, however, for it is only the fear-response which has been shown to be learnable and therefore directly dependent upon environmental cues rather than merely building up with successive noxious stimulations.[3]

Detailed consideration of the processes involved in the acquisition and extinction of the hypothetical fear-response,[4] a necessary prelude to any clinical application,

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* Received for publication January 26, 1962.

[1] K. W. Spence, A theory of emotionally based drive ($D$) and its relation to performance in simple learning situations, *Amer. Psychologist*, 13, 1958, 131-141.

[2] N. E. Miller, Learnable drives and rewards. In S. S. Stevens, (ed.), *Handbook of Experimental Psychology*, 1951, 435-454.

[3] J. S. Brown, H. I. Kalish, and I. E. Farber, Conditioned fear as revealed by magnitude of startle response to an auditory stimulus, *J. exp. Psychol.*, 41, 1951, 317-328.

[4] R. A. Champion, The acquisition and extinction of the fear response, *Austral. J. Psychol.*, 13, 1961, 23-38.

leads to the latency of the response as a crucial feature, and the following experiment was designed to yield further information about this property of fear. The relevance of this information is readily illustrated in the rationale of Mowrer's recent elaboration of his 'two-factor' theory of behavior.[5] Mowrer argues that the conditioned occurrence of not only fear but also the cessation of fear (hope) acts to govern overt, instrumental behavior. There is reason to suppose, however, that the fear-response is mediated in part at least by the action of the relatively slow autonomic nervous system, so that its latency may well be longer than that of the responses which Mowrer assumes it to govern.

The most fruitful procedure for determining the latency of the conditioned fear-response appears to be the probe-stimulus (*PS*) technique devised by Brown and his associates.[6] By this means Spence and Runquist produced evidence that fear has a latency greater than 0.5 sec. and possibly of the order of 4.5 sec.;[7] Ross interpreted data gathered in a similar way to mean that the latency of the response is longer than 0.5 sec. and may exceed 2 sec.[8] A common feature of these two experiments, however, was the use of a reflex (eyeblink) as the index of conditioned fear, whereas the theorizing of Mowrer, for example, is more concerned with the effects of fear on instrumental responses. It was thought, therefore, that the available evidence should be confirmed under these more general conditions. Unfortunately, any move to study the wider effects of fear leads to a further problem in the ambiguity of the expected direction of the effect. Those who have used the PS technique hitherto have assumed that, having motivational properties, fear activates any concomitant response which clearly dominates all other competing responses. Mowrer, on the other hand, works with the assumption that one main effect of fear is to inhibit other responses,[9] and Mowrer and Aiken, for example, have used decrement in feeding rate in rats as an index of the strength of fear.[10] In addition to Mowrer's distinction between active and passive avoidance learning,[11] the apparent incompatibility of the opposing effects of fear might be resolved with the idea that weak to mild fear has a dynamogenic effect on a dominant contemporary response, whereas the stimulus-properties of strong fear arouse responses which interfere with and dominate all other contemporary responses. It is also possible that, although fear activates the dominant contemporary response, this is not the one that *E* is studying, so that the test-response is weakened by a more direct form of competition. In the absence of prior knowledge about the strength of fear in absolute terms, and of the exact dynamics of the test-situation, it was not clear at the outset of the present study whether the index of fear would be provided by the enhancement of or interference with the test-response.

The fear-response was conditioned in this experiment by consistently presenting a tone (*CS*) in conjunction with shock (*UCS*). Late in training the shock was replaced on some trials by a probe-stimulus (*PS*) in the form of a light to which *S* had been instructed to give a key-pressing response, and the theoretical effect of fear was measured in the reaction-time (*RT*, or latency) of this test-response to the light-signal. To locate the peak of strength of fear as a *CR*, the *PS* was inserted at varying time-intervals after the *CS*. A particular methodological problem concerned the advisability of using a control-group of *S*s to whom the *CS* and *UCS* would not be presented in conjunction, as a check on the conditioning of fear. In the other experiments cited, UCS–CS trials were run for this purpose (backward conditioning), but recent evidence of true backward conditioning of the long-latency *GSR* suggests that such a control may be reduced in effectiveness if the fear-response also proves to have a relatively long latency.[12] It was decided, therefore, that no such control would be exercised, in the expectation of no disadvantage if previous findings were confirmed.

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[5] O. H. Mowrer, *Learning Theory and Behavior*, 1960, 212-252.

[6] Brown, Kalish, and Farber, *op cit.*, 317-328.

[7] K. W. Spence and W. N. Runquist, Temporal effects of conditioned fear on the eyelid reflex, *J. exp. Psychol.*, 55, 1958, 613-616.

[8] L. E. Ross, Conditioned fear as a function of CS-UCS and probe stimulus intervals, *J. exp. Psychol.*, 61, 1961, 265-273.

[9] Mowrer, *op. cit.*, 254-257.

[10] O. H. Mowrer and E. G. Aiken, Contiguity vs. drive-reduction in conditioned fear: Temporal variations in conditioned and unconditioned stimulus, this JOURNAL, 67, 1954, 26-38.

[11] Mowrer, *op. cit.*, 28.

## METHOD

*Subjects.* The *S*s were 45 volunteers from a course in introductory psychology. The results for one *S* were discarded after the completion of the experiment when a check showed that he had already served in a similar experiment.

*Apparatus.* The *CS* was a 2000-cps, sine-wave tone of 60 db. spl and 8 sec. duration delivered to *S* through headphones, and the *UCS* was a 90-cps, 0.5-sec., spike-wave shock of 2.27 ma. (as measured with a 15,000-ohm resistor in place of *S*) with a peak-to- peak voltage of 34 v. The *UCS* was administered through thimble-type electrodes of German silver, ½ in. in length, attached to the index and middle fingers of the right hand. The interval between the onset of *CS* and *UCS* was held constant at 0.5 sec. The *PS* was a 0.5-sec. flash of a 6-v. DC lamp placed near *S*'s right hand, and *S* was required to respond to the *PS* with the right hand by pressing a telegraph key on which was mounted a 4 × 2-in. wooden platform as a hand-rest. An electronic chronoscope was started with the onset of the *PS* and stopped by pressure on the key so as to measure the *RT*.

*Procedure. S* was placed in a sound-reduced room separate from that containing *E* and the main apparatus, and all measures were taken in one session of about 45-min. duration. The session began with 5 practice trials of the key-pressing response to the *PS* and one presentation of the *UCS* alone. Then followed 6 presentations of the *CS–PS* sequence as a fore-test; the *CS* preceded by the *PS* by each of the 6 intervals to be used later in locating the fear response, viz. 0.5, 1.5, 2.5, 3.5, 4.5, and 5.5 sec. These intervals were used in a different predetermined random order for each *S*, with the restriction that no interval occurred on any ordinal trial on more than 10 or less than 6 occasions over the group of *S*s as a whole. It should be noted that the *CS* of 8 sec. overlapped the *PS* by at least 2 sec.

The second stage in the procedure consisted of 9 *CS–UCS* trials to obtain a conditioned fear-response; to prevent 'surprise' reactions on *S*'s part in later tests, one *CS–PS* trial was inserted after the second and fifth *CS–UCS* trials, the *CS–PS* interval being varied among *S*s. The main measures were taken in the third stage of the procedure which took the form of an after-test. Training trials (*CS–UCS*) were continued, but were interspersed with *CS–PS* trials as tests of the strength of fear. A further 6 *CS–PS* trials were used, at each of the intervals listed above and in a predetermined random order subject to the same restriction as in the fore-test, with the further proviso that no *S* receive the same random order in the after-test as in the fore-test. Test trials (*CS–PS*) were run after training Trials 9, 10, or 11, 12 or 13, 14 or 15, 16 or 17, and 18 or 19. The exact points at which *CS–PS* trials were inserted were predetermined at random within these limits with the restriction that such trials be not separated by 1, 2, or 3 *CS–UCS* trials more than twice in the sequence for any one *S*. Throughout the session as a whole trials were separated by 45, 50, or 55 sec. in prearranged random order. The *CS–PS* trials were preceded by a verbal 'ready' signal given by *E* over an intercom approximately 4 sec. before the onset of the *CS*, but no warning signal of any kind was given on *CS–UCS* trials.

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[12] R. A. Champion and J. E. Jones, Forward, backward, and pseudo-conditioning of the GSR, *J. exp. Psychol.,* 62, 1961, 58-61.

## RESULTS

In keeping with the theoretical considerations outlined above, the effect of the conditioning procedure on the test-response showed some inconsistency. Of the 264 *CS–PS* trials run in the after test there was a decrement in performance relative to the fore-test measure in 62 cases, but these were scattered over *S*s and *CS–PS* intervals. For this reason, and because of the presence of some extreme scores in the data, the measures on the fore- and after-tests were first analyzed separately and by means of nonparametric tests.

The fore- and after-test data are represented in Fig. 1. It will be recalled that the *CS–PS* trials consisted of a 'ready' signal, followed 4 sec. later by the onset of the *CS*, followed in turn by the *PS* at intervals varying from 0.5 sec. to 5.5 sec. in 1.0-sec. steps. The *RT* of the key-pressing response to the *PS* in the fore-test did not vary with the *CS–PS* interval to a significant degree; a Friedman analysis of variance gave a $X^2$ value of 0.09 ($p > 0.99$ for 5$df$). In the after-test, however, following a number of *CS–UCS* trials, signficant differences emerged ($X^2 = 12.13$, $p < 0.05$ for 5 $df$). Comparing the results on the various *CS–PS* intervals in the after-test by means of rank tests, there were significant differences between 0.5 and 2.5 sec. ($Z = 2.58$, $p < 0.01$), 0.5 and 4.5 sec. ($Z = 2.64$, $p < 0.01$), and 2.5 and 5.5 sec. ($Z = 2.57$, $p < 0.05$). One other comparison, that between 4.5 and 5.5 sec., approached significance ($Z = 1.86$, $p = 0.06$). Assuming that the main effect of fear in this experiment was to reduce the *RT* of the test-response, the latency of the fear-response appears to be greater than 0.5 sec. but less than 5.5 sec.

A feature of the results of the after-test was the performance of the test-response with a *CS–PS* interval of 3.5 sec., which deviated from the trend of the data as a whole. This result could emerge if the conditioned fear-response was at its peak about 3.5 sec. after the onset of the *CS*, and if the stimulus-properties of this relatively strong fear-response were to elicit a number of other responses which competed with the test-response so as to reduce its performance. To test this possibility, further analyses were made of the data of those *Ss* who showed some consistency in their reaction to the conditioning of fear. When the data of the fore- and after-test for each *S* at each *CS–PS* interval were represented in the form of a decrease or increase in the *RT* (gain or loss in performance, respectively) as a result of conditioning, there proved to be 14 *Ss* whose *RT* decreased at all 6 *CS–PS* intervals ('gainers') and 9 *Ss* whose *RT* increased at 3 or 4 intervals. ('losers'). No *S* showed an increase in *RT* at as many as 5 or 6 intervals. If it be allowed that the test-response of key-pressing was activated by fear in the group of *Ss* showing gains in performance, then their peak performance should have occurred at about 3.5 sec., if such is the latency of the conditioned fear-response. If the other group of *Ss* showed a high

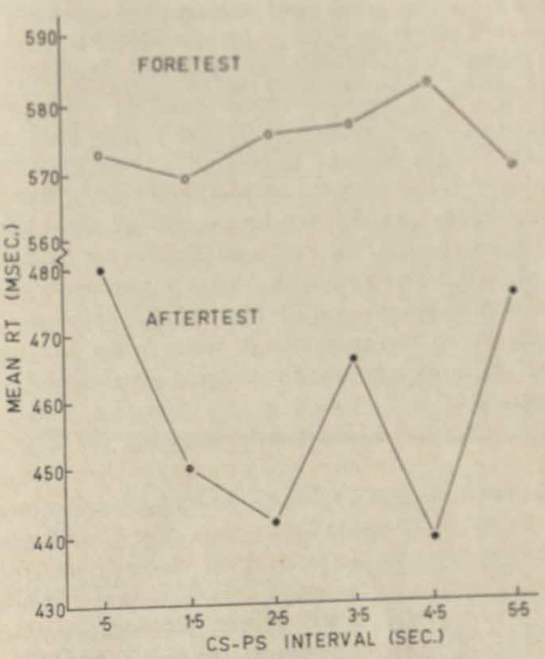

FIG. 1. PERFORMANCE AS A FUNCTION OF *CS–PS* INTERVAL ON THE FORE- AND AFTER-TESTS

frequency of loss in performance because the test-response of key-pressing was not dominant and was interfered with (or inhibited) by some other response, then their poorest performance should have occurred at about 3.5 sec. by the same token. The obtained results of these two groups of selected *S*s are depicted in Fig. 2; their form is in keeping both with the general theoretical expectations about the dynamics of the fear-response and with the hypothesis that the fear-response has a latency of the order of 3.5 sec. The data were combined for statistical tests ($N = 23$), allowing for the difference in the expected direction of the effect with 'gainers' and 'losers.' Applications of rank-tests showed significant differences between change in performance at *CS–PS* intervals of 0.5 and 3.5 sec. ($T = 64.5$, $p < 0.05$), and 3.5 and 5.5 sec. ($T = 59.5$, $p < 0.05$). As a final check, the same test was applied to all other *S*s who showed two gains or two losses at the two *CS–PS* intervals in question; for 0.5

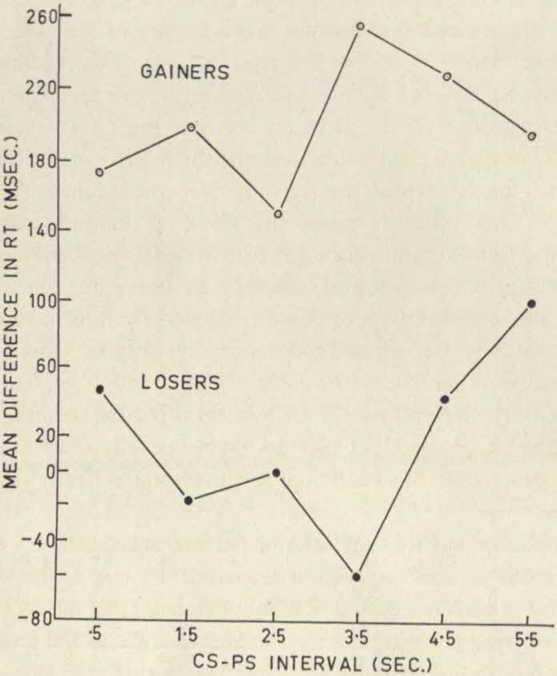

FIG. 2. MEAN DIFFERENCE IN *RT* FOR *S*s SHOWING GAINS AND LOSSES AT VARIOUS *CS–PS* INTERVALS
(*N* for 'Gainers' = 14; for 'Losers' = 9)

sec. vs. 3.5 sec. ($N = 8$) there was a signficant difference of the form predicted ($T = 5$, $p < 0.05$ for a one-tailed test), but there was no significant difference under these conditions between change in performance at 3.5 sec. and 5.5 sec.

## DISCUSSION

The general conclusion to be drawn from the data of this experiment is that the conditioned fear-response has a latency of the order of 3.5 sec., being greater than 0.5 sec. but less than 5.5 sec. This conclusion is compatible with the data of Spence and Runquist, and the significant difference in performance on the after-test between the *CS–PS* intervals of 0.5 and 4.5 sec. in the present study confirms their main result.[13] The finding of Ross that the latency of the response was greater than 0.5 sec. is also confirmed.[14] This latter *E* varied the *CS–UCS* interval from group to group during training and concluded that little if any conditioning of fear occurred with a 0.5-sec. interval, although he noted that Spence and Runquist used this interval. The evidence presented by Ross in support of the conclusion that the latency of conditioned fear may be longer than 2 sec. was obtained only with *CS–UCS* intervals of 2 and 5 sec. These observations suggest that the optimal *CS–UCS* interval for the conditioning of fear remains to be established, but they do not necessarily affect the conclusion that, once generated, the conditioned fear-response has a latency greater than 0.5 sec.

It may be supposed that the inferior performance at the 0.5 and 5.5-sec. *CS–PS* intervals in this experiment is an artifact due to the technique of testing each *S* at each interval. It will be appreciated that the *CS* (tone) acted as a form of 'ready' signal for the reaction to light as the probe-stimulus, and in the terminology of the *RT* experiments this meant irregular variation in the duration of the foreperiod. Under conditions of this type, Woodrow found that well-practiced *S*s tended to give their shortest average *RT* with warning intervals of intermediate length in any given series of intervals, instead of with an interval of 2 sec. as when the duration of the foreperiod was held constant;[15] thus the 2.5, 3.5, and 4.5-sec. intervals of this experiment may have been favored. At least two considerations, however, seem to rule out this possibility. First, the group of *S*s showing a high frequency of decrement in performance from fore- to after-test

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[13] Spence and Runquist, *op. cit.*, 613-616.

[14] Ross, *op. cit.*, 265-273.

[15] Herbert Woodrow, The measurement of attention, *Psychol. Monogr.*, 17, 1914 (No. 76), 43-44.

('losers' in Fig. 2) gave exactly the opposite result, performing best at the extreme rather than the intermediate values. Secondly, examination of results in the fore-test, before any conditioning, showed that although performance at each interval improved somewhat as the given interval was used further toward the end of the series of six trials, this improvement did not vary in any meaningful way from interval to interval, and it is unlikely that the intermediate intervals gained more than other intervals in the remaining eight *CS–PS* trials, including the trials of the after-test.

Another methodological point concerns the ambiguity in the predicted effect of conditioned fear, ranging from activation to inhibition of the test-response. It was inferred that the inversion at 3.5 sec. in the main data of the after-test (Fig. 1) was due to the interfering effects of fear, and the interests of consistency might demand that fear also be assumed to occur strongly at 0.5 and 5.5 sec. Conversely, complete concentration on the activating effect of fear could lead to the conclusion that the response is strongest at 2.5 and 4.5 sec. In the present instance it is thought that the ambiguity has been resolved by reference to the performance of the groups of 'gainers' and 'losers', and to previous findings. The solution to the problem in general terms, however, appears to be more appropriately found in prior experimental control so as to insure either the clear dominance of, or interference with, the test-response. In retrospect, for example, clearer results might have been obtained in this study if the *S*s had practiced the key-pressing response to a greater extent before fear-conditioning.

Two features of the present data appear to be of general theoretical significance. First, the findings suggest that when one and the same stimulus elicits both the fear-response and an overt, instrumental response, the latter will occur before the former. This being so, fear cannot unfailingly prevent a given response from occurring, although it may have a modifying or inhibiting effect once the response has begun. Secondly, the data provide further evidence that fear may modify a contemporary response through activation as well as inhibition. The possibility of some consistency in an individual's reaction to fear, as exemplified in the groups of 'gainers' and 'losers,' may relate back to emotionality as a 'state' variable. It is most likely that the occurrence of an increment or a decrement in performance as a result of fear-conditioning depends on the dynamics of the experimental situation at the moment the *PS* is administered, in terms of the present experiment, as well as on *S*'s past experience in similar situations. In addition to these factors, however, there may be some inherent property of the organism which contributes to the nature of the reaction to fear. It might be fruitful to conduct an experiment of the present type on groups of high-anxious and low-anxious *Ss*, perhaps with the expectation that the former group would present responses to fear which would in turn interfere with all but the very simplest (reflex) test-response to a *PS*. The same procedure could be used to test the effects of anxiety-inducing and anxiety-reducing drugs on the conditioning of fear in humans.

### SUMMARY

A fear response to a tone was conditioned in a group of 44 *Ss* by consistently pairing the tone with shock. Late in training the shock was replaced on some trials by a light in response to which *S* had been instructed to press a key as quickly as possible, and the tone was presented 0.5, 1.5, 2.5, 3.5, 4.5 and 5.5 sec. before the light. By comparison with similar tests of *RT* before fear-conditioning, this use of the probe-stimulus technique produced data which suggested that the fear-response has a latency greater than 0.5 sec. and possibly of the order of 3.5 sec. This conclusion was confirmed by an examination of the results of those of *Ss* who showed some consistency in their reaction to fear-conditioning by way of gains or losses in performance. Some theoretical implications of these findings were discussed, with particular reference to Mowrer's theory of behavior and to the distinction between anxiety and fear.

# STIMULUS-CHARACTERISTICS AS DETERMINANTS OF SEMANTIC CHANGES WITH REPEATED PRESENTATION

By LEON A. JAKOBOVITS and WALLACE E. LAMBERT, McGill University

'Verbal satiation' is defined as the change or loss of a word's meaning due to the continued inspection or verbal repetition of the word itself.

The phenomenon was investigated as early as 1907 in Titchener's laboratory.[1] Since then, however, the number of studies that have appeared in the literature on this subject is small.[2] The full significance of the phenomenon was not duly recognized, possibly because of the lack of objective measures of satiation. In view of the current interest among psychologists in meaning and cognitive processes, the phenomenon of verbal satiation has taken on a new significance. Recently, Lambert and Jakobovits developed an objective method for measuring the effects of word-repetition on meaning and several experiments have demonstrated that verbal or 'semantic' satiation has a wide range of generality.[3]

Titchener's group studied the relation between certain characteristics of words and their liability to satiation. It was found, for example, that short words and

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[*] Received for publication December 4, 1962. This research was supported in part by the Canadian Defence Research Board Grant D77-9401-10, and in part by a subvention from the Carnegie Corporation of New York.

[1] Elizabeth Severance and M. F. Washburn, The loss of associative power in words after long fixation, this JOURNAL, 18, 1907, 182-186; E. B. Titchener, *A Beginner's Psychology,* 1915, 118; M. C. Bassett and H. P. Warne, On the lapse of verbal meaning with repetition, this JOURNAL, 30, 1919, 415-418; V. J. Don and H. P. Weld, Lapse of meaning with visual fixation, this JOURNAL, 35, 1924, 446-450.

[2] Molly Mason, Changes in the galvanic skin response accompanying reports of changes in meaning during oral repetition, *J. gen. Psychol.,* 25, 1941, 353-401; J. J. Gibson, *The Perception of the Visual World,* 1950, 204; D. E. P. Smith and A. L. Raygor, Verbal satiation and personality, *J. abnorm. soc. Psychol.,* 52, 1956, 323-326; Michael Wertheimer, The relation between the sound of a word and its meaning, this JOURNAL, 71, 1958, 412-415; Studies of some Gestalt qualities of words, in F. Weinhandl's (ed.), *Gestalthaftes Sehen: Erbegnisse und Aufgaben der Morphologie,* 1960, 398-405; Michael Wertheimer and W. M. Gillis, Satiation and the rate of lapse of verbal meaning, *J. gen. Psychol.,* 59, 1958, 79-85; A. A. Miller, Verbal satiation and the role of concurrent activity, *J. abnorm. soc. Psychol.,* 66, 1962, 206-212; A study of the role of sensory-motor activity in the maintenance of verbal meaning of action words, Unpublished Ph.D. Dissertation, Clark University, 1959; W. E. Lambert and L. A. Jakobovits, Verbal satiation and changes in the intensity of meaning, *J. exp. Psychol.,* 60, 1960, 376-383.

[3] L. A. Jakobovits and W. E. Lambert, Mediated satiation in verbal transfer, *J. exp. Psychol.,* 64, 1962, 346-351; Semantic satiation in an addition task, *Canad. J. Psychol.,* 16, 1962, 112-119; The effects of repetition in communication on meanings and attitudes, in *Television and Human Behavior: Tomorrow's Research in Mass Communications,* (in press); Semantic satiation among bilinguals, *J. exp. Psychol.,* 62, 1961, 576-582; R. Kanungo, W. E. Lambert, and S. M. Mauer, Semantic satiation and paired-associate learning, *J. exp. Psychol.,* 64, 1962, 600-607.

words with concrete referents are more resistant to lapses of meaning through repetition than are longer words and words with abstract referents. These early findings were confirmed by Wertheimer and his associates, who, in addition, have tried to show that 'emotional' words retain their meanings longer than non-emotional words.[4] To our knowledge, no study has been reported that extends this line of investigation to stimulus-materials other than words, relating variations in their characteristics to meaningful changes attributable to continuous inspection. The method used by Lambert and Jakobovits is appropriate for measuring changes in the meanings of both non-verbal and verbal materials.[5]

The present study examines the effects of repeated presentation of different types of materials on their meanings.

## METHOD AND PROCEDURE

*Material.* A variety of related stimulus-materials was used: the names of objects, the actual objects, and both clear and underexposed photographs of them. Names are actually category-labels that encompass a large number and variety of specific objects all differing from each other in many details. Thus the word 'pencil' does not identify idiosyncratic characteristics of the referent (size, color, specific detail) as much as a particular pencil does. Black and white photographs of objects are less specific than the objects themselves in that they are two-dimensional and have no color, but are more specific than words, because they reveal shape and size. In the same sense, underexposed photographs are less specific than clear ones, since small details such as indentation, shadowing, inequalities, or roughness are eliminated. Table I illustrates how the four types of materials differ in certain characteristics. It can be seen that objects, being most specific, contain the largest number of relevant visual characteristics, and that the series forms a very rough continuum of 'specificity' similar to the dimension of 'abstractness' used by Karwoski, Gramlick, and Arnott.[6] Osgood, in his discussion of differences in free-association responses related to differences between these materials, described three levels or degrees of abstraction: ". . . the pattern of stimulation from the object is the concrete 'object'

TABLE I

RELEVANT (+) AND NON-RELEVANT (−) DIMENSIONS FOR THE FOUR TYPES OF STIMULUS-MATERIALS USED

| Stimulus-materials | Visual characteristics | | | | Non-visual |
|---|---|---|---|---|---|
| | depth | color | detail | size | utility |
| Objects | + | + | + | + | + |
| Clear photographs | − | − | + | + | + |
| Underexposed photographs | − | − | − | + | + |
| Words | − | − | − | − | + |

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[4] Wertheimer, *op. cit.*, 1960, 398-405.

[5] Lambert and Jakobovits, *op. cit.*, 1960, 376-383.

[6] T. F. Karwoski, F. W. Gramlick, and Peter Arnott, Psychological studies in semantics: I. Free association reactions to words, drawings, and objects, *J. soc. Psychol.*, 20, 1944, 233-247.

itself; the pictorial representation is a sign of the object but is itself physically similar to the object in certain respects; and the label is yet further abstracted."[7]

Nine small objects, described in Table II, were selected from those used in a study by Wimer and Lambert.[8] Two black and white high-gloss prints (5 × 3 in.) of each object were prepared by a commercial photographer. One of these was a perfect reproduction, the other was underexposed by about 20%, making the object appear light and washed out, although still recognizable.

*Procedure.* Each of the four classes of stimulus-objects was presented to a different group of *Ss*. Three objects (button, key, wheel) were administered to the *Ss* of Group 1 in the experimental conditions and three other objects (pencil, ring, mirror) in the control condition. Each object was pasted on a 5 × 3-in. white index-card and so inserted in a Kardex folder that *E* could expose them in a predetermined random order. Six semantic differential scales (pleasant-unpleasant; nice-awful; large-small; heavy-light; fast-slow; sharp-dull) were printed on separate cards (5 × 3 in.) and also inserted in the Kardex unit. The *Ss* were tested individually under each of the following conditions.

TABLE II

DESCRIPTION OF PROCEDURE AND MATERIALS USED FOR GROUP 1

The object in parenthesis is inspected, while the object that follows it is rated on semantic scales. The stimulus-materials for Group 2 were clear photographs of the objects; for Group 3, underexposed photographs of the objects; and for Group 4, the names (underlined) of the objects printed on white index cards.

| Condition | Description | Objects used |
|---|---|---|
| Initial normal | Ratings given to objects exposed for about 1 sec. | large plain black *button*, unnotched gold *key*, small *wheel* with four axes, short red *pencil*, gold-coloured *ring*, oval hand *mirror*. |
| Experimental | 15-sec. inspection of an object followed by semantic rating of that object. | (button): *button;*<br>(key): *key;*<br>(wheel): *wheel.* |
| Control | 15-sec. inspection of an object followed by semantic rating of a different object. | (*nickel* coin): *pencil;*<br>(unburnt wooden *match*): *ring*;<br>(*hairpin*): *mirror.* |

*Initial normal.* At the beginning of the experimental session, *S* was required to rate each object on each semantic scale by pointing to one of the seven positions on the scale; his responses were recorded by *E*. For the three objects subsequently used as experimental inspection-materials, the following procedure was used: *E* would first expose the object, for example the button, for about a second, by lifting the flap of the Kardex folder twice in quick succession; then, immediately after, he would expose a semantic scale, and *S* would make his rating. For the three objects subsequently used as controls, *E* would first expose the object once (*e.g.* the nickel, see Table II); this would be quickly followed by the exposure of a second object

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[7] C. E. Osgood, *Method and Theory in Experimental Psychology*, 1953, 711.

[8] C. C. Wimer and W. E. Lambert, The differential effects of word and object stimuli on the learning of paired associates, *J. exp. Psychol.*, 57, 1959, 31-36.

(*e.g.* the pencil), and *S* would rate it. The reason for this procedure was to make the initial condition exactly similiar to the control condition described below. The order of the 36 ratings (6 objects on 6 scales) was systematically randomized for half of the group; this order was then reversed for the other half of the group.

*Experimental inspection.* Following a short rest-period, each of the three objects used in this condition was exposed a second time for a period of 15 sec. before *S* rated it on a scale. As there were six semantic scales, each object was seen for six 15-sec. periods.

*Control condition.* For this condition, *E* exposed the stimulus-material for 15 sec.; then a second object for 1 sec., and then required *S* to give his semantic rating to the second object. Since the exposure-time for the rated (second) stimulus-object was the same as under the initial normal condition, no systematic change in ratings was expected here, and this condition was intended to serve as a control for the 15-sec. inspection-task in the experimental condition. The 36 final ratings under these two conditions were systematically randomized for half of the group, and the reverse order was used for the other half. A summary of the procedure used for Group 1 is given in Table II. The present procedure was exactly duplicated for each of the other three groups, the only difference being the type of stimulus used: Group 2 received the clear photographs; Group 3, the underexposed photographs; and Group 4, words standing for the objects themselves.

*Subjects.* The *Ss* were 88 students enrolled in various courses at McGill University who volunteered to participate. Each of the four groups was composed of 22 people. The first 22 names on the list were placed in Group 1, the subsequent 22 in Group 2, etc. The same *E* tested all *Ss* and they were given identical instructions, except, of course, for the reference to the type of stimulus-objects used with each group.

*Scoring.* The data are reported in the form of polarity-difference scores which are differences in the intensity of ratings between the initial normal condition and the final inspection conditions. The semantic differential yields four intensity-positions (0 to 3). A positive polarity-difference score indicates an increase in intensity of semantic ratings ('semantic generation'), while a negative score indicates a decrease ('semantic satiation').

## RESULTS

Tests of significance were applied to the polarity-difference scores between the first and second half of each group to determine the effect of the presentation-order of the stimuli. None of the four *t*-values was significant (the largest, for Group 3, was 1.26). The results for the two halves of each group were therefore combined.

Table III summarizes the results and analyses. For each group, the mean initial normal and final (after inspection) ratings and their standard deviations were given. This mean represents the average intensity of rating per stimulus-material (object, photograph, or word) over the sum of 6 scales, and was obtained for each *S* by dividing the sum of his 36 ratings by 3. Also given are the mean polarity-difference scores for each group and their significance-level for departure from zero (or no change). It can be seen that Group 1 (objects) has a signficant positive difference-score, indicating a semantic generation-effect, while Group 4 (words) has a significant negative difference-score, indicating a semantic satiation-effect. The difference-means for Groups 2 and 3 are both negative, but neither is significant.

Comparison between the polarity-difference scores for experimental and control conditions within each group reveals that all but one of these (Group 2) are significantly different from each other.

The data in Table II are plotted in Figs. 1 and 2. It can be seen in Fig. 1 that the initial extremity of ratings for the control stimuli are consistently and significantly (except for Group 4) higher than that of the experimental stimulus-objects. These differences could be due to the different procedures for presenting the materials in the two conditions or, possibly, to the materials actually presented for each control. It is unlikely, however, that initial differences between experimental and control conditions influenced the experimental effects to be noted in Fig. 2, since all scores fall within the middle range of the extremity continuum and since no systematic relation is noted between initial and final polarity scores.

TABLE III

MEAN POLARITY SCORES (INITIAL, FINAL, AND DIFFERENCE) PER STIMULUS-MATERIALS OVER THE SUM OF SIX SCALES FOR ALL FOUR GROUPS
($N = 22$ for each Group)

| Polarity score | Group 1 | | Group 2 | | Group 3 | | Group 4 | |
|---|---|---|---|---|---|---|---|---|
| | Mean | *SD* | Mean | *SD* | Mean | *SD* | Mean | *SD* |
| Experimental: | | | | | | | | |
| Initial | 6.79 | 3.11 | 5.98 | 4.23 | 6.55 | 3.63 | 6.20 | 4.16 |
| Final | 8.39 | 4.87 | 5.75 | 5.03 | 6.00 | 4.82 | 5.29 | 4.90 |
| Difference* | 1.60 | 2.06 | −0.23 | 2.15 | −0.55 | 1.63 | −0.91 | 1.28 |
| | (3.55) | | (0.49) | | (1.54) | | (3.25) | |
| Control: | | | | | | | | |
| Initial | 7.97 | 4.07 | 7.56 | 4.28 | 7.95 | 3.02 | 6.76 | 4.70 |
| Final | 8.01 | 4.33 | 8.24 | 6.37 | 8.74 | 3.89 | 6.63 | 5.33 |
| Difference* | 0.04 | 1.83 | 0.68 | 2.71 | 0.79 | 1.79 | −0.13 | 1.19 |
| | (0.10) | | (1.15) | | (2.02) | | (0.50) | |
| *t* (exp.-diff.) vs. (cont.-diff.) | 2.69 | — | 1.25 | — | 2.63 | — | 2.11 | — |

* The number in parenthesis is the *t*-value for departure from zero. Values of 2.08 are significant at the 5% level; values of 2.51 are significant at the 2% level, two-tailed tests.

On the other hand, the differences in extremity of initial ratings for the four types of material is not significant for either the experimental or control condition, as is indicated by a one way analysis of variance by ranks ($H = 1.48$, *n.s.*, for the initial experimental curve, and $H = 3.62$, *n.s.*, for the initial control curve).

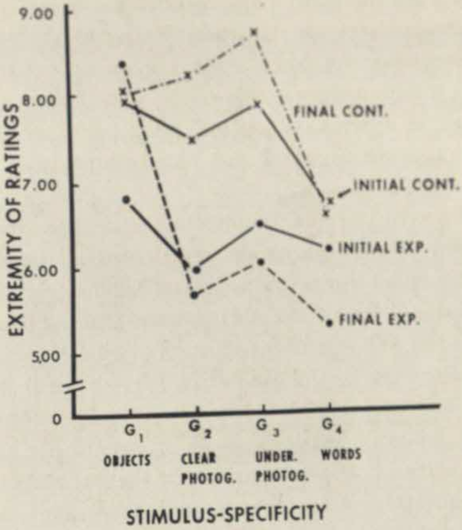

FIG 1. MEAN INITIAL AND FINAL EXTREMITY OF RATINGS FOR EACH OF THE FOUR GROUPS

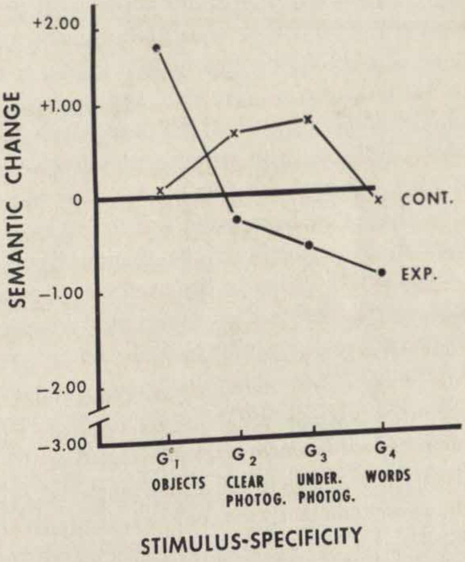

FIG. 2. DIRECTION AND AMOUNT OF SEMANTIC CHANGES WITH REPEATED PRESENTATION AS A FUNCTION OF STIMULUS-SPECIFICITY

The main experimental finding is plotted in Fig. 2. A one-way analysis of variance for the experimental curve revealed a highly significant effect ($F = 7.95$; $P < 0.001$). The comparable analysis for the control curve showed an insignificant effect ($F = 0.81$; *n.s.*). It would seem, then, that the kind and degree of semantic change following repeated presentation depend on the characteristics of the stimulus-materials used.

## Discussion

Osgood identifies meaning with the representative, mediational process which a stimulus-object elicits.[9] The semantic differential is believed to index the content and strength of this process. Semantic change can thus be conceptualized as a change in the strength or kind of mediational processes involved. For example, Johnson *et al.* have shown that the intensity with which nonsense-words are rated by *S*s on the "good-bad" dimension of the semantic differential scale increases after an *S* has an opportunity of seeing them several times in print, despite the fact that no meaningful associates were presented.[10] In other words, the strength with which an unfamiliar stimulus-object elicits a particular mediational process (represented in this case by the 'good-bad' dimension) is close to zero (as indexed by near-zero ratings). As the number of presentations of the stimulus-object is increased, the strength of the mediational process which is elicited also increases to a proportion equal to the number of presentations (see Johnson *et al.*, Fig. 1).[11] This finding is analogous to the increase in amplitude of a reaction (*e.g. GSR*) with repeated reinforcement (see Hovland)[12] and is in agreement with Hebb's neurophysiological postulate of increased neural facilitation with repeated stimulation.[13] In the present experiment, the presentation of the stimulus-materials was held constant, while the type of the material presented was varied along a specificity-generality continuum. We argue that the continued presentation of a stimulus-object repeatedly elicits the particular mediation process associated with it. Under these circumstances three events can take place. One is that the mediational processes continue to be elicited with no systematic change in strength. This outcome is unlikely in view of the postulated neurophysiological facilitation following repeated stimulation on one hand and, on the other, the neural reactive inhibition associated with repeated firing hypothesized by Lambert and Jakobovits.[14] Which of the last two events will take place apparently depends on the characteristics of the mediational process under consideration (which are determined by characteristics of the stimulus materials) and the state of excitation at the start of the process. Assuming the latter to be constant, the material presented will be the major factor determining whether repeated presentation will induce an increase in the intensity of meaning (generation) or a decrease (satiation).

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[9] Osgood, *op. cit.*, 1953, 695-698; C. E. Osgood, G. J. Suci, and P. H. Tannenbaum, *The Measurement of Meaning*, 1957, 5-9.

[10] R. C. Johnson, C. W. Thomson, and Gerald Frincke, Word values, word frequency, and visual duration thresholds, *Psychol. Rev.*, 67, 1960, 332-342.

[11] Johnson, Thomson, and Frincke, *op. cit.*, 337.

[12] C. I. Hovland, The generalization of conditioned responses: I. The sensory generalization of conditioned responses with varying frequencies of tone, *J. gen. Psychol.*, 17, 1937, 125-148.

[13] D. O. Hebb, *The Organization of Behavior*, 1949, 62.

We shall assume that the direction and strength of semantic change with repeated presentation of a stimulus-object are a function of the total number of processes which can potentially be elicited by that object when it is inspected for a period of time. If this number is large, the strength and number of mediational processes which are elicited will increase, promoting semantic generation; if the number is relatively small, repeated elicitation will lead to a decrease in strength due to the accumulation of neural reactive inhibition (semantic satiation). Table I illustrates how the four types of stimulus-materials used in the present study can be conceptualized as differing with regard to certain visual characteristics, such as depth, detail, and size. Theoretically, the semantic differential should be able to index these differences. It is likely that if the appropriate scales are used, these differences would emerge (*e.g.:* red-green; two dimensional-three dimensional; rough-smooth; long-short; etc.). When the stimulus-material is a word, none of the visual dimensions in Table I is particularly relevant and the semantic rating given them should approach zero. With photographs, some are relevant while others are not, and with objects, all of the dimensions are relevant. Hence, repeated presentation should lead to semantic satiation for words, semantic generation for objects, and some intermediary effect for photographs, depending on the number of potentially relevant dimensions they contain.

This interpretation follows from the mediational hypothesis of meaning and the assumption that the concepts of response-strength and inhibition apply to central processes,a position also adopted, among others, by Neal Miller.[15] This theoretical notion is consistent with the effects of continuous repetition of words in paired-associates learning,[16] in arithmetic computational tasks,[17] and in linguistic tasks given bilinguals.[18] It also suggests next steps that can test the adequacy of the theory itself. For example, if the inspection-time of objects is increased beyond a certain point (to be determined empirically) semantic satiation should result. Similarly, if inspection-time for words is decreased sufficiently, semantic generation should result. Furthermore, mediational processes that are initially 'strong' will be more resistant to change, where "strong" might refer to food presented to hungry *S*s, or words selected from *S*s' dominant value-areas, or words chosen from the 'mother tongue' of a bilingual.

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[14] Lambert and Jakobovits, Verbal satiation and changes in the intensity of meaning, *J. exp. Psychol.*, 60, 1960, 376-383.

[15] N. E. Miller, Liberalization of basic S-R concepts: Extensions to conflict behavior, motivation, and social learning, in Sigmund Koch's (ed.), *Psychology: A Study of a Science,* 2, 1959, 238-252.

[16] Kanungo, Lambert, and Mauer, *op. cit.,* 1962.

## SUMMARY

The purpose of the present study was to investigate the effects of repeated presentation of different types of stimuli upon their meanings. Since semantic satiation has been previously demonstrated with names of objects, it was decided to use words as one of several related materials, together with objects themselves, and both clear and underexposed photographs of objects. The four types of materials were administered separately to four different groups of *S*s under an experimental condition of inspection as well as under control conditions. Comparison of the semantic changes (as indexed by the semantic differential) obtained with each of the four groups showed that they differed significantly on the inspectional condition, but not on the control. These differences were related to the characteristics of the type of materials used with each group. The results were interpreted within the terms of a mediational theory of meaning.

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[17] Jakobovits and Lambert, Semantic satiation in an addition task, *Canad. J. Psychol.*, 16, 1962, 112-119.

[18] *Idem*, Semantic satiation among bilinguals, *J. exp. Psychol.*, 62, 1961, 576-582.

# THE EFFECT OF VISUAL INTENSITIES UPON THE JUDGMENTS OF LOUDNESS

By WILLIAM BEVAN and JOAN FAYE PRITCHARD
Kansas State University

In a recent issue of this JOURNAL, Behar and Bevan reported a series of experiments dealing with cross-modal relations in the judgment of filled temporal intervals.[1] Using the anchor-paradigm frequently employed in studies of adaptation-level, these investigators asked their *Ss* to judge either auditory or visual signals, 1-5 sec. in duration, along with visual or auditory anchors of, in most experiments, 10-20 sec. duration.[2] Their results demonstrate consistently that intermodal anchor-effects are capable of induction, although they are clearly less marked than corresponding intramodal effects. Visual intermodal effects were 67% as great as visual intramodal effects, while auditory intermodal anchors produced shifts only 22% as great as those associated with auditory intramodal anchors.

In the present experiment, cross-modal relations in the judgment of sensory magnitude are studied. The earlier work already cited was concerned with a single input-dimension—signal duration—although the avenue of input varied. In the present case, it is assumed that the intermodal relations are analogical. The *Ss* judge the brightness of visual signals and the loudness of auditory stimuli, both dimensions being characterized as intensitive. Since the peripheral mechanisms associated with each dimension are qualitatively distinct, the demonstration of a cross-modal effect for brightness-loudness should have important implications for an understanding of sensory judgments.

A secondary purpose of this experiment is to study the relation of response-language to judgment. It has been suggested that anchor-effects are simply the result of adjustment in the verbal labels applied to sensory magnitudes which is made to accommodate the greater range of inputs

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* Received for publication February 10, 1962. This work was supported by the Office of Naval Research under terms of Contracts Nonr-3290(01) and Nonr-3634(01).

[1] Isaac Behar and William Bevan, The perceived duration of auditory and visual intervals: Cross-modal comparison and interaction, this JOURNAL, 74, 1961, 17-26.

[2] The anchor-stimuli were presented for judgment without any particular identification except widely deviant magnitudes. Helson (personal communication) prefers to call such stimuli "predominant" members of the series.

associated with the inclusion of the anchor.[3] If such is the case, one might expect anchor-effects to be enhanced if judges are allowed to express their responses in terms of a single conceptual scheme instead of in terms of the language of two separate, qualitatively different sensory spheres.

*Method: (1) Design.* The present experiment involves a simple, multiple-group design. In the first stage, two groups were tested to obtain an intramodal effect as baseline data. Group 1 provided control data for the auditory series alone, and Group A received the control series plus an anchor-series above the control-range.[4] Fig. 1A graphically presents the results for these two groups. The intramodal anchor-effect is apparent and highly significant (for means, $F = 15.23$, $df = 1$ and 18, $p < 0.01$; for slopes, $F = 71.50$, $df = 1$ and 72, $p < 0.001$).[5] Group *B*, studied next, had the auditory test-series plus a series of visual stimuli matched, stimulus-for-stimulus, to the subjective intensity of the auditory-anchor series. Two additional groups, C and E, had visual-anchor series differing in intensity from the stimuli of Group B. The purpose of these groups was to demonstrate a graded cross-modal anchor-effect and the decision to increase or decrease the anchor-magnitude was to depend on the results for Group B. The decision was to increase. Groups 2 and D repeated the conditions of Groups 1 and C except that the response-language was changed, the purpose being to check on the possible effect of a semantic factor.

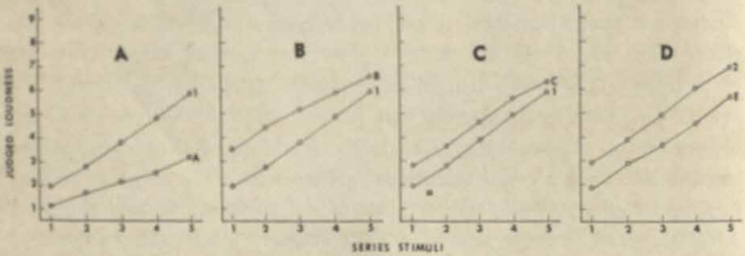

FIG. 1. THE EFFECT OF AUDITORY AND VISUAL ANCHORS ON THE JUDGMENT OF LOUDNESS

'A' presents data for the test-series alone and for the test-series plus a more intense auditory anchor-series; 'B' presents comparable data for the test-series alone and for the test-series plus a visual anchor-series; 'C,' a series moderately more intense than the matched series; and 'D,' a series markedly more intense than the matched series. The control groups (test-series alone) are numbered; the experimental groups (test-series plus anchor-series) are lettered.

*(2) Subjects.* Seventy undergraduate students, 35 men and 35 women, served as

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[3] S. S. Stevens, Adaptation-level vs. the relativity of judgment, this JOURNAL, 71, 1958, 633-646.

[4] The control groups (test-series alone) are identified by number; the experimental groups (test-series plus anchor-series) by letter.

[5] All meaningful comparisons were evaluated statistically by means of Alexander's Trend Test with between-individual-means as the error term for between group-means, and the combined between-individual-slopes and individual deviations from estimate as the error-term for between group slopes.

*Ss*. Five men and five women were assigned to each of the seven groups listed above.

*(3) Apparatus.* The auditory signals, different intensities of a 1000-cycle tone, were generated by a Hewlett Packard Model 200 CD wide-range oscillator. Intensity was controlled by means of a Hewlett Packard Model 350B 5-w., 600-ohm attenuator set. The elimination of clicks associated with onset and offset and control of rise and decay was achieved by means of a Grason Stadler electronic switch with the rise-decay time set at 25 m.sec. Stimuli were presented binaurally through Telephonics Model TDH-39 high-fidelity headphones.

The visual-anchor series was presented by means of a modified Lafayette Instrument Model 701 Light-Discrimination Apparatus. The left aperture was occluded and a 100-w. incandescent lamp used as the source. The apparatus was so mounted that the stimulus-patch was at eye-level, about 5 ft. directly in front of *S*. Viewing was binocular. The test-patch consisted of the milk-glass-covered right aperture, 1.75 in. in diameter. Intensity of the light source was regulated by means of a Superior Electric Co. Type 116 Powerstat. Presentation-interval and stimulus-duration were controlled by means of two Hunter Golden Silence Model 111C interval-timers.

All equipment except the light-source was mounted on a large table about 3 ft. behind *S*. The experiment was conducted in a sound-insulated room kept dark except for the visual stimulus and a dim lamp used by *E* to allow for operating the equipment and recording responses.

*(4) Stimulus-series.* The stimuli were each of 2-sec. duration and presented at intervals of 8 sec. Each *S* made a total of 130 judgments in a single session, the first 30 of which were practice-trails to allow *S* to become familiar with the procedure and to develop his judgmental criterion. Each stimulus, both test-series and anchor, had a predetermined relative frequency of presentation which was preserved in successive blocks of 20 test-trials and in the block of 30 practice-trials as well. Otherwise the order of presentation was essentially random.

The auditory test-series was made up of five different intensities, 38, 41, 44, 47, and 50 *db*. S.P.L., each presented on 20% of the trials. Groups 1 and 2 (the control groups) received the test-series alone for 130 trials. All other groups received this series on half the total or 65 trials.

The auditory anchor-series was made up of five intensities, all louder than those of the test-series and programmed as a distribution skewed toward the more intense end. The intensities were 53, 56, 59, 62, and 65 *db*. S.P.L., presented 5, 5, 10, 10, and 20% of the trials respectively. This series was presented to Group A on 65 trials interspersed with the 65 trials of the auditory test-series.

The matched visual anchor-series consisted of five lights matched in subjective intensity to each of the tones of the auditory anchor-series. The matched values were derived from the judgments of an instructor and three graduate students. Each made 10 matched settings of brightness to loudness for each stimulus in the auditory anchor-series. Medians of the sets of 10 settings were taken as the equivalence-values for each judge. The mean of the medians for the four judges provided the values presented to the experimental *Ss*. These were 5.4, 7.6, 11.2, 15.1, and 46.1 ft.-c. presented to Group B on 5, 5, 10, 10, and 20% of the 65 anchor-trials respectively.

A moderately more intense visual anchor-series was presented to Groups C and D on their 65 anchor-trials. The frequency-structure of the series remained the same as that used for Groups A and B, but the physical magnitudes were increased to 230, 315, 390, 563, and 797 ft. A markedly more intense visual anchor-series was presented to Group E on its 65 anchor-trials. It represented a further increase in physical intensity over the anchor-series given to Groups C and D: 1078, 1188, 1469, 1840, and 2900 ft.-c.

*(5) Test-procedure.* Each *S* was tested individually. After being made comfortable, he was told that the experiment was concerned with how people judged tones, or lights and tones. He then was instructed in the use of a nine-category rating scale placed before him. For *S*s receiving tones alone (Groups 1 and A) the descriptive labels varied from *very, very soft* to *very, very loud*. For *S*s receiving tones and lights (Groups B, C, and E) double labels were provided from *very, very soft* or *dim* to *very, very loud* or *bright*. Nothing was said to Groups 2 and D concerning the qualitative properties associated with either modality. Instead, they were instructed to refer *all* judgments to a *single* scale varying from *very, very weak* to *very, very intense*. Each *S* was cautioned to base his judgments on the experimental series and to resist any preconceived notions about intensity which he happened to have. At the same time he was urged to choose the rating that best described the stimulus and to use the full range of the scale or scales, adding categories (by using the adverb *very* an appropriate number of times) when he regarded this to be necessary.

*Results: (1) Judgments and the response-language.* The notion of semantic relativity has important implications. If anchor-effects mean nothing more than the shifting of response-labels, they become relatively trivial phenomena. If, however, patterns of stimulus-input influence the perceptions that underly the judgments of individual stimuli, then the study of anchor-effects remains an effective device for identifying fundamental properties of psychophysiological function.

In the case of the present study it was reasoned that, if anchoring reflected simply the reassignment of labels, then allowing *S* to express all judgments in terms of a single set of categorical names rather than requiring him to use two separate sets of terms, each associated with a different modality, should make scaling easier and thus enhance the possibility of obtaining an anchor-effect. Analyses of variance were performed upon the data of Groups 1 and 2 and Groups C and D as a test of the semantic hypothesis. Neither comparison revealed a significant difference in the average judgments for the two conditions (for 1 vs. 2, $F = 3.46$, $df. = 1$ and 18, $p > 0.05$; for C vs. D, $F = 1.13$, $df. = 1$ and 18, $p > 0.05$). This suggests that response-language had no systematic effect upon judgment for either the control or for an anchor-condition.

Such results do not, of course, rule out a semantic mechanism. There are situations, *e.g.* those involving purely verbal materials, in which the operation of such a mechanism seems quite plausible.[6] Data also are available, however, which suggest the existence of context-effects independent of semantic considerations. For example, recent papers have reported judgmental shifts associated with subliminal anchors, maze and runway performances which reflect the influence of relative as compared with absolute intensities of reinforcement, and background-effects upon judgment when the responses involved the direct estimation of magnitude rather than the relative responses of category-scales.[7] Finally, that judgment is not merely the arrangement of response-labels is indicated by the reports of systematic intermodal differences in the judgment of duration.[8]

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[6] Harry Helson, R. S. Dworkin, and W. C. Michels, Quantitative denotations of common terms as a function of background, this JOURNAL, 69, 1956, 194-208.

*(2) Cross-modal anchor-effects.* Panels B, C, and D of Fig. 1 present the data on cross-modal anchor-effects. Since no differences were found to be associated with response-language, Group E was instructed, as a matter of convenience, to respond in terms of intensity rather than brightness or loudness. The results for this group are therefore plotted against those of Control Group 2 rather than those of Group 1.

The data for the matched cross-modal anchor-conditions are shown in Fig. 1B. The graph suggests, and statistical tests reveal, a significant anchor-effect ($F_{\text{for means}} = 4.52$, $df = 1$ and 18, $p < 0.05$; $F_{\text{for slopes}} = 71.50$, $df = 1$ and 72, $p < 0.001$). This effect might be called a *paradoxical anchor effect,* in the same sense that a cold-sensation produced by a stimulus with a temperature above physiological zero is called 'paradoxical.' The effect is paradoxical on two counts: First, the judgments of the anchor-group are above the corresponding control values, although the anchor-lights were matched to the auditory test-series and, therefore, if they produced an anchor-effect, should have yielded a curve below the control curve. Secondly, the control and the anchor-curves diverge at their lower values. This is the pattern produced by anchors *below* the series.

We can only speculate as to the cause of the *paradoxical anchor effect.* One possibility relates to the demonstration by Behar and Bevan that cross-modal anchors are less effective than anchors in the same modality as the series. Anchor-effectiveness can refer to anchor-reliability; it also can mean, as the data of Behar and Bevan suggest, anchor-potency. If anchor-potency is related to anchor-magnitude, then using a cross-modal anchor is equivalent to selecting an anchor of lesser magnitude. If the loss in potency is great enough, the result could be the equivalent of that produced by an anchor below the series.

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[7] R. W. Black and William Bevan, The effect of subliminal shock upon the judged intensity of weak shock, this JOURNAL, 73, 1960, 262-267, William Bevan and Robert Adamson, Reinforcers and reinforcement: Their relation to maze performance, *J. exp. Psychol.*, 59, 1960, 226-232; Roger Black, Robert Adamson, and William Bevan, Runway behavior as a function of apparent intensity of shock, *J. comp. physiol. Psychol.*, 54, 1961, 270-274; William Bevan, R. A. Maier, and Harry Helson, The influence of context upon the estimation of number, this JOURNAL, 76, 1963, 464-469.

[8] Sanford Goldstone, W. K. Boardman, and W. T. Lhamon, Intersensory comparison of temporal judgments, *J. exp. Psychol.*, 57, 1959, 243-248; Isaac Behar and William Bevan, Analysis of the prime psychophysical judgment, *Percept. mot. Skills*, 10, 1960, 82.

Regardless of the explanation, it should be possible to check the validity of the paradoxical effect simply by increasing the intensity of the light-anchor series. This operation should reduce the paradoxical effect, or, if appropriately strong, result in a nullification or even a reversal of the effect. Accordingly, Groups C and E were used, and Fig. 1C and 1D confirm the prediction. The curves of Fig. 1C suggests a reduction of the paradoxical effect; statistical test indicates a nullification of the anchor-effect, for the differences between the two curves is not significant ($F_{\text{for means}} = 2.44$, $df = 1$ and 18, $p > 0.05$; $F_{\text{for slopes}} = 1.42$, $df = 1$ and 72, $p > 0.05$). The curves of Fig. 1D and statistical tests suggest that a further increase in physical intensity has produced a reversal of the paradoxical effect, the normal relation appearing between the control and anchor-curves on the graph ($F_{\text{for means}} = 5.98$, $df = 1$ and 18, $p < 0.05$; $F_{\text{for slopes}} = 0.63$, $df = 1$ and 72, $p > 0.05$). A further increase in anchor-intensity should make the normal effect more clear cut.

Our final concern in with the relative magnitude of the cross-modal effects. Inspection of Fig. 1 indicates that the cross-modal series in no instance approached the effectiveness of the intramodal series, even when the anchor-lights were physically quite intense. This result is consistent with earlier findings of Behar and Bevan.

## SUMMARY

The present experiment investigated the influence of visual stimuli upon the judgment of auditory intensities. In all, 70 *S*s participated. Different groups received a visual anchor-series equated, stimulus-for-stimulus, with a set of auditory anchors above the test-series; a visual anchor-series moderately more intense than the initial visual anchors, and a visual anchor-series markedly more intense than the initial set. When the visual anchors were equated with the auditory anchors above the test-series, the result was a significant, but paradoxical, anchor effect, the shift resembling that produced by an anchor below the series. Graded increases in the intensity of the lights nullified and reversed the paradoxical effect. A change of the response—language from judgments of loudness *or* brightness (two conceptually separate dimensions) to a single scale of stimulus-intensity had no reliable effect on judgment.

# RELATIONS BETWEEN PRODUCTION AND JUDGMENT OF PLOT-TITLES

By D. M. JOHNSON and D. J. ZERBOLIO
Michigan State University

It is possible to review the empirical evidence on thinking in terms of separate processes, such as preparation, production, and judgment;[1] and experimental attempts to differentiate one process from another have been reported, such as John's separation of analysis from synthesis,[2] and Johnson's isolation of preparation.[3] When judgment is considered, however, the strategy is not one of separation, because judgment of any object of thought is often separated in time and place from production of that object, and psychologists have traditionally investigated judgment and production as separate activities. The task of the present research is, rather, to show how judgment is related to the other intellectual activities. One experiment is reported on the influence of judgment on production, and another on the influence of production on judgment.[4]

Research of this kind requires *S* to construct products, the merits of which can be scaled on some significant dimension, and also to judge such products on the same dimension. Simple materials like lines and sounds can be conveniently produced and judged in the laboratory, but there is some advantage to using more complex materials that make greater intellectual demands and presumably have more significance for the psychology of thinking. If we turn to the factor-analysis of Wilson, Guilford, and Christensen,[5] which included a variety of tasks involving production, we find that the task with the highest loading for originality is *plot-titles* scored for cleverness. Hence, it is reasonable to hope that investigation of the relations between production and judgment of plot-titles will have some general significance for thinking.

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* Received for publication May 23, 1962. This research was supported by a grant from the National Science Foundation.

[1] D. M. Johnson, *The Psychology of Thought and Judgment*, 1955, Ch. 2.

[2] E. R. John, Contributions to the study of the problem-solving process, *Psychol. Monogr.*, 71, 1957, (No. 447), 1-39.

[3] Johnson, Formulation and reformulation of figure-concepts, this JOURNAL, 74, 1961, 418-424.

[4] Unpublished research by J. W. Jennings has demonstrated that a context-effect established during judgment will influence production, and the reverse.

[5] R. C. Wilson, J. P. Guilford, and P. R. Christensen, The measurement of individual differences in originality, *Psychol. Bull.*, 50, 1953, 362-370.

As a preliminary test, two paragraphs were written giving the outlines of simple plots, one concerning the Gilson family, the other Lady Durden, and these were tried out on small groups. The results showed that each plot elicited a satisfactory number of titles and that graduate research assistants could rate them for cleverness with satisfactory agreement.[6] Since the two plots did not differ significantly in number of titles elicited or in ratings of the titles, they were accepted for the main research. Experiments then were arranged to test the hypothesis that practice in judging plot titles for cleverness will facilitate the production of clever titles, and the reciprocal hypothesis that practice in producing clever titles will improve the accuracy of judgments of the cleverness of titles.

## THE INFLUENCE OF JUDGMENT ON PRODUCTION

The rationale of 'brainstorming' includes the assumption that the elimination or the postponement of judgment will facilitate production. The present hypothesis is the opposite, in the sense that deliberate practice in judgment is expected to facilitate production. The timing is different, however, in that the practice in judging is prior to rather than simultaneous with the production. Our hypothesis ties in also with the custom of giving students practice in the critical examination of works of literature with the hope that such practice will facilitate production of similar works.

Looking at this hypothesis in the light of the principles of transfer, we should not expect that production would be facilitated by practice in judgment as much as by practice in production, but we should expect that practice in identifying the criterion of judgment, *i.e.* cleverness, would transfer in some degree to the second task. If, by practice in judging, *S* learns to recognize a clever title when he sees one, perhaps he can use this skill to write more clever titles when requested to do so. Previous research indicates that instruction to be clever decreases the number and increases the cleverness of the titles produced,[7] and it is conceivable that practice in judging cleverness may establish a set that controls production in the same way. It seems desirable to compare the effect of practice in judging with the effects of instructions to be clever.

*Procedure.* Students in two college classes were treated as four groups, called 'Judgment-Clever' (*J-C*), 'Judgment-Appropriate' ,(*J-A*), 'Reading-Clever' (*R-C*), and 'Reading-Appropriate' (*R-A*). The titles obtained from preliminary work with the Gilson plot were rated by the two expert judges, graduate research assistants with experience on the preliminary run, on a 5-point scale. From those on which the judges agreed, five at each point of the scale were selected to make a rectangular distribution of 25 titles as practice for the *J-C* and *J-A* groups. After the plot, five titles were presented with a rating for each and an explanation of the ratings. Then 10 titles were presented for ratings, after which the expert ratings were announced and recorded by the *Ss* for comparison with their own, and then 10 more titles were presented for rating. The *R-C* and *R-A* groups were given the same titles to read as examples, with no mention of judgment.

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[6] J. W. Jennings kindly assisted with these ratings.

[7] P. R. Christensen, J. P. Guilford, and R. C. Wilson, Relations of creative responses to working time and instructions, *J. exp. Psychol.*, 53, 1957, 82-88.

After the judgment or reading, all groups were given the Durden plot with instructions to write titles for 12 min. The *J-C* and *R-C* groups were instructed as follows: "Read the short plot below. After reading it, make up and write down as many clever titles for this plot as you can. Your titles will be judged as to the degree of their cleverness." The *J-A* and *R-A* groups were instructed as follows: "Read the short plot below. After reading it make up and write down as many appropriate titles for this plot as you can. Appropriate titles are those that fit the plot."

The materials and instructions were printed in booklets, hence little oral instruction was required. One college class was divided into *J-C* and *J-A* groups of 23 and 22 *Ss*. Another class was divided into *R-C* and *R-A* groups of 23 and 24 *Ss*.

TABLE I

MEANS FOR FOUR GROUPS ON THREE MEASURES OF PRODUCTION OF PLOT-TITLES

| Groups | *N* | Number of titles | Cleverness-rating | Number of high-quality titles |
|---|---|---|---|---|
| Judgment-Clever | 23 | 12.0 | 2.55 | 2.52 |
| Judgment-Appropriate | 22 | 13.0 | 2.43 | 2.73 |
| Reading-Clever | 23 | 14.7 | 2.42 | 2.52 |
| Reading-Appropriate | 24 | 14.8 | 2.35 | 2.25 |

*Results.* The 1254 titles were typed on separte sheets, randomized, and given to the two expert judges for independent rating on a 5-point scale. The agreement between the judges, computed over single titles, is represented by a correlation-coefficient of 0.35. Three scores were calculated for each *S*: number of titles written, mean rating of the cleverness of these titles, and number of titles of high quality. The agreement between the judges, computed over means of *Ss*, is represented by a correlation-coefficient of 0.64. Since the ratings of both judges were used, the effective reliability is somewhat above this value. The criterion of high quality was 7 or higher for the sum of the two judges' ratings of cleverness.

It is clear from Table I that the differences between the groups are negligible. The only statement that can be made at the 5% level of confidence is that the two groups with practice in judgment wrote fewer titles than the two groups with practice in reading. This finding raises the question of whether the practice in judging was effective, and a comparison of the first and second halves of the judging-practice in respect to agreement with the experts showed that both groups moved in the expected direction, but that neither difference approached significance.

## THE INFLUENCE OF PRODUCTION ON JUDGMENT

The hypothesis that practice in production will improve the accuracy of judgment may lie behind the assumption that book reviewers and editors should have some experience in writing. The effort to write to meet a certain criterion, such as cleverness, might improve ability to recognize cleverness when one sees it. Current doctrine on transfer suggests, however, that judgment would not be improved as much by practice in production as by practice in judgment.

*Procedures.* Three groups were used, named according to the sequence of activities: 'Judgment-Judgment' (*J-J*.), 'Judgment-Production-Judgment' (*J-P-J*), and 'Production-Judgment' (*P-J*). The materials were the same as in the first experiment. In all groups, the last task was the reading of a plot and judgment of a standard series of 25 titles. The *J-J* group first had practice in judgment just as in the first experiment. The *J-P-J* group also had practice in judgment first, followed by practice in production, and then the standard judging task. The *P-J* group had the same practice in production, then the standard judging task. The two plots of the first experiment were used for judgment, but for production the *J-P-J* and *P-J* groups were given a similar plot supplied by Professor Guilford. These *S*s were instructed to write clever titles, but the output was not analyzed.

Half the *S*s in each group received the Gilson plot for the final judgment and half the Durden plot. The other plot was used for the initial judgment in the *J-J* and *J-P-J* groups. Three college classes formed the three groups, and, since the results were encouraging, three more classes were run in the same way.

*Results.* With the ratings of the expert judges as the standard, errors of judgment were computed as the difference between *S*'s judgment and that of the experts. The errors were summed without regard to sign to get a measure of total error for each *S*. These scores could range from 0-80, and a total error of 25 would be an average of one point on the 5-point scale for 25 judgments. The two classes in each group gave similar results and therefore were combined. To check on the equivalence of the two plots, the two subgroups that judged the Gilson plot first ($N = 85$) and the two that judged the Durden plot first ($N = 86$) were compared as to total error. Since the difference was small and far from significant, error-scores for the two plots were combined, and the mean of the errors on the initial judgments (34.9) taken as the baseline against which to evaluate the effects of practice. Three other comparisons also indicate that the two plots did not differ on final judgments.

The results after these consolidations are shown in Table II. It is evident that practice in judgment did not significantly improve judgment. The *J-P-J* sequence improved the final judgments significantly, but the greatest improvement, that of the *P-J* group, is due to production alone. Furthermore, the final judgments of the *P-J* group are significantly more accurate at the 5% level than those of the *J-J* group.

## DISCUSSION

The results demonstrate that, with suitable materials, two intellectual activities, previously treated separately, can meaningfully be investigated together. What these two activities have in common in the present experiments is not the overt response but an abstract dimension or quality; namely, cleverness, which is a specification of the titles to be produced and the basis of judgment of the titles to be rated.

There are several possible reasons why the expected difference due to instructions did not appear. The exact wording of the instructions may be important, hence the critical portions of the instructions for the first experiment are quoted above. Another consideration is that each *S* of the present experiment had only one set of instructions, while Christensen and co-workers used both instructions for the same *S*s, and the latter technique may maximize the difference in their effects. In the present experiment, furthermore, all *S*s had some kind of practice before beginning production, and it is possible that this practice minimized the effect of differences in instructions.

TABLE II

ERRORS ON INITIAL AND FINAL JUDGMENTS OF PLOT-TITLES FOR THREE GROUPS WITH DIFFERENT PROGRAMS OF PRACTICE

| | *N* | Mean error | Diff. | *P* |
|---|---|---|---|---|
| Initial judgments (J-J and J-P-J) | 171 | 34.9 | | |
| Final judgments (J-J) | 89 | 33.4 | 1.5 | n.s. |
| Final judgments (J-P-J) | 82 | 31.8 | 3.1 | .05 |
| Final judgments (*P-J*) | 86 | 29.7 | 5.2 | .01 |

The striking fact to be explained is that practice in production improved the accuracy of judgment, while practice in judgment did not improve either production or judgment. The most obvious explanation, consistent with other evidence on learning, is that production is a more active kind of practice. The periods of practice for both activities were equally short (about 12 min.) but *S* probably has to think more actively to write a title, even a poor one, than to write a number from 1-5. It is also possible, since the hypothesis of the first experiment was not supported, to retain the assumption of brainstorming that practice in judgment engenders a critical attitude that interferes with improvement. The difference between the *J-P-J* group and the *P-J* group is consistent with this assumption. Another possibility is that, in selecting a task which is a good measure of individual differences in originality, we have selected one that resists improvement. Christensen and co-workers found no improvement in plot-titles scored for cleverness over a 12-min. period of production. Meadow and Parnes found improvement on several tests of production, including plot-titles scored for number of low-quality titles, after a course in Creative Problem Solving, but the improvement in plot-titles scored for high quality did not reach significance.[8] The findings of the present experiments may not apply to other tasks in which the factor of originality is less prominent.

If we ask what is learned and transferred from production to judgment, the answer cannot be "a simple habit," for the writing of titles is quite different from the writing of numbers in blank spaces. On the basis of research in military training. Gagné has questioned the familiar principle that direct practice on a task is the best way to learn it and has emphasized the importance of analysis of the task for the design of training sequences that will maximize transfer.[9] The component of production that transfers to judgment in the present experiment is most likely the common dimension of both tasks—the quality of cleverness. This concept is familiar to college students, of course, but the practice in production could sharpen this concept and permit it to be used with more discrimination. Like some of the military research cited by Gagné, the present study shows that to improve in judgment, one should practice something else.

## SUMMARY

The relation of production and judgment was studied in this experiment. Plot-titles were used because they afford a good test of originality, and because they are suitable for both production and judgment. The hypotheses tested were that practice in judgment improves production and that practice in production improves judgment.

College students read short plots and practiced judging the cleverness of titles for them, comparing their judgments with the judgments of experts. Then they read other plots and wrote titles for them. As compared with control groups whose members merely read the titles, there was no

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[8] Arnold Meadow and S. J. Parnes, Evaluation of training in creative problem solving, *J. appl. Psychol.*, 43, 1959, 189-194.

[9] R. M. Gagné, Military training and principles of learning, *Amer. Psychologist*, 17, 1962, 83-91.

superiority of production due to the practice in judging. Nor were instructions to write clever titles effective, as compared with instructions to write appropriate titles.

Other *S*s practiced writing titles and then judged the cleverness of a standard series of titles, which had been judged by experts. This practice improved the accuracy of judgment. A group with direct practice in judgment did not improve in judgment. Another group with practice in judgment prior to practice in production improved only slightly in judgment, possibly because of the inhibitory effects of the initial practice in judging.

These findings lead to the conclusion that it is feasible to investigate relations between intellectual activities that are grossly different. In respect to transfer, the conclusion is that it is the concept of cleverness and skill in its identification that transfer. Accuracy in judging titles is improved, not by direct practice in judgment, but by practice in the production of titles to meet the criterion involved in the subsequent judgment.

# PRACTICE-EFFECTS AND THE ESTIMATION OF ADAPTATION-LEVEL

By WILLIAM W. RAMBO and ELMER L. JOHNSON
Oklahoma State University

The literature on absolute judgment contains frequent reference to the role played by learning, but there have been very few reports of a quantitative nature concerning the development over trials of stable scales of judgment. A convenient first approach to this problem might be to select a single index to represent the distribution of judgments rather than to attempt a description of the changes taking place along the entire scale. Adaptation-level (*AL*) theory should serve this purpose, since *AL* estimates are derived from the entire distribution of stimuli and responses.[1]

Helson recognizes the role of learning in the formation of a stable *AL*, expressing this process in terms of the attenuation of the relative contribution of past-experience variables and a corresponding increase in the contribution of the immediate stimulating circumstances.[2] Helson's theory does, therefore, provide explanatory constructs capable of handling the transitional phases of judgmental stabilization. If, however, one accepts the notion of *AL*s systematically changing over trials, the *AL* to be expected on any given trial cannot be anticipated from the quantitative dimension of the theory.

The purpose of this paper is to trace the development of absolute judgments, represented by *AL*-estimates, over trials. Two experiments will be reported in which *AL*-estimates obtained from judgments of numerousness were plotted over a series of 45 trials and equations were determined which reflected the change in *AL* with practice.

*Method: (1) Subjects.* The *S*s used in the first experiment were 108 undergraduates enrolled in sections of Introductory Psychology. Approximately 60% of the *S*s were women.

*(2) Materials.* The stimuli used in this study were groups of dots 7/32 in. in diameter which were drawn in black India ink on a 3½ × 5½-in. white field. The patterning of the dots was unsystematic. The complete stimulus-series contained nine

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[1] Harry Helson, Adaptation-level, in Sigmund Koch (ed.), *Psychology: A Study of a Science*, v. 1, 1959, 565-621.

[2] Helson, *op. cit.*, 595.

cards on which appeared 5, 7, 10, 14, 19, 26, 36, 49, or 68 dots. For each deck of cards, each of the 9-dot groups was reproduced in 5 different patterns, thus yielding a deck containing 45 cards.

*(3) Procedure.* One of the difficulties associated with a study of this type lies in the definition of a single trial. Since stabilization of the judgmental scale takes place at a fairly rapid pace,[3] much of the learning might be obscured if one trial were to be defined as a complete presentation of the stimulus-series. One solution to this problem might be to define a trial as the presentation of a single member of the stimulus-series, and then by completely counterbalancing presentation sequences in a Latin-square arrangement it would be possible to sum across groups on any trial and derive an estimate of *AL*. Computing *AL* across groups would yield an estimate of *AL* each time a new card was presented. The confounding difficulties associated with this design were appreciated, but it was considered necessary in order to avoid the insensitivity of a unit of analysis which included all responses made to a complete presentation of the stimulus-series. Since the primary purpose of the research was to select a function that would account for the regression of *AL* on trials, it was assumed that the type of function obtained would remain unchanged over variations in the sequence of presentation defined by the Latin-square arrangement. This assumption, as we shall see later, received support from the data.

The *S*s were randomly assigned to nine groups, and each group received a different order of presentation; hence, when the *i*th card was presented, each group received a different card, and the group as a whole received the entire stimulus-series. There were 12 *S*s in each group.

The *S*s were brought into the experimental room and were asked to look into the viewing aperture of a Tachitron model tachistoscope. This viewing tube was mounted in a 36 × 33-in. partition which obscured *E* and the experimental materials from the direct view of *S*. The *S*s judged along a 9-category scale the numerousness of the dot groups which were exposed for 0.1 sec. All groups received five complete presentations of the stimulus-series, thereby requiring that they make 45 judgments. For each group, the order of presentation assigned that series was replicated five times. The *S*s were provided with booklets which contained one scale for each judgment they were asked to make, and they were told to respond by checking the category which most closely corresponded with their judgment. Three of the categories, the center and each extreme, were appropriately labeled *very large, very small,* and *average*. *S*s were given two practice-trials on which groups of numbers were exposed. No judgments were recorded for these trials.

*(4) Replication.* A replication of the above experiment was performed on a new group of 108 undergraduate *S*s, 75% of whom were women. Minor changes were made in the stimulus-series in that the nine cards now included groups of 5, 7, 10, 13, 18, 24, 32, 45, and 65. The geometric mean of this series is 17.85. With a few minor exceptions, the procedure followed in this replication was identical with the previous procedure. Now *S*s were given only seven response-alternatives, and since order was again determined randomly within the restrictions imposed by the completely counterbalanced design, the order of presentation was modified.

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[3] M. E. Tresselt and J. Volkmann, The production of uniform opinion by nonsocial stimulation, *J. abnorm. soc. Psychol.*, 37, 1943, 234-243; Tresselt, The influence of amount of practice upon the formation of a scale of judgment, *J. exp. Psychol.*, 37, 1947, 251-260.

*Results*. Techniques for estimating *AL*-values are outlined in some detail by Guilford.[4] Responses were quantified by arbitrarily assigning weights beginning with 1 to the *very-small* category and progressing in equal increments to the *very-large* category. Then, for each cell in the design, the average judgment for the stimulus identified by that cell was computed. Next, moving across groups, for each trial a straight line was fitted to the regression of the average judgments on the log of the stimulus-magnitudes. Values for *AL* were obtained from this regression line by solving for the stimulus-magnitude predicted to generate a judgment having the value assigned to the *average* category.

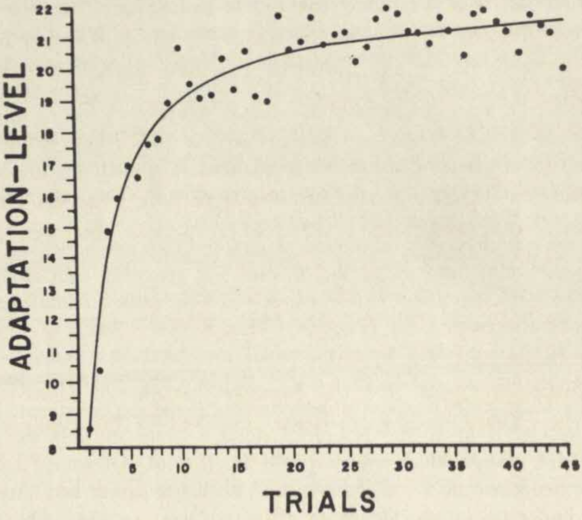

FIG. 1. HYPERBOLIC FUNCTION FITTED TO DATA-POINTS FOR EXPERIMENT I

The data-points in Fig. 1 represent the results of this analysis for the first experiment. Inspection of this figure reveals that although there appear to be marked changes in the *AL*-values, the trend of the data is decidedly non-linear. Attempts were made to select a function which would accurately reflect the trend of these data, and it was decided that a hyperbolic function of the form $AL = T/(a + bT)$, where $T$ represents trials, might yield an expression that would accommodate the results of the experiment. To test this hypothesis, both *AL*-estimates and trials were transformed into reciprocals, and the linear regression of $1/AL$ on $1/T$ was computed. An acceptable least-squares fit for these transformed data would indicate a hyperbolic function as an acceptable choice for the data. The equation resulting from these computations was $1/AL = (0.0451 + 0.0755)/T$, which, when the data were transformed back into their original form, resulted in the equation: $AL = T/(0.0755 + 0.0451\ T)$

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[4] J. P. Guilford, *Psychometric Methods,* 2nd. ed., 1954, 331-333.

Inspection of Fig. 1 will indicate that the hyperbolic function appears to fit the data quite well. Since there was only one observation (*AL*) for each value of *T*, it was impossible to extract sums-of-squares components that would permit a test of significance of the component explainable by the regression-function, but, as an index of goodness-of-fit, the coefficient of determination was computed for the reduction-line that was obtained from the reciprocals. This coefficient was 0.97, which is rather impressive support for the linear fit, and indirectly for the hyperbolic function.[5] The slope-constant 0.0755 in the equation for the reduction-line was tested for significance, and a *t*-value of 11.17 indicated a deviation away from zero that was significant beyond the 0.1% level, with $n - 2\ df$.

As an index of scatter away from the hyperbolic function, the *S.E.* of estimate was computed, and a value of 0.71 was obtained. Although within the context of the present investigation no criterion for acceptance or rejection exists for this estimate of error, this value seems to permit the acceptance of the hyperbolic function. This *S.E.* was 3.6% of the magnitude of the mean predicted *AL*-value.

The ordinate-asymptote for the hyperbolic curve is defined by $1/b$, which, in the case of the present study, was 22.17. This asymptotic value will be referred to as the terminal adaptation-level (*TAL*), and it will be defined as the theoretical value associated with the upper limit of *AL* in a given judgmental situation. Theoretically, it is at this value that *AL* will completely stabilize. A crude estimate of the extent to which a scale has achieved stability may be obtained from a comparison of the *AL*-estimates based on any trial or series of trials and the *TAL*.

The analysis of the data obtained from the replication experiment proceeded along lines similar to those just reported. When *AL* values were now plotted against trials, the result was quite similar to that shown in Fig. 1, which led to an attempt to fit a hyperbola to the data. A straight line was fitted to the reciprocals of *AL* and *T*, and the resulting equation was: $1/AL = (0.0472 + 0.0483)/T$. The index of determination computed from the relationship between $1/AL$ and $1/T$ was a substantial 0.93, and a $t$-value of 7.49 with $n - 2$ *df.* again demonstrated a regression coefficient which differed from zero beyond the 1% level. Here, then, once again was support for the hyperbolic function describing the successive change in *AL* with practice.

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[5] The reader probably is aware that the line fitted to the reciprocal transformations conforms to least-squares criteria. The transformation back into the original data does not however, yield a function that conforms to a least-squares fit. In the present situation, the deviations from a least-squares fit are expected to be minor (Don Lewis, *Quantitative Methods in Psychology*, 1960, 55).

Transforming to the original scale resulted in the equation $AL = T/(0.0483 + 0.0472\ T)$, and computing the *s.e.* of estimate for this equation yielded a value of 0.87 which was somewhat higher than the estimate derived from the first experiment. The value for the ordinate-asymptote (*TAL*) was expected to be somewhat lower for this set of data compared with that of the previous experiment, since the geometric mean of the stimulus-series was lowered from 18.77 in the first experiment to 17.85 in the second. Consistent with expectations, the *TAL*-estimate for these data was 21.19 compared with 22.17 for the first set of data.

It appears reasonably safe to conclude that, within the experimental conditions herein described, the transition of *AL*-estimates toward a stable value follows a negatively accelerated function of the form $AL = T/(a + bT)$. This conclusion is consistent with the findings of Tresselt and Volkmann, and of Tresselt, which indicate that it is during the very early trials that there is rapid movement toward a stable judgment-scale.[6] Johnson has also reported a hyperbolic equation describing the shifting of a judgmental scale following modification of the judgmental series.[7] The equation was in the form of a weighted average which includes predicted limens before and after the shift in the stimulus-series. Johnson reports some success with this function in an experiment which required *S*s to make judgments of pitch.

## SUMMARY

Two experiments have been reported in which adaptation-level (*AL*) estimates were plotted over a series of 45 judgments of numerousness. In both experiments, curve-fitting procedures yielded an equation of the general form, $AL = T/(a + bT)$, where $T$ represents trials, to describe the regression of *AL* on trials. The asymptotic values for these equations were used to estimate a terminal adaptation-level (*TAL*), an estimate of *AL* which, theoretically, has stabilized as a function of practice. The distance between the *TAL* and the geometric mean of the series remained essentially the same in both experiments.

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[6] Tresselt and Volkmann, *op. cit.,* 239.

[7] D. M. Johnson, Learning function for a change in the scale of judgment, *J. exp. Psychol.,* 1949, 39, 851-860.

# THE EFFECTS OF SENSORY DEPRIVATION AND SOCIAL ISOLATION ON THE PERFORMANCE OF AN UNSTRUCTURED COGNITIVE TASK

By PETER SUEDFELD, ROBERT J. GRISSOM, and JACK VERNON
Princeton University

Although *S*s in most sensory deprivation studies have reported difficulty in concentrating, in maintaining and organizing connected trains of thought, and so forth,[1] investigators have seldom found conclusive evidence of cognitive decrements as measured by problem-solving, learning, and reasoning tasks.[2]

Two factors appear to be responsible for this inconsistency. One is the use of inappropriate tests of cognitive functioning—that is, tasks in which relatively brief periods of effort are required, clear (and often overlearned) manipulations must be made, and an easily identified 'answer' terminates the problem-solving process.[3] Such tasks may require very different abilities from those needed for tasks which are relatively unstructured and of long duration, involve an indeterminate number and great variety of possible manipulations, and have no readily identifiable terminus. These latter characteristics are typical of the self-imposed problems which *S*s have found difficult to solve during sensory deprivation. The second factor which may have led to the inconsistency of previous experimental findings is the diversity of the procedures subsumed under the titles of 'sensory deprivation,' 'perceptual isolation,' and other variants.[4] Many studies have inadvertently incorporated features which reduced the degree of sensory deprivation or affected *S*s' emotional response to the experimental situation. Among these contaminating features have been: interruptions of the experimental condition for eating, toilet-needs, and test-

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* Received for publication June 22, 1962. The Princeton project on sensory deprivation was supported by the Office of the Surgeon General, U.S. Army, Contract DA-49-007-MO-671.

[1] P. E. Kubzansky, The effects of reduced environmental stimulation on human behavior: A review, in A. D. Biderman and H. Zimmer (eds.), *The Manipulation of Human Behavior*, 1961, 51-95.

[2] *Ibid.*, 51-95.

[3] L. Goldberger and R. R. Holt, Experimental interference with reality contact (perceptual isolation): Method and group results, *J. nerv. ment. Dis.*, 127, 1958, 99-112.

[4] Kubzansky, *op. cit.*, 55.

ing; frequent stimulation of the *S*, in the form of communication between him and *E* and the administration of multiple-test batteries; the knowledge that someone is constantly monitoring the confinement chamber, which may detract many from the feeling of isolation; and the knowledge of control *Ss* that they were in fact controls.

The present study was planned to ascertain whether empirical support could be given to subjective reports of cognitive impairment. The task used to measure cognitive efficiency—inventing a story based upon a verbally presented scene—was minimally structured; and environmental stimulation during the confinement period was reduced to an unusually low level. It was predicted that sensory deprivation would result in cognitive impairment. Since the instructions emphasized maximal detail and dramatization—and consequently length—in the response, lowered verbal output would indicate the existence of such a decrement, particularly since the reduction would be contrary to predictions based upon motivational factors (*i.e.* the uniformly demonstrated desire of deprived *S*s to maintain and to intensify stimulation[5]). Difficulty in organizing speech, indicated by reduced speech-rate, also was predicted for these *S*s. Socially isolated but not sensorily deprived *S*s were expected to increase the length of their responses as a result of the operation of the 'social drive.'[6]

*Subjects.* Thirty undergraduate men were used as *S*s.

*Procedure.* Three to seven days before confinement, each *S* was given the initial 'test of imagination' consisting of a verbally presented *TAT*-like scene about which he was asked to invent as dramatic and detailed a story as he could, describing the past, present and future events involved, the thoughts and emotions of each character, and so forth. Responses, given while *E* was absent from the room, were recorded on tape. Ten *S*s were then randomly assigned to each of the three treatment-groups. These were:

*(1) Sensory deprivation and isolation (SD).* The sensorily deprived and isolated *S*s were confined for 24 hr. in a floating chamber, 9 × 4½ × 8 ft. This chamber effected an 80-db. sound-loss, and was completely dark. It contained a bed, a chemical toilet, and a urine jar. A urinator strapped to the body enabled *S*s to urinate without moving from a supine position. A bland liquid food, and water, were available to the *S* through plastic tubes reached merely by turning his head on the pillow. The *S*s were instructed to defecate prior to reporting for confinement, and none left the bed for this purpose during the experiment. Instructions to *S*s emphasized that they were not to sit up on, nor leave, the bed, and that they were to refrain from making noise of any sort except in reply to further instructions. They were warned that they would be monitored intermittently and released without payment if found to be violating these rules.

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[5] *Ibid.*, 83-84.

[6] J. L. Gewirtz and D. M. Baer, The effects of brief social deprivation on behaviors for a social reinforcer, *J. abnorm. soc. Psychol.*, 56, 1958, 49-56.

*(2) Social isolation only (SI).* The *Ss* in the isolation-only group were confined for 24 hr. in another chamber, 9 × 5½ × 7 ft. This chamber effected a sound-loss of approximately 25 db., and was illuminated by a 100-w. bulb. The *Ss* were provided with sandwiches, chocolate-bars, and water. The chamber was furnished in the same way as the *SD*-cubicle, but *Ss* had to leave the bed to use the urine jar. They were told that they could move around the chamber at will, and that they could sing, whistle, or talk to themselves.

*(3) Control (C).* The control *Ss* were dismissed and instructed to return 24 hr. later for final testing.

No *S* was informed of the conditions of his confinement until he reported for the experimental session, nor was he made aware of the existence of the other treatment-groups.

When *SD*- and *SI-Ss* reported for the confinement-session, they were familiarized with the cubicle, and informed that *E* would be in the vicinity of the laboratory only at infrequent intervals. They were shown how to release themselves from confinement if they desired to terminate the experiment prior to the scheduled release.

The final item of instruction to all *Ss* was that near the end of the experimental period (3-6 hr. before release for the confined groups) they would hear a description of a scene, about which they were to generate a story. Previous instructions regarding the story were repeated, and *Ss* were tested until they were completely familiar with these instructions. It was emphasized (a) that the stimulus-tape would present only the scene, without repetition of the instructions, and (b) that the scene would not be the same as that presented at the initial testing session.

The stimulus, lasting 20 sec., was presented to each *S* 24 hr. after confinement (or after temporary dismissal, in the case of the *C*-group), and the responses were recorded on tape. The reason for the false information previously given regarding the timing of this presentation was to avoid having the *S* shorten his response in order to be released sooner. During the presentation of the stimulus and the recording of the response, the chambers remained in the same condition as in the experimental period. *C-Ss* were given this final task in the *SD*-chamber, which was illuminated during the test-period for half of this group.

Immediately upon finishing their responses, confined *Ss* were asked to repeat the instructions concerning their stories, and to estimate the time remaining until release. All responded in a way which showed that they expected to be confined for several more hours, and that they had remembered the instructions adequately.

Before final dismissal, all *Ss* signed a pledge in accordance with the University honor-code to the effect that they would not divulge any details of their experience without express permission from the reserach-staff. The importance of this prohibition in preventing the contamination of future findings was made clear and emphasized by *E*.

*Stimulus-scenes.* The two stimulus-scenes were designed to be similar in structure, simple enough for adequate verbal presentation, and general enough for a variety of details, interpretations, and outcomes to be perceived as fitting responses. The scenes were:

(A) A man is running along a road. Another man is running behind him, waving his hand, while a third man watches them both from behind a tree.

(B) A man is standing inside a phone booth. Another man is knocking on the door of the booth, while a third man stands by, watching.

The order of presentation of the scenes was counter-balanced in all treatment groups.

*Results.* Table I shows the change in mean number of words per story between the initial and final tests. The *C*-group showed no significant change in length of story; the *SI*-group evidenced a significant increase in number of words; while the final stories of the *SD*-group were significantly shorter than their initial stories. Of the 10 *SD-S*s, 9 showed a decrease in story length, while only 5 *C*- and 4 *SI-S*s gave shorter final than initial stories. The initial difference between the stories of the *SD*- and the *C*-groups was not significant ($t = 0.49$), but those of the *SI*-group were significantly shorter than those of the *C*-group ($t = 2.72$, $p < 0.05$).

TABLE I

CHANGES IN MEAN STORY-LENGTH AND MEAN SPEECH-RATE BETWEEN INITIAL AND FINAL TESTS

| | Group | | |
|---|---|---|---|
| Story length (number of words) | *C* | *SD* | *SI* |
| Initial | 1521 | 1881 | 526 |
| Final | 1442 | 1299 | 818 |
| Diff. | −79 | −582 | 292 |
| *t* | 0.41 | 6.30 | 2.43 |
| *p* | — | 0.0005* | 0.025* |
| Speech rate (words per min.) | | | |
| Initial | 164.5 | 240.5 | 161.0 |
| Final | 150.0 | 177.8 | 142.3 |
| Diff. | −14.5 | −62.7 | −18.7 |
| *t* | 1.82 | 5.37 | 1.36 |
| *p* | — | 0.0005* | —* |

* One-tailed.

Changes in speech-rate, measured in words per minute, also are presented in Table I. Although the speech-rate on the final test was lower than on the preliminary test for all groups, the difference reached significance only in the *SD*-group. Although the initial speech-rate of the *C*- and *SI*-groups was approximately the same, the *SD*-group spoke significantly more rapidly than the *C*-group ($t = 3.70$, $p < 0.01$). This significant difference between the speech rates of the two groups disappeared on the final test ($t = 1.76$, $p > 0.10$).

Initial-to-final-test changes did not reflect significant differences between the A-B and the B-A orders of stimulus-presentation in any group.

*Discussion.* In light of the instructions that stories be as detailed (and by implication, as long) as possible, the increased productivity of the

*SI*-group was consistent with theories of the isolation-caused arousal of a 'social drive' and a desire to comply with instructions to elicit the approval of *E*.[7] Yet, these same motivational factors, with the further addition of the intense "stimulus-action hunger"[8] of the sensorily deprived individual, failed to produce similarly lengthened responses in the *SD* group. The highly significant reduction in both the speech-rate and the response-length of the *S*s is firm evidence for earlier introspective reports of the cognitive impairment produced by sensory deprivation. The fact that previous investigators have found but little evidence for this phenomenon may be attributed to their use either of less appropriate measures of cognitive efficiency or of experimental treatments which did not sufficiently reduce the environmental stimulation experienced by their *S*s. Initial differences among the groups, due to the uneven distribution of a few exceptional individuals, constitute a contaminating factor which could be avoided in future replications by the use of matched groups. This chance occurrence seems inadequate, however, to explain the striking and complete support of the hypotheses.

*Summary.* An open-ended projective technique was employed to evaluate cognitive impairment as a result of social isolation and severe sensory deprivation. After 24 hr. of confinement, 10 isolated and sensorily deprived *S*s showed a significant decrease in the length of stories generated in response to verbal descriptions of a *TAT*-like scene; 10 isolated but not deprived *S*s showed significant increases in story-length; while 10 control *S*s showed no significant change. All groups evidenced a decrease in speech-rate, but only the isolated-deprived group changed significantly.

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[7] *Ibid.*, 49-56.

[8] J. C. Lilly, Mental effects of reduction of ordinary levels of physical stimuli on intact, healthy persons, *Psychiat. res. Rep. Amer. Psychiat. Assn.*, 5, 1956, 1-28.

# EFFECT OF MEANINGFUL RELATION ON LEARNING AND RETENTION OF PAIRED-ASSOCIATES

By V. K. KOTHURKAR, University of Poona, India

It has long been known that learning is more efficient and retention is better when the materials learned are meaningful. In attempts to ascertain the relative importance of meaning of each member of every pair, earlier investigators have approached the problem from both sides of the paired terms—from the side of the stimulus and from the side of the response. Though the weight of the experimental evidence supports the primacy of the meaningfulness of the response-term,[1] results supporting the importance of the meaningfulness of the stimulus-term have also been obtained.[2] Indeed, it is not unusual to have attention drawn to the facilitative influence of meaning of both stimulus and response. A common feature of all these studies is their preoccupation with the meaning of one or the other or of both the terms to the almost complete neglect of the meaningfulness of the relation between the two terms.

It is possible that words of equal meaning-value (*m*) may subtend relations of greater or less meaningfulness. It is also possible to speak of the greater or lesser meaningfulness of the relation itself as distinct from the meaningfulness of the terms. Two pairs of words may be equally meaningful, and yet differ greatly in the meaningfulness of the connection between the words forming the pairs. This meaningfulness of the relation, as distinguished from the meaningfulness of the terms, may also influence paired-associate learning and retention. The meaningfulness of the relation itself could be an important variable determining the ease or difficulty of paired-associate learning.

The purpose of the present study is to explore the role of the meaningfulness of the S–R relation itself in the acquisition and retention of paired-

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* Received for publication October 5, 1962.

[1] V. J. Cieutat, F. E. Stockwell, and C. E. Noble, The interaction of ability and amount of practice with stimulus and response meaningfulness (*m*, *m*') in paired-associate learning, *J. exp. Psychol.*, 56, 1958, 193-202; R. G. Hunt, Meaningfulness and articulation of stimulus and response in paired-associate learning and stimulus recall, *ibid.*, 57, 1959, 262-267; C. E. Noble and D. A. McNeely, The role of meaningfulness (*m*) in paired-associate verbal learning, *ibid.*, 53, 1957, 16-22.

[2] G. A. Kimble and R. H. Dufort, Meaningfulness and isolation as factors in verbal learning, *J. exp. Psychol.*, 50, 1955, 361-368.

associate materials. Our hypothesis is that, other things being equal, the more meaningful the relation between the S–R pairs, the quicker the terms will be learned and the better they will be remembered.

*Method.* The usual procedure of the paired-associate method was used. Every *S* was given the same number of trials (eight) and learning was scored in terms of the number of items of each kind that were correctly reproduced in the block of eight trials. To overcome variability among the *Ss*, the experiment was designed in such a way that every *S* learned the same materials under identical conditions. Thus, every *S* served as his own control.

*Materials.* A list of 16 paired-associates, composed of four levels of meaningfulness of the relation between the pairs, was prepared. The 16 pairs were randomized in four sequences. The four levels of meaningfulness of relations were decided upon and defined operationally. Two bases of classifying relations according to their meaningfulness were employed—one, logically; the other, empirically. Four of the pairs had the relation of logical entailment existing between the members, and they were selected from the subtest of "Logical Selection" in the Terman-McNemar test of mental ability.[3] The second group of four pairs did not have such relation of logical entailment among their members. The empirical basis of classifying relations was provided by the Kent-Rosanoff tables of associated frequencies.[4] Thus, the third group of four pairs consisted of response-words of an average communality of 117 with respect to the stimulus-words, while the response-members of the last group of four pairs had zero average communality. The four groups of four pairs of words were almost identical with respect to (a) $m'$ values and (b) other structural properties like number of syllables in the words. The $m'$ values were mean association-frequencies of a group of 25 postgraduate students, similar in socio-economic and cultural background to the students who participated in the experiment. The average $m'$-values of the stimulus- and response-sides of the four groups of pairs were Logical Relations, $S(7.72) - R(7.68)$; Absence of Logical Relations, $S(7.43) - R(7.09)$; High Communality, $S(7.92) - R(6.55)$; and Zero Communality, $S(7.91) - R(7.60)$.

A practice list of four pairs was used to acquaint the *Ss* with the apparatus and the procedure.

*Subjects.* Eighty graduate students participated in this experiment on a voluntary basis. They were randomly assigned to four groups of 20 each for the purpose of studying retention at four different time-intervals.

*Procedure.* A memory-drum was used for presenting the materials. The *Ss* learned by the anticipation method, except for the first learning trial, in which both words of the pairs were exposed simultaneously for 3 sec. After the exposure of the list, the stimulus-word of each pair was presented for 3 sec., during which *S* was required to anticipate and produce the associated response-words, and then the stimulus- and the response-words of the pair appeared together for another 3 sec. The learning materials were presented in four randomized sequences in the order

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[3] L. M. Terman and Quin McNemar, *Terman-McNemar test of Mental Ability; Forms C and D for grades 7 to 12, 1942.*

[4] G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insan.*, 67, 1910, 317-320.

A BA BC DCD. There was a pause of 10 sec. after the first learning trial and a rest of 1 min. after every two runs through the list.

The four groups of Ss were tested for recall 24 hr., 96 hr., 1 week, and 28 days, respectively, after learning.

*Results:* (1) *Learning.* Learning was measured in terms of the number of response-words correctly reproduced. Thus, with 4 pairs in each group and 8 trials, the maximal score an *S* could obtain for a group of relations was 32. The mean scores for the four principal experimental groups of Logical Relations, absence of Logical Relations, High Communality, and Zero Communality relations were 24.75, 12.46, 24.61, and 14.03 respectively. As expected, superior learning occurred under experimental conditions where the response-words were either logically related to, or had high communality with the stimulus-words. Probably, the *S*s did perceive that certain pairs of words were more intimately and organically related than certain others. All in all, learning was more efficient for verbal materials that were either inherently related or related through frequency of experience. The difference between the mean learning-scores of the Logically Related and Unrelated pairs was greater than the difference between the mean learning-scores of High and Zero Communality pairs. Analysis of variance revealed very highly significant *F*-ratio indicating that the experimental variations in the meaningfulness of relations produced significant, systematic variations in learning scores. The obtained *F*-value of 493.22 was highly significant with a probability much less than 0.01.

*Retention.* The mean retention-scores (the number of items correctly recalled in each group of four paired associates) of the principal experimental groups of Logical Relations, Absence of Logical Relations, High Communality, and Zero Communality were 3.13, 1.65, 3.18, and 2.05, respectively, the maximal retention-score being four. Analysis of variance in two criteria-classifications revealed that variations in time-interval and, more importantly, the experimental variations in the meaningfulness of relations carry with them systematic variations in recall scores. *F*-values of 19.35 and 42.94 were obtained for time intervals and variations in the meaningfulness of relations respectively. Both *F*-ratios were significant beyond the 1% level.

*Discussion.* The main results of this experiment amply confirm the hypothesis that the more meaningfully related S–R pairs are, the better are they learned and remembered. The meaningfulness of relations, as such, carries with it a distinct advantage for learning and retention. This negates a purely elementaristic and a crudely connectionist interpretation of paired-associate learning and points to the need of recognizing the role of integrating and unifying relations in it. Things do not get linked up mechanically through sheer frequency of juxtaposition, but they are organized into meaningful patterns of relatedness.

Now, did the *Ss* perceive that some pairs had more significant and vital ties between their members than others? It is our guess that the *Ss* were sensitive to the differences in meaningfulness of relations. In the present experiment, however, the need to control the perceptual awareness of relations was continuously felt. Perception of relations is an important determinant of associative learning. Of the myriad relations, a given *S* may selectively perceive only some or some kinds of them depending upon his purpose or point of view. It is suggested that Woodworth's concept of reinforcement of perception should be extended to perception of relations.[5] Extending the principle of reinforcement of perception to cover perception of relations, involves more than mere extrapolation of the concept to 'new' material. The principle covers the phenomena both of external and of internal 'built-in' reinforcement. Not only is perception of certain relations or certain kinds of relations reinforced from without, but also there is internal reinforcement when words are perceived as being relevant to one another. The more or less intimately related paired-words function as reinforcers when they are perceived as satisfying needs of relevance, logical involvement, good organization, and what Bartlett calls "effort after meaning."[6] Perception of relation can be its own reinforcement.

Thus, perception of more or less meaningful relations facilitates learning, while it is itself an act of learning. This is in line with the revealing analysis of Solley and Murphy that a psychology of perception implies a psychology of learning, and contrariwise.[7] This study provides an experimental confirmation of the view that perception and learning are closely related. Between highly meaningfully related pairs of words, there is greater internal or self-reinforcement of the associative response, and hence, their learning and retention are facilitated.

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[5] R. S. Woodworth, Reënforcement of perception, this JOURNAL, 60, 1947, 119-124.

[6] F. C. Bartlett, *Remembering*, 1932, 20.

[7] C. M. Solley and Gardner Murphy, *Development of the Perceptual World*, 1960, 13.

# PREDICTIVE STABILITY OF THE HIGHER ORDERED METRIC SCALE

By DONALD N. FARRER, Washington State University

Previous studies on risk and probability have shown that an individual is likely to risk one dollar to obtain one dollar on a bet with a probability of 1/2, such as the toss of a fair coin; but an individual is not likely to risk one hundred dollars to obtain one thousand dollars with a probability of 1/10.[1]

Such behavior, which is at variance with objective probability, has been demonstrated by Hurst and Siegel to be consistent for individuals with intelligent quotients (*IQ*s) above 100.[2] Studies with his "higher-ordered metric scale of utility" indicates that this procedure makes predictions of a person's selections between alternative bets more accurately than predictions based on objective, statistical probability. As an example, assume that an individual is offered a choice between two alternatives: (1) A 50-50 chance of obtaining $100.00 with the risk of obtaining $0.00 or, (2) $40.00 for sure. A prediction of the individual's choice based on objective probability assumes the selection of (1) because the objective expected outcome is $10.00 greater than alternative (2). As a description of behavior, however, it is logical to assume that an individual might not wish to risk the 0.5 probability of getting $0.00 with the first alternative. The higher ordered metric scale of utility was designed to predict selections of alternate choices according to the subjective utility function of a given individual. Predictions can be made if the individual is consistent and transitive in all choices offered. This scale has been applied to utiles such as books,[3] cigarettes,[4] grades,[5] social distance,[6] and level of aspiration.[7]

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* Received for publication December 4, 1962. The author of this study is now at 6571st Aeromedical Research Laboratory, Holloman AFB, New Mexico.

[1] This statement is an example of the famous St. Petersburg paradox which emphasizes the point that people will not behave in accord with the objective monetary expected value of a gamble. A review of the research on the discrepancies between statistical probability and subjective utility is found in Ward Edwards, The theory of decision making, *Psychol. Bull.*, 51, 1954, 380-417.

[2] Sidney Siegel, A method for obtaining an ordered metric scale, *Psychometrika*, 21, 1956, 207-216; P. M. Hurst and Sidney Siegel, Predictions of decisions from a higher-ordered metric scale of utility, *J. exp. Psychol.*, 52, 1956, 138-144.

[3] Siegel, *op. cit.*, 1956, 213.

[4] Hurst and Siegel, *op. cit.*, 1956, 138.

[5] S. W. Becker and Sidney Siegel, Utility of grades: Level of aspiration in a decision theory content, *J. exp. Psychol.*, 55, 1958, 81 f.

[6] Personal communication.

[7] S. W. Becker and Sidney Siegel, Utility and level of aspiration, this JOURNAL, 75, 1962, 115-120.

If the subjective value of the commodity is constant for a given individual, then the higher ordered metric scale could be used not only at the time the scale is constructed, but also at a later date. The purpose of this study is to investigate the stability of predictions over one month with people above and below 100 IQ.

*Method: Subjects.* Twenty randomly selected young men (16 to 18 yr. old) who were in an institution for delinquents served in the study. Each *S* was asked two questions: "Do you smoke?" and "Would you like an opportunity to obtain some free cigarettes?" A negative response to these questions eliminated the person as a participant in the study. All initially selected answered both questions affirmatively and participated in the experiment.

*Apparatus.* All of the sessions were conducted individually and in the same room at the institution. A popular brand of king-sized cigarettes was used as the commodity. Two nonsense-syllables of low associative value (ZOJ and WUH; ZEJ and XEQ) were equally distributed on the six surfaces of each of the two dice. A dice shaker, felt pad, and a backstop were used to determined outcomes of the choice-situations. Pencil and paper were furnished each *S* so that he could record each of the 15 choice-situations. The list of the alternatives used is reported in Siegel.[8]

*Procedure.* Two sessions separated by a period of one month were conducted with each *S*, and the same 15 offers were presented on both occasions. Each *S* was given 5 packs of king-sized cigarettes at the beginning of each session and informed that the total number of cigarettes he could obtain was dependant upon his ability to choose the best alternatives and his luck with the dice. All the choices were recorded before the outcome was known. After the first session, a higher-ordered metric scale was constructed for each *S* using some of the 15 choices made by each *S*. (Of the total 15 offers presented to each *S* the minimum used in the construction of a scale was 5 and the maximum was 9.) The offers not used to construct the scale were called surplus offers. Surplus offers for each *S* varied from 10 to 6 depending upon the number needed to construct the scale. *S*'s scale was then used to predict the decisions that he made on those surplus offers. The scale constructed after this first session for each *S* was used to predict the choices that each would make on the same surplus offers on the second session, one month later.

*Results.* For each *S* one higher-ordered metric scale was constructed from the information obtained on the first session and predictions were made for the surplus items of both testing sessions. The number of errors of predictions for the first session was compared to the number of errors of predictions on the second session. A total of 166 predictions was made on both the first and the second administrations. Of the total predictions there were 34 errors of predictions on the first administration. The mean number of errors in predictions was 1.7. Of the same 166 predictions made on the second administration there were 32 errors in predictions; the mean number of errors in prediction was 1.6. The Wilcoxon test was used to test for significant differences in errors of predictions.[9] A $T$-value of 59 was obtained which is less than the 0.05 level of confidence with an $N$ of 15. (Five *S*s had *d*s of 0.) The matched-pairs $t$-test applied to these data indicated a $t$-value less than 1.00. This analysis supports the conclusion that there were no significant differences in errors of predictions over the one month time span using the original higher-ordered metric scale to make the predictions.

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[8] Hurst and Siegel, *op. cit.*, 1956, 138.

The *S*s were divided into two groups: above and below mean *IQ*. (The mean Revised Beta *IQ* was 101.4 and the range was from 78 to 118.) The mean number of errors of predictions for the group above the mean *IQ* was 1.54 on the first administration and 1.36 on the second administration. The mean number of errors of predictions for the group below the mean *IQ* was 1.88 on the first administration and 1.88 on the second administration. These data suggest that the lower *IQ* group is as stable as the higher *IQ* group.

*Discussion.* The Hurst and Siegel study demonstrated the predictive ability of the higher-ordered metric scale of utility to be more accurate than predictions based on an objective value model,[10] and the present study indicates that the subjective utility scale remains stable when used with adolescent boys. The comparison of the mean errors of predictions of decisions by the two *IQ* groups gave no indication that lower *IQ* boys made more contradictory or intransitive decisions than those made by the higher *IQ* boys. The assumption that the *S*s with the lower *IQ* might not have the understanding necessary to complete the task was not supported by these results.

## SUMMARY

This study examined the predictive accuracy of the higher-ordered metric scale of utility with 20 young men (16–18 yr. old). A subjective utility scale was constructed for each *S* and predictions of decisions on the surplus offers were made. These predictions were used one month later on the second administration of the utility scale. A comparison of the errors in predicting the decisions of the first administration with those during the second administration indicated subjective utility as measured by this scale does not change during the one month between administrations. The decision making ability of the *S*s with *IQ*s below the group mean was demonstrated to be as stable as the decisions made by the *S*s whose *IQ*s were above the group mean.

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[9] Siegel, *Nonparametric Statistics*, 1956, 75.
[10] Hurst and Siegel, *op. cit.*, 1956, 138.

# APPARATUS

## A DEVICE FOR THE MEASURING OF TRACKING PERFORMANCES

By RUDOLF G. MORTIMER, ROBERT E. RUSSELL, and JOSEPH TIFFIN
Purdue University

Photoelectric cells have a wide range of application in programming events and scoring responses in behavioral studies, both animal and human. The use of these cells for switching has numerous advantages over other methods: Mechanical pressure is not required, they are silent, rapid, flexible in mounting, and small in size.

A transistorized circuit which responds in a binary fashion to the change in conductivity of a photoelectric cell is described here and shown in Fig. 1. The trigger of the photocell is a Schmidt trigger directly coupled to the amplifier of the photocell.[1] It operates in a nonsaturated mode for the most rapid, possible response. The cycle of operation is essentially as follows: Assume that the photocell is being so activated by a source of light that its resistance is decreased. This increases the base-current of $T_1$, which, in turn, increases the collector-current of $T_1$ and decreases the voltage at the base of $T_2$. This decreases the collector-current of $T_2$ increasing the base-voltage of $T_3$ which takes over conduction from $T_2$ via the feedback through the common emitter-resistor. $T_2$ is cut off while $T_3$ is conducting approximately 8 ma. causing the relay to be energized, and to remain energized until the light impinging on the photocell decreases sufficiently to cause the base-voltage of $T_2$ to rise above cut-off. The collector-current begins to flow in $T_2$, decreasing the base voltage of $T_3$ which rapidly tends toward the cut-off, as conduction of $T_2$ is reinforced by positive feedback until the $T_3$ cut-off is reached.

Trigger-time is approximately 50 µsec., reset-time being slightly longer due to the inductance of the relay. In practice, trigger- and reset-time are limited by the recovery-time of the photocell.

To calibrate the circuit the control of sensitivity (500 K pot.) is set to maximal sensitivity (minimal resistance) with the photocell receiving the maximal light. The threshold (50 K pot.) is then set slightly beyond the pull-in point of the relay. The control of sensitivity is then set to a point just before the drop-out of the relay. These settings are made easily with the aid of a voltmeter connected across the collector of $T_1$ and the ground.

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* The apparatus described here was used in research on stimulated night driving which was supported by the Public Health Service, Division of Accident Prevention, Research Grant AC-87. R. E. Russell is responsible for the design of the circuit.

[1] General Electric Company, *Transistor Manual,* 5th ed., 1961, 122.

The amplifier- and trigger-circuit of the photocell were designed as part of a scoring system to measure tracking accuracy in a study concerned with the effect of various conditions of illumination in a task simulating night driving. The dependent variable is the integrated distance-time error in tracking. The display is a simulated gray roadway 3 in. in width attached to a wide, continuous, black conveyor-belt moving in front of *S* who controls the cursor by means of a steering wheel. The cursor houses the sensing elements of the scoring system. The other elements are the trigger-circuit and power supply, the scoring integrator, and the read-out meter. This system is shown in Fig. 2.

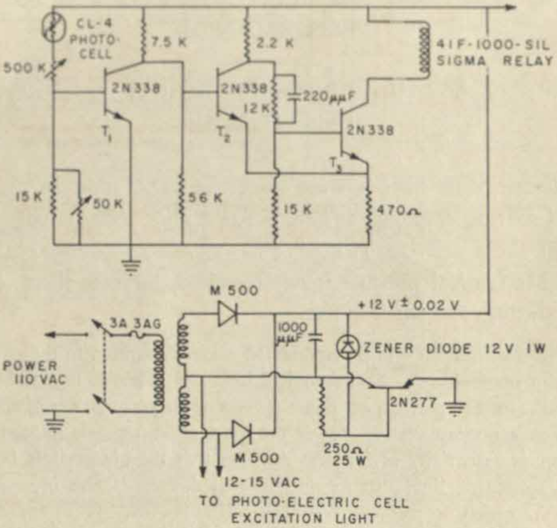

FIG. 1. CIRCUIT DIAGRAM OF TRIGGER AND POWER-SUPPLY

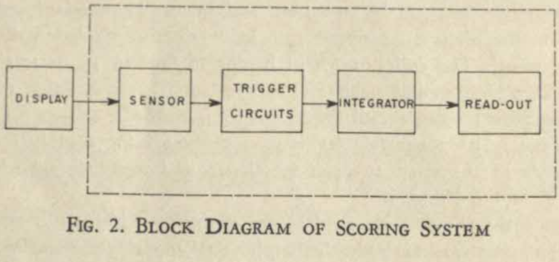

FIG. 2. BLOCK DIAGRAM OF SCORING SYSTEM

The front of the cursor carries five photoelectric cells with their sensitive faces vertically above the moving track. Two 12-v. bulbs are placed 1 in. farther back in the cursor and their light is allowed to illuminate the track through a slit cut in the base of the cursor. This arrangement is shown in Fig. 3. The geometry of this system is such as to permit the light from the bulbs to be reflected from the track on to the faces of the photocells.

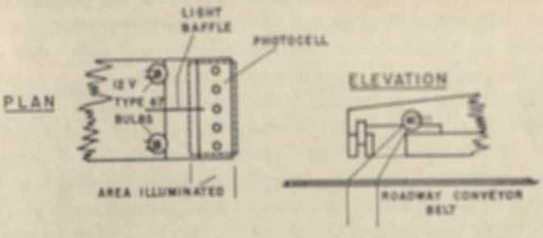

FIG. 3. FRONT OF CURSOR SHOWING ARRANGEMENT OF PHOTOCELLS AND LIGHT-SOURCE

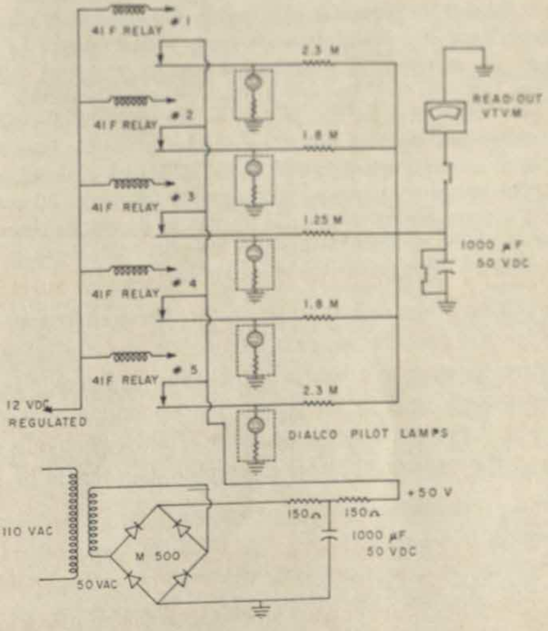

FIG. 4. CIRCUIT DIAGRAM OF SCORING INTEGRATOR

It will be apparent that the amount of light reaching the photocells will be dependent upon whether the light is reflected from the gray roadway (reflectance 27%) or the black conveyor belt (reflectance 9%). When an error in tracking takes place such that one or more of the photocells is riding above the black rather than gray surface a change in the resistance (conductivity) of the photocell occurs and it is this change which acts as the stimulus to the trigger-circuit. Five trigger-circuits are employed, one for each photocell-sensor.

Each trigger-circuit, receiving inputs from its sensor-unit, controls a Sigma relay which forms a part of the integrator shown in Fig. 4. It will be noted that as each relay is sequentially activated, depending upon the tracking error at a given instant, the resistors become effectively switched into parallel with each other so that the rate at which the capacitor is charged is changed. The values of the resistances have been chosen to give a linear increase or decrease in the rate of charge relative to error in tracking. For the specific circuit-values shown in Fig. 4, this relationship is: 1:2:3:4:5 for the five circuits employed. The linear relationship of charge-rate with tracking-error has obvious implications for the normality assumption in parametric, statistical tests, and in this respect care has also been taken to ensure that error-score can only accumulate on the linear portion of the charge-curve of the capacitor.

The final unit consists of a vacuum-tube voltmeter which is momentarily connected across the capacitor at the end of a trial. The accumulated voltage is used as the indicator of performance and directly reflects the integrated distance-time error.

It will be noted that the integration employed by this apparatus is not continuous but is discrete in terms of distance-increments. In the present instance the integration is in five steps. By increasing the number of sensors and associated units the integrative stepwise function would be made more continuous. The decision concerning the number of channels required is dependent on the specific application.

The device described has been used for many hours and has been faultless in operation. Further, there is almost no drift in the calibration of the trigger-circuits. Calibration is checked prior to data-collection each day following a 30-min. warm-up period only as a precautionary measure. The action of the scoring channels during data-collection can be so monitored by means of the pilot lights shown in Fig. 4 that the malfunction of the scoring system can be quickly detected.

While we have illustrated the use of the photocell trigger-circuit in tracking, it is clear that the same circuit may be used for other purposes where a binary response to a light which changes in intensity is required. For the components used in the circuit, the minimal relative change in intensity was 3:1. For smaller changes, more sensitive photocells should be employed and the number of stages in amplification should be increased.

# AN AC-AMPLIFICATION SYSTEM FOR RECORDING EYEBLINKS AND OTHER MOVEMENTS

By Kenneth P. Goodrich, Joseph Markowitz, and
Donald A. Norman, University of Pennsylvania

Students of the conditioned blink-response typically record the momentary position of *S*'s eyelid by a system in which the eyelid is coupled to the shaft of a precision-potentiometer and the resulting changes in resistance are recorded on an oscillograph.[1] The DC-amplifiers usually employed between the potentiometer and the oscillograph present problems of instability, drift, and inadequate gain. The present paper describes an alternative AC-system which should be of considerable usefulness in work with the eyeblink and wherever small, rapid movements must be recorded.

The elements of this system are shown in Fig. 1. An audio-oscillator is used to provide a small constant AC-potential across a bridge network. One side of the bridge consists of the response-potentiometer (*R2*). As the position of the tap on this potentiometer is changed by *S*'s responses, the potential between the tap and ground changes proportionately. This potential change is amplified by an inexpensive audio-amplifier. The AC-output is rectified and smoothed through a half-wave rectifier (D) and condenser (C), and the resulting DC-voltage is applied to the pen-motor of the oscillograph.

The isolation-transformer (T) may be eliminated if both sides of the output of the oscillator are isolated from ground internally. Potentiometer R1 serves to balance the bridge relative to the 'baseline' position of the tap on the response-potentiometer (see below). The sensitivity of the system is adjusted by the gain-control of the oscillator or amplifier. Addition of a milliammeter in series with the pen-motor and a fixed voltage-divider which can be switched into the bridge in place of the response-potentiometer makes possible rapid calibration of the gain and balance of the system. Resistor R3, in conjunction with the parallel impedance of the pen-motor, functions to match the output-impedance of the amplifier. In this particular application, an R3 of 750 Ω and a pen-motor of 1500 Ω bring about an approximate match with the 500 Ω output-impedance of the amplifier.

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* This work was supported in part by Grants G-14725 and GB-203 from the National Science Foundation.

[1] See, for example, K. W. Spence and Janet Taylor, Anxiety and strength of the UCS as determiners of the amount of eyelid conditioning, *J. exp. Psychol.*, 42, 1951, 183-188; and D. A. Grant, L. M. Schipper, and B. M. Ross, Effect of intertrial interval during acquisition on extinction of the conditioned eyelid response following partial reinforcement, *ibid.*, 44, 1952, 203-210.

A possible disadvantage of the system as described is that it is an absolute-value detector. That is, the final DC-potential is the same polarity for a change in the response-potentiometer in either direction from the zero balance-point. This limitation could be overcome by substituting a phase-sensitive detector for the rectifier. It is important to note, however, that such a change is not required in most applications. If all possible positions of the response-potentiometer are on the same side of the balance-point of the bridge, there is no question about the direction of response. As used in eyeblink-conditioning, for example, the balance-potentiometer is so set that at the highest possible position of the eyelid there is a slight DC-imbalance in the direction of that produced by a blink. Thus, all possible positions of the eyelid produce imbalance in the same direction, and the pen of the recorder faithfully follows the momentary position of the lid. The same principle would seem to apply to most applications of this system.

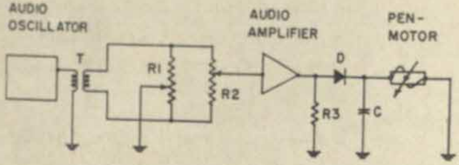

FIG. 1. DIAGRAM OF THE APPARATUS
*T*, audio-transformer; *R*1, balance-potentiometer (1000 Ω); *R*2, response-potentiometer (1000 Ω); *R*3, resistor (750 Ω); *D*, Silicon rectifier (IN2486); *C*, capacitor (40 MFD., 50 VDC).

The advantages of the AC-system are many in comparison with the DC-arrangement usually employed. It eliminates or minimizes the problems of instability, drift, and inadequate gain mentioned previously. Oscillators and inexpensive audio-amplifiers are readily available, relatively trouble-free, and easily serviced. Construction is facilitated by the fact that circuit-values are not critical. The particular values given in Fig. 1 describe the system discussed here, but considerable latitude is possible in the choice of values of resistance, capacitance, and AC-frequency. Another advantage lies in the longer life to be expected from the response-potentiometer, since the AC of the present system does not cause the arcing-damage produced with DC-current.

# A PROJECTION COLOR MIXER

By LORRIN A. RIGGS, Brown University

Laboratory and classroom demonstrations of color vision usually require a number of separate pieces of equipment. The conventional color-wheels, colorimeters, color blindness test-plates, and combinations of projectors are cumbersome and are of limited effectiveness as demonstrations for large classes. The purpose of this note is to describe the construction of a relatively simple device that provides additive color of sufficient intensity to be used on a screen. It is a single portable instrument that demonstrates the principal facts of color vision. It may also be used as a group-test for the various forms of color blindness. It has been constructed from commercially available components.*

## DESIGN

The major problem in constructing a color-projection system is to provide sufficient intensity of light. The present device solves this problem by accurately focusing an image of the projection-bulb filament in the plane of the color filters. The optical principle involved is that, in an ideal projector, light from any part of the filament image reaches uniformly every part of the projected field on the screen. Hence, the entire filament image, divided in variable proportions among three primary color filters, is used to achieve a homogeneous field of additive color mixture.

Fig. 1 shows the circular field of light that is projected onto a conventional screen. The right semicircle contains the additive mixture of red, green, and blue primaries. The left semicircle is used to present light from a color sample. The sum of the primaries in the color-mixing field remains approximately constant, but the relative proportions of each are variable to achieve a hue-match, as in certain visual colorimeters.[1] A brightness-match is achieved by varying the intensity of the sample field. The result is, in effect, to make available on a screen the color-

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* This equipment was developed under grant G-17676 from the National Science Foundation. It was assembled in the Hunter Laboratory of Psychology by Mr. C. DeLucia.

[1] See, for example: H. E. Ives, A color-match photometer for illuminants, *J. opt. Soc. Amer.*, 7, 1923, 243-261; Adam Hilger, Ltd., Tri-chromatic colorimeter, *J. scient. Instruments*, 13, 1936, 199-201; R. W. Burnham, A colorimeter for research in color perception, this JOURNAL, 65, 1952, 603-608; W. D. Wright, *The Measurement of Color*, 1958, ix, 1-263. The present arrangement of color filters is most similar to those of Burnham and Wright. Colorimeters make use of integrating spheres, diffusing plates, or other means of combining the beams through the various filters. These procedures result in a loss of a large percentage of the light so that they are not suitable for use by more than one person at a time.

matching fields that are usually seen only within the eyepiece of such an instrument as a colorimeter or an anomaloscope.

*Projection-equipment.* Two simple 2 × 2-in. slide-projectors are used to provide patches of light on a screen. Figs. 2–4 show the basic equipment. The color-mixing field is produced by the projector shown on the right in Fig. 2. An adjustable color filter mount, shown in Fig. 3, contains red, green, and blue filters through which the light is projected.

Focusing the projection-bulb filament in the plane of the filters is accomplished by removing the condensing lenses from the projector and inserting two achromatic lenses into the tube of the projection-lens, as shown in Fig. 2. The filament image then falls approximately 2.5 in. from the front surface of the projection-lens and the filters are mounted in such a way that the coils of the filament are seen sharply in focus on them. Light from any point in this image-plane must then fall equally on every part of the color-mixing field, which therefore appears homogeneous in color on the screen in front of the projectors.

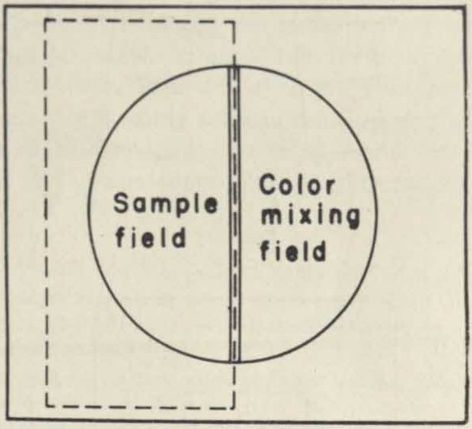

FIG. 1. APPEARANCE OF THE COLOR FIELDS ON THE SCREEN
(Dashed line indicates position for inserting a colored paper sample.)

The color-mixing field has the semicircular shape shown in Fig. 1. This field is the image of a semicircular aperture, ¾-in. in diameter, in a diaphragm that is mounted between the slide holder and the lenses of the projector. The relative amounts of red, green, and blue light in the color-mixing field are controlled by the proportion of filament image located within the corresponding filter. The filters themselves are pieces of dyed gelatine, bound, as in a 2 × 2-in. color slide, between two pieces of cover-glass plates. This filter slide is cemented to a square diaphragm of sheet metal with a 1-in square aperture. The diaphragm is mounted on a microscope mechanical stage, as shown in detail in Fig. 3. Separate knobs control the horizontal and vertical positions of the diaphragm, each of which can be read to an accuracy of 0.1 mm. on a vernier scale. In this way, the filter slide can be so positioned that the light passes through one, two, or all three of the filters in any desired proportion.

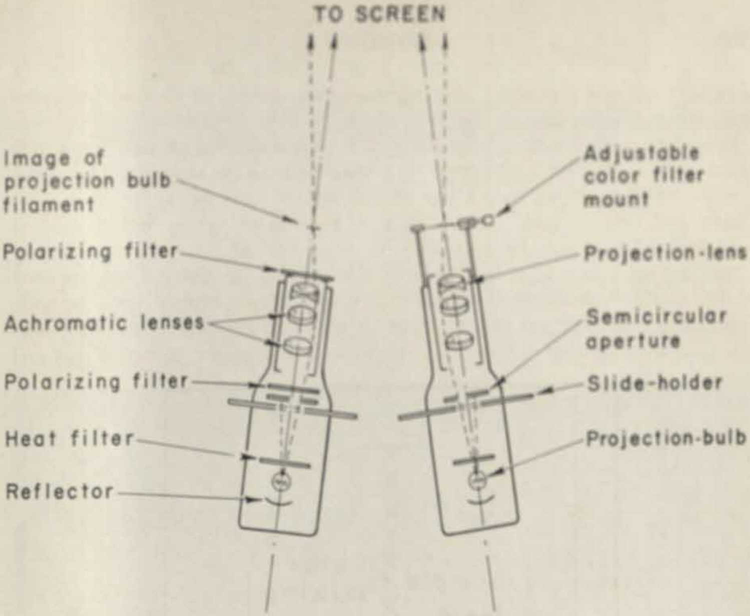

FIG. 2. DIAGRAM OF THE PROJECTION EQUIPMENT

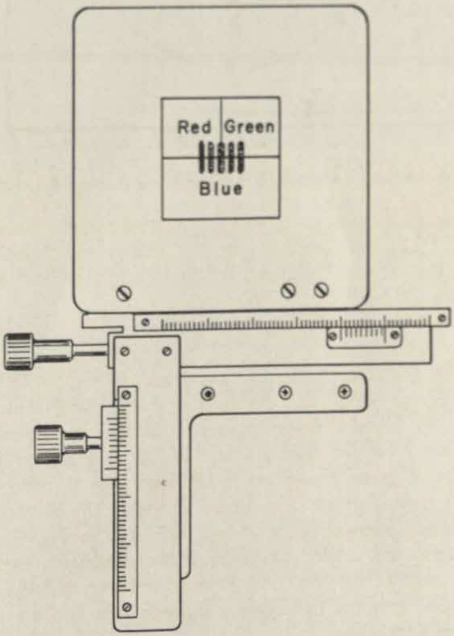

FIG. 3. THE COLOR-FILTER ASSEMBLY SHOWING IMAGE OF THE TUNGSTEN FILAMENT

A judicious choice of color filters provides three primaries whose additive mixture covers a large part of the chromaticity domain. Filters of equal luminous transmittance (relative to tungsten light) yield mixtures whose luminance remains approximately constant, irrespective of chromaticity. This is because the entire filament image is always being utilized to produce the color-mixing field.

The color-sample field is produced by the projector on the left in Fig. 2. This field consists of a second semicircle of light whose luminance is controlled by the use of a pair of polarizing filters. One of these is mounted on the barrel of the projector. Rotation of the barrel can therefore be used to vary the transmission of light that has passed through the other polarizing filter that is fixed to the main housing of the projector. In other respects, this projector is similar to the one used for the color-mixing field; it has a semicircular aperture and is similarly equipped with achromatic lenses.

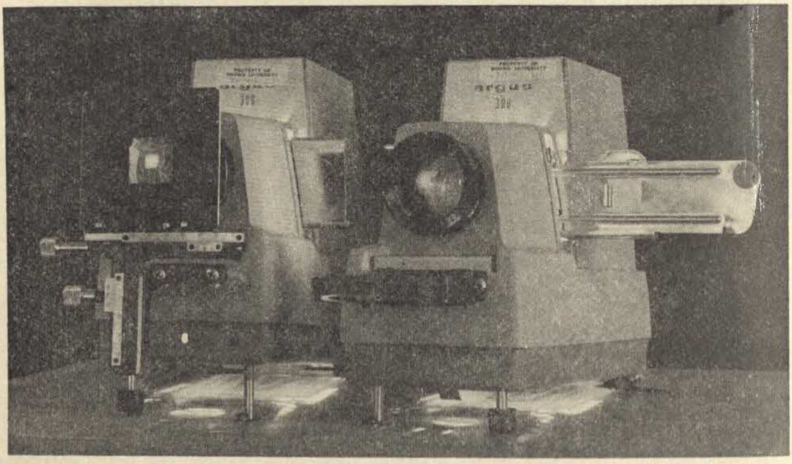

FIG. 4. THE PROJECTION COLOR MIXER

Each of several possible functions of the instrument is achieved by placing an appropriate filter in the slide holder of the sample-field projector. A yellow filter, for example, is inserted when the device is to be used as an anomaloscope. In this case, the yellow sample field is matched by a mixture of red and green light in the color-mixing field. More generally, a filter of any color may be inserted into the sample-field projector. A colorimetric match is then achieved by a selected combination of the red, green, and blue lights in the color-mixture field. Still another procedure is to place a piece of colored paper or other reflective sample in the position indicated by dotted lines in Fig. 3. In this case, the sample is illuminated by white light from the color-sample projector and a colorimetric match is again found by an appropriate combination of colors in the color-mixing field. In all these uses, the brightness of the color-sample field is adjusted to approximate that of the color-mixing field by rotating the polarizing filter.

### PROCEDURES FOR USE IN THE CLASSROOM

*Color mixture.* The principal facts of color mixture can be demonstrated with the color-mixture field. Thus, the two knobs can be manipulated in such a way as to present the red, green, and blue primaries successively on the screen. Any two primaries can then be combined to produce intermediate hues, reference being made to a diagram such as a hue circle or CIE chromaticity chart. Finally, the three can be combined to produce a white. This white can be matched by a white field of the proper brightness from the other projector. Reference can be made to the trichromatic theory of color vision.

*Complementary colors.* Three procedures can be employed for demonstrating complementary colors. First, the mixing field can be left in adjustment to produce a white, as before. Rotation of the elevation knob then causes the mixture to become more blue or more yellow. The range from blue through white to yellow can be used to illustrate the fact that complementaries are diametrically opposite one another on the color diagram and neutralize one another to produce white. Saturation can also be defined in terms of this continuum.

A second procedure for demonstrating complementary colors is by the use of successive contrast. The observers can be asked to fixate the color patch for half-a-minute, after which time the patch is turned off. The resulting negative after-image is of the complementary color. For best results, the screen is partially illuminated with white light. The audience is told to fixate the field as steadily as possible while the colored light is on and to wink frequently after the light is turned off. Several different colors may be used, provided that sufficient time between is allowed for recovery.

A third example of complementaries is in simultaneous contrast. The double shadow technique, suggested by Goethe and developed by Hering, yields colors that are approximately a complementary pair. This is done by moving the projectors back from the screen or else by turning one of the projectors inward. Either way, one obtains a nearly rectangular region of overlap between the inner portions of the two semicircles. An object, such as a pencil, may then be interposed between the screen and the projectors in such a position that two shadows of the object appear within this rectangle on the screen. Thus, one shadow contains only colored light and the other, white light, while the surround is a mixture of white plus color. In this case, the two shadows appear to have complementary colors. The 'white' shadow assumes maximal contrast with the 'colored' shadow even though only a single hue is present on the screen. This example of complementaries may be used to exemplify an opponent-color theory of color vision.

*Color blindness.* Used as an anomaloscope, the mixer becomes a device for demonstrating and identifying the principal forms of color defect. The procedure is to insert a yellow filter into the slide carrier of the color-sample projector and to adjust the color-mixing projector for mixing red and green (without blue). Normal trichromats are then able to find a particular setting of the red-green control knob that gives a good approximation to a match with the yellow sample field. The luminance of the sample field can be so set for a brightness match that the two semicircles appear equal in all respects. The concept of metameric pairs can then be discussed.

True dichromats accept a wide range of hues, from red through orange, yellow, and yellow-green to green, as matching the yellow sample field. Anomalous trichromats, however, have a narrower range of red-green settings for an acceptable hue match. Some of them approach the precision of a normal observer, though the match may appear orange or yellow-green to a normal trichromat.

Protanomalous and protanopic subjects are characterized by low sensitivity to red. They may, therefore, be identified by the fact that the yellow sample field is adjusted to a low level of luminance in attempting a brightness match with an orange or red mixture field.

We may summarize by saying that *normal trichromats* are in rather close agreement on the precise setting of the red-green control knob that yields a hue match with a yellow sample field. They also agree closely on the luminance adjustment of that sample field to equate it with the mixture field. With regard to *anomalous trichromats*, the *deuteranomalous* subtype makes red-green settings outside the narrow yellow region that is acceptable to the normal, but his luminance matching settings are not very different from normal. The *protanomalous* subtype also makes red-green settings outside the normal range and sets the luminance of the sample field to a very low level when attempting a brightness match to an orange or red mixture field. With regard to *dichromats*, a *protanope* is the form in which all settings of the red-green knob provide an acceptable hue match to the yellow. He lacks red sensitivity, so that the yellow must be made very dim to match the brightness of the red. A *deuteranope* also accepts the entire red-green range as a hue match for yellow, but he does not lack red sensitivity. Hence, he uses a yellow field of approximately normal luminance to achieve a brightness match with a red. Rarer forms of color defect, such as tritanopia or monochromatism, can also be illustrated by the device, but there is little likelihood of encountering one of these cases even when the classes are large.

## CONSTRUCTION

The main units of the device are all commercially available at moderate cost. The following list contains the particular units that were selected for the pilot model of the device. The cost for parts was approximately $85.

| *Item* | *Specification* |
|---|---|
| Slide projectors (2) | Argus 300, Model III |
| Polarizing filters (2) | Edmund Scientific Company, Barrington, New Jersey; Catalog No. 70,418 |
| Achromatic lenses (4) | Diameter 44 mm., focal length 189 mm; Edmund; Catalog No. 6,313 |
| Gelatine color filters (4) | Eastman Kodak Red No. 26, Green, No. 61N; Rosco Light Blue, No. 129, Medium Lemon Yellow, No. 3 |
| Mechanical stage for color filter mount | Edmund; Catalog No. 30,058 |

This model of the device projects a circular field of light 10 in. in diameter from a distance of three feet to the screen of an average sized classroom. Resting on a lecture table, it is tilted upward slightly that the class can see over it to the screen.

A second model of the device, for use in a larger lecture room, projects a 14-in. field from a distance of 7 ft. to the screen. This model, costing about $50 more, uses two Bell and Howell Model 724 slide projectors, 500 w. each, in place of the Argus 300 w. projectors. These projectors are modified in the same way, except that a single achromatic lens is used, rather than a pair of them, in the lens barrel of each.

# APPARATUS NOTES

## THE 'FORM' DETECTION-TEST

To supplement previous contributions to the design of psychological tests involving perceptual patterns, the following material was designed.[1] The basic idea was to confront the subject (*S*) with numerous dots, apparently scattered at random over the sheet, and to instruct him to find those dots forming the corners of squares. For this purpose a Squares Detection-Test consisting of a trial-sheet (Fig. 1) and a series of test-sheets, an example of which is shown in Fig. 2, was prepared.

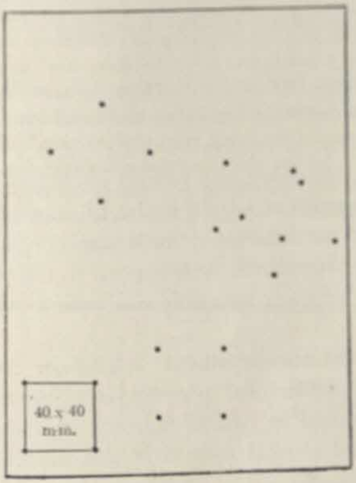

FIG. 1 TRIAL-SHEET.

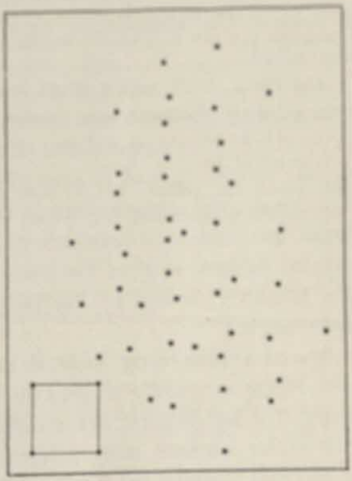

FIG. 2 TEST-SHEET (No. 5).

On each sheet is a printed square (40 × 40 mm.) with a dot at each corner. Forty-four dots are scattered over each test-sheet and so arranged that 11 squares, identical to the printed one, can be found by connecting the dots correctly. On some of the test-sheets (No. 1–3) the squares are isolated, on others (No. 4–6) they overlap, thus increasing the item-difficulty. The sides of the completed squares are either horizontal and vertical or the diagonals are. On some of the test-sheets the sides of the completed squares are at varying angles. There are no irrelevant dots on the first six sheets, but an additional sheet on which no squares can be found, was devised for experimentation.

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[1] Heinz Hector, Results from a simple Gestalt Continuation Test applied to illiterate black mineworkers, *J. Nat. Inst. Personnel Res., Johannesburg*, 8, 1960, 145-147; The PATCO Test, 5th set, *Johannesburg NIRP*, 1961; S. H. Fridjhon, The PATCO Test, symmetry and intelligence, *J. Nat. Inst. Personnel Res., Johannesburg*, 8; 1961, 180-188.

*S* should draw the lines free-hand using a soft pencil. The quality of the drawn lines is not scored. Scores refer to the total number of correct solutions; on Test-Sheet 7 the only correct score is 0. The tester takes the time for each sheet separately. While the test is given untimed at present, the introduction of a time-limit per sheet will have to be considered.

The test can be administered language-free, and for group administration a group demonstration poster, an enlarged copy of the trial sheet, is provided. It is assumed that detection of squares by this method will involve abilities related to perception, reasoning, and general intelligence. The square was chosen since it represents a simple model of inherent parallelism and symmetry, but other versions could make use of geometrical figures, such as the triangle, trapezium, or hexagon. A similar test has been devised by Rey using for each item only one square and one triangle.[2]

Several studies were initiated to test the hypothesis. While the approach was originally devised to implement the program of aptitude-testing for which there is a manifest need in African industry, its fruitfulness for research with children and clinical patients is also evident.

National Institute for Personnel Research — Heinz Hector
Johannesburg, South Africa

## GROUP DEMONSTRATION OF BINOCULAR RIVALRY

The well-known method of producing stereoscopic displays by polarizing material requires placement of filters before the eyes of each viewer. The purpose of this note is to call attention to a source of inexpensive filters which make feasible demonstrations of binocular rivalry and other stereoscopic phenomena to large groups.

The filters consist of plastic polaroid sheets mounted as spectacles in cardboard frames. These glasses are familiar to many persons since their original use for the 3-D movie fad of the early 1950s. Maximal transmission is 30%; minimum, 2%.

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[2] André Rey and J. B. Dupont, Organisation de groupes de points en figures géométriques simples (test des "points à organiser"), *Arch Psychol.*, 34, 1953, 39-65.

Polarization-axes are set at 45°. The frames are large enough to fit over regular glasses and construction is sufficiently rugged to withstand repeated use.[1]

In conjunction with the glasses it is necessary so to polarize the light from each of two projectors that each eye can view only the image emitted by one projector; further, the images must be projected onto a screen which does not depolarize the light. There appear to be no special requirements for projectors, although it is probably desirable to employ units with an output of at least 500 w. Polarization of the projector-beams can be accomplished by placing (in the appropriate orientation) any convenient filter in front of the projector-lenses. Beaded glass projection-screens will depolarize the light, while matte white and lenticular screens will not. Because of its higher reflectance, the lenticular screen is the more desirable. This type of screen, also known as a 'silver screen,' can be identified by the gray appearance of the surface and the presence of closely spaced vertical striations. It is slightly more expensive than beaded glass but can be found at most audio-visual centers.

In demonstrating rivalry phenomena to introductory psychology classes and other naïve groups, the writer has employed 35-mm., color slides of geometric figures and of meaningful stimuli. These slides, projected in coincidence on a lenticular screen, produced quite striking suppression- and combination-effects similar to those obtained in individual stereoscopic viewing.

Vanderbilt University ROBERT FOX

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[1] The supplier of the glasses is Polacoat, Inc., 9750 Conklin Road, Blue Ash, Ohio 45242. Their catalog designation is M1A 3-D Glasses.

# NOTES AND DISCUSSIONS

## ANALYSIS OF A 'RECENTLY DISCOVERED STEREOSCOPIC EFFECT'

It is the purpose of these comments to show a number of relations between White's study of a "recently discovered stereoscopic effect"[1] and established knowledge about binocular depth-perception.

White states that the "inner region" in his stereogram shown in Fig. 1 is seen displaced closer to *O*, but it is apparent from the description that only part of the inner region presented to the left eye corresponds to the inner region presented to the right eye.[2] Ordinarily it is conceived that binocular depth-perception arises from the presentation of congruent or at least identifiably similar patterns to the two eyes, where these patterns exhibit disparity relative to other associated binocular patterns. In some of the stereograms described, the inner region for one eye may have up to 25% of its area that has no duplicate in the corresponding region for the other eye.[3] To see such areas displaced in depth would seem to violate the basic principle of binocular depth-perception. This apparent problem will be resolved by showing that only the corresponding areas of the inner regions are perceived as closer. It will also be shown that the noncorresponding areas are localized in the more remote plane and that this perception is appropriate, and in conformity with the spatial configuration represented by the stereogram.

White further states that convergence, interposition, and linear perspective do not play a part. These comments will propose that convergence can and probably does function just as in viewing other stereograms and objects in real depth, that there is an element of interposition in these stereograms, and that linear perspective may operate, but in a way that is not consonant with the disparity.

To proceed, it is necessary to review the method employed in preparing the type of stereogram under consideration. Basically, as shown in White's Fig. 1, the left-eye view is a random arrangement of white dots in the cells of a 64 × 64 matrix. The right-eye view is identical in the peripheral regions, but in a centrally located

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[1] B. W. White, Stimulus-conditions affecting a recently discovered stereoscopic effect, this JOURNAL, 75, 1962, 411-420.

[2] White, *op. cit.*, 411, and Fig. 1, 412.

[3] White, *op. cit.*, 415.

"inner matrix," 32 × 32, the pattern of dots in each column is shifted one column to the left, and the dot-pattern which would have appeared in the first column is transposed to Column 32. As a result, there are 31 pairs of columns, each pair having the same placement of dots, but exhibiting a one-column positive disparity relative to the background. Column 1 in the left inner matrix is the same as Column 32 in the right inner matrix, but, as will be indicated, this correspondence does not contribute to the resulting depth-perception. These columns, which have no duplicates in the corresponding region of the opposite-eye view, are referred to below as "odd columns." Shifts of more than one column may be introduced, giving an increase in the disparity for the paired columns and in the number of odd columns.

White states that the inner region is seen as closer when the stereogram in his

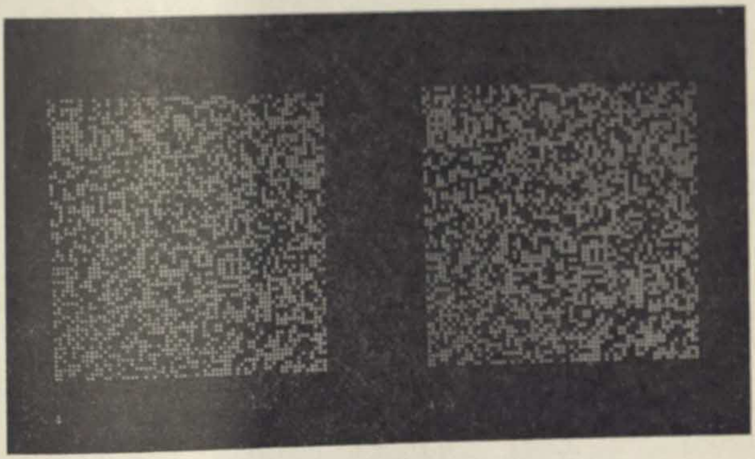

FIG. 1. WHITE'S STEREOGRAM

Fig. 1 is viewed stereoscopically. There is no question that this is true for the perceived area corresponding to the 31 pairs of columns, as would be expected on the basis of established principles. The position of the odd columns is not specifically mentioned, but by implication, at least, they are included by White with the closer region. When White's Fig. 1 is viewed stereoscopically, careful observation reveals that Column 1 (left-eye view, inner matrix) is seen in the plane of the background just beyond the left edge of the closer central area. This same column-pattern, transposed to Column 32 of the inner matrix in the right-eye view, is seen just beyond the right edge of the closer central area, also in the plane of the more remote surround. Thus, the central target that is seen as closer consists only of 31 columns, and this perception arises from the paired areas in the stereogram that have positive disparity relative to the background.

In general, there will be $N - 1$ identical pairs of columns when a one-column shift is used with a central matrix consisting of $N$ columns. If a two-column shift were introduced, there would be $N - 2$ identical pairs, and so on. If it is assumed that *O* is successful in achieving binocular depth-perception, the area seen displaced forward will correspond to the area of the identical pairs.

The number of odd columns in each view will correspond to the magnitude of the shift. The odd columns on each side are presented only in one view. There are no corresponding patterns in the same region of the view presented to the other eye. Analysis of the geometry of the situation reveals that each set of odd columns represents a part of a real depth-presentation seen by one eye but hidden from the other by interposition of the closer central area. These monocularly presented parts of the stereograms are localized precisely and unambiguously in the plane of the more remote surface. Their presence, rather than confusing the situation, contributes to the fidelity of representation and thus, presumably, to the depth-perception. This element of the stereogram constitutes an example of what is known as "Panum's limiting case," where depth is seen in a stereogram when the view for one eye of a second object is omitted, and the geometric and size-relations indicate that the second object lies behind the first and on the same line of sight for one eye.

The correspondence between the type of stereogram under discussion and the real depth-situation that it represents may be clarified by reference to Fig. 2. This is an isometric drawing showing a 1 × 3 matrix out in front of a 3 × 8 matrix. The smaller matrix is displaced in depth and reduced in size just enough that each column as viewed by the right eye is aligned with the next column to the left, as compared to the alignment seen by the left eye. The numerals in any row can represent any pattern whatsoever. The numbering is repeated in the cells of each column only to identify the column, without implying restriction on the variation in pattern. The left-eye view in this situation consists simply of three rows with sequential numbering from 1-8. The extra 3 in the background in Column 6 is not seen by the left eye, since it is blocked off by the cell numbered 6 in the closer matrix. The right-eye view is identical, except that the closer cells, 4, 5, 6 are seen aligned with Columns 3-5 in the background; the 3 in Row 2 of Column 3 is not visible, since it is blocked by the cell numbered 4 in the closer matrix; and the 3 in row 2 of Column 6 is visible. Thus, the right-eye view is the same as the left-eye view except for the one-column shift to the left in the inner region and the transposition of the 3 into Column 6. The stereogram representing the situation pictured in Fig. 2 therefore would have exactly the same general characteristics as the stereogram produced by White's procedure. The "inner region" as defined by White consists of the numerals 3, 4, 5, and 6 in the second row of the stereogram, but it is apparent that the odd columns, indicated by the numeral 3 in each view, represent separate parts of the background.

It may be of passing interest to note that the boundary lines in the schematic diagram in White's Table I,[4] if they are to represent the part of the matrix that will be seen as closer, should be so drawn as to exclude the odd column (numbers 2, 1, 3, and 8) in both views. This would leave three identical pairs of columns, with a one-column positive disparity, that provide the stimulus for the area seen closer to *O*. With this modification, the matrices in Table I constitute, in themselves, an adequate although difficult stereogram.

When the column-shift is in the opposite direction (to the right in the right-eye view), the paired columns have a negative disparity relative to the surround, and the central area is, therefore, seen at a greater distance than the periphery. Now the odd columns are precisely localized in the plane of this more remote central surface, which appears to be seen through an aperture in the surround. The odd columns in the left-eye view occur on the right side of the central matrix, and are seen as the part of the more distant central area that is hidden from the right eye by the closer surround. Similar relations hold for the odd columns on the left of the right-eye view.[5] The numer of identical pairs has the previously

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[4] White, *op. cit.*, 412.

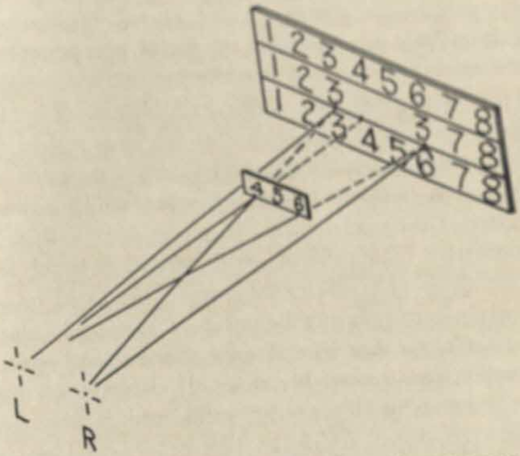

FIG. 2. REAL-DEPTH SITUATION GIVING BINOCULAR STIMULATION SIMILAR TO THAT OBTAINED WITH WHITE'S STEREOGRAM

The points labeled *L* and *R* represent the centers of the entrance-pupils of the two eyes. The lines represent certain chief rays, drawn from *L* or *R* through the centers of the lower edges of selected cells, to indicate the following features: (a) The pattern-units of the closer (inner or central) surface are seen by the left eye as aligned with the correspondingly numbered columns in the background, whereas these closer units are seen by the right eye as aligned with the next column to the left. (b) The cell numbered 3 in Row 2 of Column 3 is visible to the left eye only, and the cell numbered 3 in Row 2 of Column 6 is visible to the right eye only. Broken segments indicate superposition of foreground-cells on background-cells.

An important feature of White's stereogram is the absence of a monocularly defined contour at the edges of the closer surface. This characteristic is not satisfactorily represented in the line-drawing, but could be readily achieved in the actual situation.

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[5] The odd columns are localized in the plane of the more remote central matrix even when they have a pattern like that of the peripheral foreground, and differing from that of the central area. This effect is pointed out by Julesz, and is shown in his Fig. 39 (B. Julesz, Binocular Depth Perception of Computer Generated Patterns, *Bell System Tech. J.*, 39, 1960, 1125-1162. Attention is called to the more recent publication of Julesz (Stereopsis and binocular rivalry of contours, *J. opt. Soc. Amer.*, 1963, 63, 994-999). Julesz initiated the work with random pattern-stereograms, has completed extensive studies, and has presented information pertinent to the present discussion. An extension of these comments, taking Julesz's work more fully into account, is available on request.

described relation to the amount of shift; however, with the addition of the odd columns on both sides, the central area seen at a greater distance has $N + 1$ columns when a one-step shift has been introduced, $N + 2$ with a shift of two columns, and so forth. The aperture will appear to have a width of $N$ columns for all degrees of disparity.[6]

In general, for most of the variations presented by White, the real-depth counterpart of the stereogram can be readily determined. If the binocular information is successfully integrated, the perception is in accord with the physical counterpart that the stereogram represents.

The dot-matrix patterns have a discriminable size of pattern-unit, or 'grain.' White's stereogram represents, then, an inner matrix with a physically smaller pattern-unit, since the unit subtends the same angle at the eye even though it is closer. Ordinarily, when similar patterns are presented at different distances in real space, the grain will have the same physical dimensions, and therefore will subtend a larger angle in the nearer surface. This gives rise to monocular cues based on size and on the progressively increasing misalignment of rows and columns toward the edges of the closer area. The absence of these usual characteristics of the real situation may conceivably make depth-perception more difficult when larger amounts of disparity are introduced, leading to greater incongruity between disparity and the lack of normal linear perspective.

It is paradoxical that abstract stimuli of this sort, with random distribution of elements, designed to avoid so-called secondary or learned cues, inevitably will introduce texture, and, of greater importance, monocularly presented areas in each view. There is no intention of classifying the latter as secondary, but the monocular elements do present a different kind of stimulus in comparison to the paired areas.

With regard to convergence, there seems to be no basis for denying its possible contribution to the organization of response to the type of stereograms under discussion. Whenever there is disparity in a stereogram (or difference in distance in an actual situation) convergence can change, from one set of corresponding stimuli to another. This change serves to bring first one and then another set of corresponding image-patterns onto corresponding areas of the two retinas.

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[6] Because of the method of generating White's stimuli, the odd columns appearing on the left happen to have the same pattern as those appearing on the right, but this correspondence will not ordinarily affect the perception of depth. The same effect could be obtained by substituting any new pattern in the column or columns opened up by a shift, instead of using the patterns of the columns displaced out of the central area by the shift. With White's method, however, a shift of $x$ columns to the left is equivalent to a shift of $N - x$ columns to the right. Conceivably, with large shifts to the left, ambiguity might arise as to which are the odd columns and which are the stereoscopic pairs. The odd columns could be brought into correspondence by a sufficient reduction in convergence. Then the roles of the two sets of columns would be reversed.

Convergence does not have to be exact, but the organization of the information related to disparity or difference in distance is favored by convergence-changes. If the disparity is small enough, as in White's Fig. 1, the organization of the perception of depth may follow immediately, once convergence on one plane or the other is achieved. Depth-perception will not be reported, however, until rather accurate convergence is established.

If the disparity is greater, it may be necessary to shift convergence back and forth several times before a unified perception is achieved. Failure of some of White's *O*s to attain depth-perception with larger amounts of disparity (produced by shifts of several columns) may have been related to the fact that they were not able to, or did not, converge by the amount required to bring the images of the central areas onto corresponding retinal areas. In the dot-matrix patterns, the peripheral areas provide a strong stimulus to convergence and a strong 'fusion hold' by virtue of their relatively highly demarcated borders with the black background. The corresponding central areas have only rather poorly defined internal patterns and no monocularly defined edge-contours. Under these conditions, it may be difficult to shift from accurate convergence on the periphery to accurate convergence on the central area.

Finally, if disparity is too great, it is not possible to organize the visual data into a single binocular response over the whole field. In older terminology, the disparate stimuli fall outside of "Panum's area," which is simply another way of saying that physiological diplopia results if the disparate stimuli are well-defined, or inhibition or rivalry may occur, depending on the stimulus-parameters involved. Nevertheless, depth-perception may still be present, and it is aided by convergence-movements. Convergence is certainly a primary mechanism for slipping "the non-identical parts of the superimposed matrices past each other in search of a displacement which yields a better match."[7] In any case, the possible role of convergence might be considered as an item for investigation.

It is thought that the above analysis will place the phenomena reported by White in a perspective not immediately available to all interested readers. The analysis may also permit more certain identification of that which is new, and thus, perhaps, facilitate interpretation and further study.

C. S. BRIDGMAN

University of Wisconsin

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[7] White, *op. cit.*, 415.

## RETROACTIVE INHIBITION IN INCIDENTAL LEARNING: A REPLICATION WITH NEGATIVE RESULTS

Rosenberg recently reported the results of a study on retroactive inhibition in which he found "that the interfering effects of interpolated learning extend to both intentionally and incidentally learned materials."[1] He used two groups of 20 *S*s each—an Experimental Group and a Control Group. Serving individually, the *S*s of both groups were given three trials of intentionally learning a list of 12 names of common objects, each being accompanied by a different two-digit number (the incidental item). Following this, the *S*s of the Experimental Group were given three trials with another list of 12 names but without accompanying numbers; and the *S*s of the Control Group, a simple puzzle to solve. After the interpolated tasks, the *S*s of each group were asked to recall the words they had first learned and then were given a recognition-test of the incidental numbers that had simultaneously been exposed.

With the exception of the addition of a second Control Group, Rosenberg's study was faithfully replicated.[2] Twenty-one *S*s, drawn from the general student body of The City University of New York, were divided into three groups of seven each: an Experimental Group; Control Group I, and Control Group II. They were assigned to the different groups by chance as they appeared, without reference to sex, major field of study, or college level. All were, however, naïve regarding the subject of this study. None reported eidetic imagery.

The materials and procedure were, throughout, the same as those used by Rosenberg. The only difference between the Control Groups was the difficulty of the interpolated tasks. The *S*s of Control I were given the same puzzle that Rosenberg had used (the "Nine dot" puzzle) and those of Control II were given the "Pig" puzzle—a 15 piece, three dimensional wooden puzzle of a pig.

*Procedure.* Rosenberg's procedure was used. Control II was required to watch the puzzle being taken apart, asked to assemble it, and given a discussion of the solution. The same time periods were used for Control II that Rosenberg used on his Control Group.

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[1] Sheldon Rosenberg, Retroactive inhibition in incidental learning, this JOURNAL, 74, 1961, 283-286, esp. 286.

[2] This study was performed at The City University of New York. The author is grateful to Dr. Gertrude Schmeidler of The City University for her guidance in this study and to Dr. W. C. Wilson of The University of Texas for his assistance in preparing this report.

All *Ss* were told that they had performed well when the experiment was concluded. None of the *Ss* thought he would be tested a fourth time for recall of the original test of words. None of the *Ss* reported having eidetic imagery.

Table I presents the mean number of incidental items recognized by the three groups of *Ss*: The Experimental had a mean of 4.35; Control I a mean of 4.57; and Control II, a mean of 3.70. The results for original learning and the third and final recalls were similar to those of Rosenberg's experiment.

TABLE I

MEAN SCORES FOR RECOGNITION OF INCIDENTAL ITEMS
(N = 7 for each group)

| Group | Recognition | |
|---|---|---|
| | *M.* | *SD* |
| Experimental | 4.35 | 1.27 |
| Control I | 4.57 | 1.39 |
| Control II | 3.70 | .95 |

The Experimental Group vs. Control I for the final recall gave a *t* of 5.30 ($p < 0.0005$), which showed that retroactive inhibition was effected in the Experimental Group. Control I did not do significantly better than the Experimental Group in Recognition, $t = .30$ ($p > 0.10$). Thus, there was a failure to replicate the adverse effect of retroactive inhibition on incidental learning. Control II, however, did significantly worse than the Experimental Group, $t = 1.83$ ($p < 0.05$), in recognition of incidental items. This might indicate that the "Pig" puzzle interfered more in incidental learning than the "Nine-dot" puzzle, or that the extent of incidental learning attainable by *S* appears to be inversely related to the difficulty of the interpolated task. The *t*-test between the two Controls for Recognition yielded a *t* of 1.38 ($p < 0.10$), which is not significant.

Before attempting to deal with the findings of the replication, it might first be advantageous to look at the results that Rosenberg obtained. Rosenberg's Experimental Group had a mean of 2.60 for Recognition, and the Control Group had a mean of 4.55 (which is practically the same value obtained for Control I in the replication). So the recognition of incidental items was significantly greater in Rosenberg's Control than in his Experimental Group, $t = 4.15$ ($p < 0.001$).

In the replication, there was no significant difference between Control I and the Experimental Group, $t = 0.30$ ($p > 0.10$). The expected adverse effect of retroactive inhibition on incidental learning was not realized. Therefore, it is pertinent to point out that the Experimental Group of the replication did significantly better than the one in Rosenberg's experiment, $t = 2.3$ ($p < 0.025$). The failure to replicate retroactive inhibition in incidental learning might indicate that the effect of retroactive inhibition on incidental learning is still undetermined.

The Experimental Group in the replication did significantly better than Control II, $t = 1.83$ ($p < 0.05$). The greater difficulty of the "Pig" puzzle as the interpolated task might be the cause. In any event, the results were consistent with the theory that difficulty of interpolated task has an effect on incidental learning. The area bears further investigation.

University of Texas R. F. PURNELL

## TABLES OF "P LOG$_2$ P"

ARINC Research Corporation, a subsidiary of Aeronautical Radio, Inc., has published a set of tables which simplify greatly the computation of informational measures. This set includes values of $p \log_2 p$ for fractional probabilities with denominators less than 151 and for all three-place decimal probabilities.

These tables are available for free distribution. Address requests for publication No. 384 to ARINC Research Corporation, Marketing and Contracts Office, 1700 K Street, N.W., Washington, D.C. Copies are also available from the Defense Documentation Center. K. M. D.

## ERRATUM

In a paper by L. P. Lipsitt and Trygg Engen (Effects of presentation of paired and single-stimulus on discrimination of length, this JOURNAL, 74, 1961, 274-277), the two sentences, comprising Lines 13 to 18 on p. 277, should read:

*Ss* are more likely to develop such extraneous hypotheses under the *successive* condition than the *simultaneous,* since twice as many slides or stimulus-settings are involved in this condition. It was noted that *Ss* in the learning-experiment tended more under the *successive* condition than the *simultaneous* to select irrelevant cues, such as slight imperfections in the shading or background of the slides. (Italicized words were erroneously inverted in print.) K. M. D.

# BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

*Jan Evangelista Purkinyě*. By VLADISLAV KRUTA and MILKULAS TEICH. Prague State Publishing House, 1962. Pp. 144. No price.

Psychologists know about the Purkyně phenomenon, the way the colors change their respective brightnesses as dawn or twilight advances, about the Purkyně images, the reflections of a lighted candle from the anterior and posterior surfaces of the lens of the eye and from the cornea, images the relative distances of which show the degree of lenticular accommodation, and perhaps also about the large Purkyně cells in the cerebellar cortex; but they do not know much about Purkyně, a pioneer in scientific physiology and a great Czech nationalist. This little book is published in English in Prague as a tribute to an historically famous Czech. The Purkyně University was founded in Brno in 1919, right after Czechoslovakia had become an independent republic, and Dr. Kruta is now Professor of Physiology there. He writes 102 pages of the scientific biography and the more detailed account, while his junior colleague, Teich, provides a much shorter parallel story that emphasizes more Purkyně's contributions to Czech nationalism.

Purkyně, who was born in 1787 and was six years old when his father died leaving his mother in need of support, was a bright boy who won recognition in school and found employment (as chorister, as tutor) to keep himself on the way to a Ph.D. in physiology at Prague in 1818, with a thesis entitled *Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht*. This is the volume, published in 1819, that contains the account of the Purkyně phenomenon (observed at dawn), the Purkyně images, and a number of other less well-known subjective observations that are facts in psychophysiological optics. It was a novel thesis, much richer in empirical fact than was usual in those days when physiology was not yet recognized as an independent science and had at the universities as yet no separate institute. Philosophical speculation then still invaded most of the scientific theses. In this monograph Purkyně made the diplomatic mistake of not mentioning the great Goethe, author of the monstrous *Zur Farbenlehre* of 1810—monstrous because of its enormous size, its conceit, and its presumptuous undertaking to devour Newton's theory of colors.

Purkyně stayed at Prague until 1823, when he accepted the chair of physiology at Breslau, where he remained until 1850. His appointment at Breslau was made against the disapproval of the faculty and may have been promoted in part by Goethe. Purkyně had by now met Goethe, had mollified him and formed with him a weak friendship, which never became strong because Purkyně could not bring himself to applaud Goethe's contribution to color theory—although Purkyně did in 1825 dedicate to Goethe the second volume of his *Beiträge* of 1819.

Besides the hostility of the faculty, at Breslau Purkyně had to face the handicap of lecturing in German, a language which he did not use readily and acquired only slowly, because one of his most enthusiastic activities consisted in advocating the use of Czech as an acceptable medium for scientific report. There was at Breslau no institute of physiology, no laboratory. Purkyně's house was cluttered with apparatus, specimens, and data. His demands for an institute were ridiculed or scorned. "Should every professor have his institute?" asked the Curator at Breslau, observing that Berlin and Bonn had in 1831 no separate institutes of physiology. Purkyně had one room assigned to him for instruments, animals, investigations and demonstrations, and was in constant danger of losing that. Finally, however, with a new Curator in office, Purkyně got his request, a *Physiologisches Institut* in 1839, a brick building, once a shed, tucked in back of the Anatomical Institute. (Anatomy had prestige; physiology was a poor relative.) It is easy to see how inhospitality to physiology actually helped the as yet unborn experimental psychology, for it was easier to work "from the subjective aspect" if you had poor facilities, and the human subject could be yourself who required less additional housing than experimental animals. All this striving for recognition of the experimental method in science sounds familiar, for it anticipates psychology's efforts forty years later.

The second volume in 1825 of the *Beiträge zur Kenntniss des Sehens* came out shortly after Purkyně's appointment at Breslau. Most of Purkyně's scientific research was accomplished during the twenty-seven years of this appointment. Purkyně worked on vertigo, picking up the line from Flourens, on the effects of drugs on the human subject, early work in psychopharmacology, in which he noted that, after experience with ipecac, the sight of the drug induces nausea, on the color-blindness of normal peripheral vision, on the functions of the cerebrum and cerebellum, again taking over from Flourens, on the development of the embryo in the bird's egg, on the structure of plants, on the physiology of human speech, and on many histological studies after he obtained one of the newly invented compound microscopes in 1832.

He was called back to Prague in 1850, where he spent the rest of his life, dying in 1869. In this period he devoted more of his energies to educational problems and to the popularization of scientific work, in addition to getting Czech accepted as a scientific language.

Purkyně was a pioneer in experimental physiology, but revolutions are slow. They have anticipations before they are brought about and their chief entrepreneurs themselves seldom accept the full implications of the changes they are advocating. Wundt may have founded experimental psychology but he never got fully away from philosophy. Purkyně retained a spiritualistic philosophy along with his science. He believed, for instance, in the earth itself being imbued with feeling and perception, a view that reminds us that Fechner at the same time was writing about the minds of plants and championing a panpsychism, while Johannes Müller, the greatest physiologist of the age, held on to the vitalism which Helmholtz and the other leaders from among his pupils repudiated. In a similar way Agassiz refused to surrender the doctrine of special creation when his younger associates were all accepting the Darwinian revolution. Purkyně was limited by the inertia of the *Zeitgeist.* His own thinking correctly anticipated and promoted the future of experimental physiology but could not go all the way. For the same reason the thinking of others, the climate of opinion at the time, held him back by making it difficult for him to have the facilities that he needed for experimental work. His importance and originality are not, however, to be overlooked because his life was inevitably limited by the period in which he lived.

Harvard University EDWIN G. BORING

*The Notebooks of Samuel Taylor Coleridge: Vol. II, 1804-1808.* Edited by KATHLEEN COBURN. New York Pantheon Books, Inc., 1962, Part 1, Text, 478 pp.; Part 2, Notes, 548 pp. $12.50.

In the September, 1958, number of this JOURNAL (Vol. 71, pp. 630-632), the first volume of Miss Coburn's edition of Coleridge's *Notebooks* is reviewed. In the new volume, also of two parts (one of Text and the other of Notes), the editing task goes on with thoroughness and tact equal to the first.

The practice of keeping notebooks was begun by Coleridge as a young man and stayed with him to the end of his life. In them he recorded phrases or passages from books (in Greek, Latin, German, Italian as well as English), plans for work, vocabulary lists of languages, addresses of friends, descriptions of places, medical prescriptions—a wierd assortment of items stirring in the mind of a fabulous reader and an introspective man. In a constant mood of self-analysis, he jotted down his dreams, his beliefs, an occasional mood of happiness, and a prevailing and deepening sense of guilt and anxiety.

With unbelievable patience and devotion, Miss Coburn traces the quotations to their sources in Coleridge's reading and links the personal items with the known facts of his career. Only now and then does she have to fall back on a statement such as "The source is not known," and the infrequency of this remark is the highest tribute that can be paid to her zeal in coping, word for word, with every book that Coleridge read and every incident in his varied life-story. The notebooks covered in these volumes and used from 1804 to 1808 present more than ordinary difficulty. During this period, Coleridge was estranged from his wife and family, in love with Sara Hutchinson, the sister of Wordsworth's wife, and, unhappy and undirected, moving from England to the Mediterranean and home again, was becoming more and more addicted to opium.

To hide from possible scrutiny his comments on love and drugs, he made use of crytograms which Miss Coburn for the first time has been able to interpret. So as to obliterate, too, some of the more intimate notes, previous users and owners of the notebooks removed pages and tried with acid and ink to expunge the Coleridge's derelictions. In spite of these difficulties, Miss Coburn presents a remarkably full and interesting text, and her notes set all of the entries in the context of Griggs's *Letters* and the other scholarly records.

What, apart from Coleridgeans who treasure and praise the least fragment of their idol's writing, can the average intelligent reader garner from this labor of love and scholarship? In the first place, and more and more clearly as the editorial work goes on, one of the most intimate and complete case-histories of the "Romantic" temperament. Shared in more or less abundance by everybody, it is the propensity to live in mood rather than in rational awareness, and to see the world outside the personality as a universe of symbols rather than as objects. To the man in the street a yellow primrose, as Wordsworth put it, is a yellow primrose and nothing more, but to the romantic the flower both conceals and reveals a massive personal message. In this universe of meaning-waiting-to-be-discovered Coleridge lived more consistently than most, and his drug-taking was at one and the same time a consequence and cause of this temperamental addiction.

In this kind of mind, too, are apt to arise the most stimulating pictures of how the human mind operates. It is not by chance that readers of Coleridge find themselves saying, "I've met this notion in Freud or Jung or Kierkegaard or William James," because the central ideas of these later philosophers may often be found sketched, briefly but effectively, in one of his casual comments. As in this, for example: "Death, in which the mind by its own wish might seem to have caused its own purpose to be performed, as instantaneously and by an instrument as spiritual, as the Wish itself!" In remarks like this, the literary critic, as well as the philosopher, discovers a curious power of insight into the source or function of a particular form of human activity. Though Coleridge was unable, for many reasons, to bring these flashes of thought together so as to construct a manageable system of thought, they have the power to "startle and waylay" the mind of the curious reader.

Perhaps most significant, however, for Coleridge himself, as for those who care about his poetry, are the 'images,' the pictorial elements of his mental process that flow in an unbroken sequence from note to note. In spite of efforts he made to learn about the science of his day and to organize his thinking in the abstract patterns of philosophy, he was by nature and desire an "image-thinker," one who saw experience unroll not as a subject for dissection—the psychologist's way—or as a pattern of ideas—the philosopher's way—but as an always exciting kaleidoscope of shapes, colors, tensions, all drenched in feeling. On a trip out of Rome, for example, up the valley of the Nera, he wrote about the scenery: "well-wooded mountains, with towns here & there surface, sometimes disclosing, more often hiding their narrow valleys, seen only half way down, & in the distance their heads only seen, as billows—all together exquisitely green—& the lines playful and tossing." The picture of the well-wooded mountains, seen as billows, their lines playful and tossing, can rise only in a mind curiously fit for poetry, and the pity of these notebooks lies in the fact that the material remained 'raw' and could not, for a number of reasons, be worked into the finished verse product of his good years.

Washington University ALEXANDER M. BUCHAN

*Activation and Behavior.* By ELIZABETH DUFFY. New York, John Wiley & Sons, 1962. Pp. xvii, 384. $7.95.

For over three decades, Elizabeth Duffy has argued that emotion is not a useful category, and has urged us to describe all behavior in terms of (a) its energy level and (b) its direction. The present book is an extensive documentation of her position, especially with regard to the energy dimension. She now calls it "Level of Activation," a better term than her earlier ones, "Excitation" or "Energy Mobilization," in that it relates her construct to the recent developments in our knowledge of the reticular activating system.

Duffy summarizes her position in general terms—

"The organism is conceived of as characteristically an energy system, storing energy and releasing it in activity of one form or another. The behavior which occurs apparently varies with (1) the extent of, and fluctuations in, energy release or activation, and (2) the parts of the body maximally activated, as well as the direction of the activity upon the external environment." (p. viii, ix)

This formal statement, or any other that could be made, fails to tell us much about her views. We can get a better idea by examining one of the many hypotheses she presents. For example, consider the inverted "U" curve that relates efficiency to level of activation. It has high face validity—it is obvious that a sleepy person wouldn't do very well on a test of intelligence (or of tweezer dexterity), nor would a person at the high level of activation characteristic of panic. These tasks, like most others, undoubtedly will be done best by a person at some optimal level of activation. But when we try to find experimental evidence for this relationship, we run into many problems:

1. What measure (or measures) do we use for level of activation?
2. What is the optimal level for a given task?
3. Do individuals vary in their optimal level for a given task?
4. Are we concerned with *optimal* level, or with *variations* in level?

Answers to questions like these are sought in approximately 900 references, drawn from physiology, pharmacology and essentially every area of psychology. The result is page after page of short abstracts, often lacking the detail necessary to evaluate a given experiment, or Duffy's interpretation of its findings. Furthermore, this method of presentation makes great demands on the reader, for he must always remember "Which way is up" for the diverse indicators of activation. For example, increased activation is associated with increased skin conductance, but with decreased skin resistance. Many of us will be extremely grateful to Miss Duffy for giving us such a complete and useful annotated bibliography, while regretting that it often hides the insightful interpretations and summaries she has scattered through the text.

Unfortunately, Duffy doesn't find unequivocal answers to many of her questions in this mass of literature. Perhaps part of the trouble is her molar approach, which pays too much attention to general level of activation, and not enough to activation of specific functional systems. One is reminded of the writings of Hull and Spence, in which drive is considered to be generalized. It is true that any drive may increase general activity, as in a running wheel, but this increase seems to be associated with activation of a specific functional system. There is much evidence for the importance of specific activation. Thus the female rat will accept a male only when her sexual system is activated by specific hormones, *i.e.,* when she is in heat. One is even reminded of the problems faced by the Chicago group of Functionalists half a century ago. In our present terms, we can say that a man who is conversing with a friend as they stroll along a road is highly activated with regard to the topic of conversation, while the system controlling walking is only moderately activated. But when he comes to a mud puddle, the relative activation of the two systems is reversed, so that he stops talking, and "pays attention" to his steps. It is interesting that these old issues are so persistent; the E.E.G. researchers are now becoming increasingly aware of problems long discussed under the traditional rubric "Attention."

But let us return to Duffy. She ends by saying, "Whether the conclusions derived should take the approximate shape that is presented here remains for further investigation to determine. If the image proves to be even partially correct, it will be illuminating in many areas of psychology. If it proves to be altogether wrong, my colleagues will at least have at hand some references which may prove useful in the construction of a better image." (p. 323)

She is far too modest. She has given us a collection of references that will be invaluable for workers in many areas. As for the image she presents, it is certainly partially correct. However, it might have shown the interrelationships among experimental findings more effectively if it had been taken with a twin-lens stereo camera.

Brown University HAROLD SCHLOSBERG

*Statistical Principles in Experimental Design.* By B. J. WINER. New York, McGraw-Hill, 1962. $12.50.

The publication of this book is an important event for those researchers and teachers who desire a comprehensive coverage of experimental design and analysis, with illustrative examples drawn from the psychological laboratory rather than from the Iowa corn fields. Winer presents experimental designs not previously discussed in texts written for psychologists, he treats several topics more extensively than have his predecessors, and he takes greater cognizance of recent developments in the statistical literature than have others who have recently published on this topic. But Winer has done more than merely present a wealth of material, a handbook of designs and analyses. Equal emphasis has been given to developing the rationales for the procedures which are presented. Relationships between design, analysis, and underlying mathematical model are clearly stated. Expected mean squares are routinely presented in analysis of variance tables. The advantages and disadvantages of each design and analysis are discussed.

Some of the material which Winer presents has previously been available only in a handful of non-psychological texts or in scattered journal articles. A major segment of the book deals with designs which involve the principle of confounding, designs which hitherto have been completely neglected in psychologically oriented texts. The use of modular arithmetic to facilitate the development of these designs, and the presentation of numerical examples and analysis of variance tables for most cases, result in a solid exposition of a differentt opic. A somewhat briefer, but equally competent presentation of nesting, or hierarchial designs are also included. This should be of special benefit to the educational and social psychologists who often use such designs, and often improperly analyse the data. The treatment of repeated measurements designs is the most extensive now available. In addition to the analyses of variance usually presented for such designs, there is a detailed discussion of trend analyses, covariance analyses, and range tests. The treatment of the model underlying repeated measurements designs is up-to-date, and includes a discussion of the usually ignored homogeneous covariance assumption, a test of this assumption, a brief description of the consequences of violation of the assumption, and a conservative $F$ test for the case where the assumption is violated. Other topics considered by Winer which are rarely or inadequately treated in comparable texts are pooling, quasi-$F$ ratios, least-squares solutions for disproportionate $n$, and additivity. The additivity assumption is clearly explained with reference to the underlying model and the rationale and computations for Tukey's single $df$ test of non-addivity are presented. The treatment of interaction is excellent, particularly a six page discussion and related graphic presentations dealing with higher-order interactions and their relation to effects of lower order.

There is little in this book that I care to take issue with. I would prefer a clearer, more extensive statement on error-rates, more material on confidence intervals, particularly intervals for variance components, and some discussion of stratification, or matching, as a design principle. The most glaring lack in the book is the complete absense of problems or exercises, which reduces its value as a text.

The treatment of trend analysis for repeated measurements designs is somewhat confusing. For the single factor-repeated measurements design, Winer suggests as an error term for testing the $p^{th}$ component of the treatment variable the *S*sx-Treatments interaction *ms* or as "an alternative, more widely used testing procedure" the pool of the interaction and treatment components higher than the $p^{th}$. Later, when dealing with the split-plot design involving both between-*S* and within-*S* effects, Winer suggests that the $p^{th}$ component of treatments be tested against the $p^{th}$ components of the treatments error term. This last procedure is preferable to the two described earlier, and is equally applicable to the simple single factor design.

To summarize, this is now the best book on experimental design purportedly written for th psychologist, and compares favorably with the best of such texts in other substantive areas. It is not an easy book, but the psychologist who understands its contents will have a considerable knowledge of experimental design and analysis.

University of Massachusetts JEROME L. MYERS

*Creativity and Intelligence: Explorations with Gifted Students.* By JACOB W. GETZELS and PHILLIP W. JACKSON. New York, John Wiley & Sons, 1962. Pp. xvii, 293. $6.50.

In *Creativity and Intelligence,* Getzels and Jackson have given a very readable and clear account of their dramatic and important study of different types of gifted adolescents. The title represents less than half of the story. In addition to their breakthrough study comparing highly creative students with highly intelligent (high IQ) students, they report what is perhaps an equally important study of students gifted from the standpoint of personal adjustment and of high moral standards. Thus, this book will probably go down in the history of psychology and of education as an important link in our expanding concept of giftedness.

In the study of highly creative and highly intelligent students, the authors identified one group of 28 students who ranked in the upper 20 per cent of their sample of 449 students on measures of intelligence but below the top 20 per cent on measures of creativity. They compared this group with a second group of 26 students who ranked in the upper 20 per cent on measures of creativity but below the top 20 per cent on measures of intelligence. In spite of a difference of 23 IQ points, they found no differences in the measured achievements of these two groups. Furthermore, they found that teachers preferred the highly intelligent pupils to the highly creative ones, in spite of the fact that the two groups were equally superior in achievement. The highly creative students differed from their teachers in their values, tended to choose unconventional or rare occupations, and were characterized by their humor and other characteristics usually associated with creative behavior.

In a similar manner, Getzels and Jackson identified a group of 28 highly adjusted and another of 27 highly moral students. By holding intelligence constant, they found that the moral group scored higher than their highly adjusted peers on appropriate tests of achievement. Teachers, however, rated the highly adjusted students higher on leadership than the highly moral students. The highly adjusted students, as might be expected, were better satisfied with their teachers, the curriculum, and their peers.

The authors do not take a position emphasizing creativity over intelligence (IQ), as some critics have interpreted. Neither do they emphasize the importance of morality over adjustment. They simply show that intellectual giftedness may be viewed in *at least* two ways, in terms of creativity and in terms of intelligence and that psychosocial excellence may be viewed in *at least* two different ways, in terms of adjustment and in terms of moral standards. Furthermore, they show that "creativity" and "intelligence" actually are two ways of viewing intellectual giftedness and that "adjustment" and "moral standards" provide two different ways of regarding psychosocial excellence. In doing this, we cannot conclude that creativity and intelligence are antithetical nor that adjustment and morality are antithetical. Perhaps it is in this respect that the presentations of the authors have misled some students of their work. They have said little about those who rank high *both* in creativity and in intelligence, *both* in adjustment and moral standards. While they call for increased emphasis on creativity and moral standards, they do not call for a de-emphasis on intelligence and adjustment.

Although this reviewer has found through numerous partial replications of the study of creativity and intelligence that the results reported by Getzels and Jackson do not hold in all schools, he knows of no data which are in conflict with the fundamental concepts developed. The Getzels and Jackson study simply does not tell the entire story. No single study can answer all of the important questions about such a complex problem as "giftedness." The phenomena do occur in most schools and other learning situations thus far studied. These additional cases emphasize the operation of an "ability gradient," the importance of opportunities for learning creatively as well as for learning by authority, and differences in the distribution of talent in different schools. All of these are problems which must be dealt with before we understand many of the complex problems of giftedness.

University of Minnesota
E. PAUL TORRANCE

*Sleeping and Waking.* By IAN OSWALD. New York, Elsevier, 1962. Pp. 232. $7.50.

When we consider that a third of our lifetime is spent sleeping and that this third significantly affects our performance in the remaining period, the relatively few pages devoted to sleep in our psychology textbooks, journals, and reviews are rather striking. If Oswald's book served no other purpose than to bring together the growing volume of research found for the most part in the nonpsychological literature, it would be making a significant contribution. The author does not claim to present an exhaustive reference work and, in fact, admits deliberately excluding reports in which a critical attitude toward the data is not shown. However, this compilation of more than 450 references, many published within the last five years, does a remarkably good job of filling the gap which has developed since Kleitman's classic "Sleep and Wakefulness" appeared in 1939. The book, however, should be much more than a reference source to the psychologist who reads it.

From its beginning, the book emphasizes the importance of an interdisciplinary approach to research on man's ability to perform in reduced states of alertness and consciousness. The first four chapters of the book summarize in an easy-to-read style the physiological correlates and the structures of the nervous system underlying sleep and arousal, as they are understood today. Particular attention is paid to the brain-stem reticular formation, one of the more exciting new discoveries in neurophysiology in recent years. The author properly distinguishes the information primarily derived from animal and from human subjects, serving to draw attention to the dangers of making interspecies generalizations. He devotes one whole chapter to a description of the electroencephalographic patterns representative of various stages of wakefulness, drowsiness and sleep. This bioelectric measurement is one of the best single physiological indicators of depth of sleep which is available to the behavioral researcher who must work with intact subjects. Throughout the book, performance is continually related to these patterns.

In spite of the space devoted to physiology, the book stresses the psychology of sleep. In doing so, it helps dispel a popular misconception that sleep is essentially a state devoid of behavior and therefore of little interest to the psychologist. Oswald devotes the next four chapters to man's performance while at different levels of drowsiness and sleep. He reports experiments on the effects of drowsiness and sleep on reaction time, muscle tonus, memory, and detection and discrimination thresholds. Conditions at the borderline of sleep are discussed. Particular interest in these chapters is directed toward subjective imagery, attention, hallucinations, and dreams. The similarities between behavior occurring during natural drowsiness and sleep and certain types of abnormal behavior found in the waking state are discussed.

Several times recently, budget-minded U.S. Congressmen have questioned the practicality of Government-supported research on sleep and related topics. The last four chapters of Oswald's book eliminate any doubt that good information obtained from the research in these areas is basic for solutions to many practical problems faced in clinical, industrial, military, space-travel, as well as ordinary day-to-day life situations. Here he covers research relating to such topics as monotony, fatigue, hypnosis, "brain washing," insomnia, narcolepsy, and sleep therapy to name a few. Problems of sleep deprivation, hibernation and wake-sleep scheduling, with which the book deals, have become particularly critical today, when military and space crews are required to maintain round-the-clock vigilance over extended periods of time in rather confined spaces.

It may be disturbing to the reader to have the author, at times, inject his personal, unconfirmed conclusion concerning conflicting or inconclusive research results. If the reader remains aware that these are only opinions, however, and considers the author's extensive experience in research on sleep, these interjections serve as useful hypotheses for further research. The book itself is an excellent stimulator of problems for the experimenter interested in new vistas of human behavioral research. The reader may be assured that reading *Sleeping and Waking* will certainly keep him in the latter state.

CHARLES W. SIMON

Aerospace Group
Hughes Aircraft Company
Culver City, California

*The Profession of Psychology*. Edited by WILSE B. WEBB. New York, Holt, Rinehart and Winston, 1962. Pp. viii, 291. $4.50.

This book is an effort to ". . . portray psychology by describing the actions of psychologists in context" (p. 3). It is directed toward psychology majors in a university program who (the editor believes) have been confused about what psychologists do because their courses have almost solely told them what psychologists know (or think they know) and through what techniques they come to think they know it. Thus, although almost every contributor performs the proper genuflections toward the importance of the psychologist being ". . . a scientist first, a practitioner second . . ." (p. 9), the intent is to tell how psychologists use data rather than how they obtained them, and to describe the ways of life which the various uses produce.

In what appears to be an attempt to encourage his contributors to maintain this orientation, the editor has based his organization on the major environments in which psychologists may work. The basic chapters are about psychologists in colleges and universities, clinics, institutions, independent practice, schools, industry, government agencies, and other professions. In addition, there are chapters of general import on conceptions and misconceptions of psychology today, the antecedents of contemporary psychology, the organization, training, and employment of psychologists, and the ethical problems confronted by psychologists in their relations with the public. Further, for some reason not clear to me, there is an appendix on psychologists in other countries which has most of the characteristics of an afterthought. The general chapters are contributed by Webb and Erasmus L. Hoch. The other contributors are: Frank W. Finger, Thomas Greening and J. F. T. Bugental, Seymour G. Klebanoff, Molly Harrower, T. Ernest Newland, Raymond A. Katzell, Benjamin Fruchter, and Ivan Norman Mensh.

Webb, Hoch, and some of the other contributors make a valiant effort to produce the impression that there is, indeed, a profession of psychology which embraces all psychologists; that, while there are different breeds of this cat, we are all cats, *e.g.* "Psychologists feel more alike than different, and this is how they would have it" (p. 12). While they have not been altogether successful, this prejudiced reviewer was surprised that they were able to present as convincing a picture as they do. There is considerable duplication from chapter to chapter in content and even in bibliography, and much of this seems truly to reflect the existence of a general core of interests, outlook, and approach. Less fortunately, a duplication of content appears instead to result from a failure by some authors to observe the editor's desire to describe what we do rather than what we know.

A very disturbing duplication results from the inclusion of the three relatively lengthy chapters on psychologists in clinics, institutions, and private practice. It is easy to see how the editor's rubric leads him into this but one wonders whether three separate treatments are really required. Furthermore, Harrower's chapter on independent practice, adapted from her textbook on clinical psychology practice, is directed toward graduate students with advanced clinical training rather than to the bewildered and exploring major specified in the introduction.

A chapter that might well have been the most difficult but turns out to be a delight in terms of clarity, faithfulness to stated purpose, and sheer readability is that on colleges and universities. Finger's device of following ". . . a representative professor through a representative week of the school year" (p. 63) comes off so well that it may turn some good majors toward teaching and research. This chapter needs strength in this milieu and Finger provides it.

In general, the book has a good purpose and serves it will. The chapters are uniformly knowledgeable and accurate, if understandably optimistic about the state of the art. They should be useful in giving the novice the start of an answer to his question which Webb quotes with relish, *i.e.* ". . . what does a psychologist do when he grows up?" (p. 2). It might even make some of us professionals ask the question again.

Life Insurance Agency Management Association — S. Rains Wallace
Hartford, Connecticut

*Uncertainty and Structure as Psychological Concepts.* By Wendell R. Garner. New York, John Wiley and Sons, 1962. Pp. viii, 369. $8.95.

This book consists of two related but fairly distinct strands woven together. On the one hand it provides a review of most of the major psychological experiments which have used concepts drawn from information theory. On the other hand, the book also contains a systematic approach by the author to the use of such concepts, and this personal approach is developed steadily and logically throughout the volume. Although connections are drawn between the conceptual approach and the experiments, they are relatively slight, since most of the experimental work has been done with slightly different conceptual backgrounds and the conclusion is very often that the existing literature does not provide evidence to elucidate the points which the theoretical analysis has emphasised. The writing is clear and does not suppose a previous knowledge of information theory; but the book is clearly intended for people who are reasonably sophisticated in the areas covered by a psychological training. For example, aspects of the contingent uncertainty which can exist in a situation involving two variables are explained by a parallel with the analysis of variance, and it is assumed that the reader has a thorough rather than a superficial understanding of the latter. For this reason, and because the theoretical development is avowedly personal, this must be regarded as a contribution to advanced psychological thinking rather than as an all-inclusive review of the various approaches to psychology which have stemmed from informational ideas. The passages dealing with the experiments done by others are, however, inclusive and will form a sound base for anybody entering these areas of work.

The general conceptual approach might be summarized as follows: The author is keenly interested in those mathematical relationships which appear when each of several variables can take on a number of values, so that there is uncertainty about the value of each, and in the structure which exists when there are restrictions on the values of one variable which may accompany values of any other. There are numerous points which can be developed from a purely logical or mathematical analysis of this kind of situation, and which are well brought out here. Thus there is a sense in which the total constraint within a system of variables must remain constant, so that any changes in the uncertainty existing between some of the variables in the system will bring reciprocal changes in the uncertainty between those variables and the others of the system. If an experimental psychologist is not aware of this kind of relationship, he may get quite misleading results by choosing his experimental material inappropriately. Thus the difficulty of a concept-formation task, for example, may change very considerably with the precise sample of stimuli that are presented during learning sessions. It is clear that this kind of analysis has been much neglected in the past, and that it should form essential preliminary reading for anybody who is about to embark on experiments in which complex stimuli are presented.

It will be obvious that an approach such as this leads one to concentrate upon the measurement of what a person does rather than how fast he does it. Thus although the experimental work on reaction time and on skilled performance generally, which is so closely related to information theory, is mentioned, it falls rather outside the main stream of the book. This may disappoint some readers, and the empirically minded may also feel frustrated by the number of stimulating conceptual points which are made in the absence of experimental evidence. Theorists may possibly argue with one another about such points as the usage of the word 'redundancy' in this approach, which is not perhaps identical with the way in which everybody else would use the word, and they will certainly regret the absence of any section dealing with statistical decision theory. Yet this book will make them discuss, and it will almost certainly make the empiricists do more experiments. It represents a real advance in our thinking about the ways in which one event can depend upon another.

Applied Psychology Research Unit
Cambridge, England

D. E. BROADBENT

*The American College: A Psychological and Social Interpretation of the Higher Learning.* Edited by NEVITT SANFORD. New York, John Wiley & Sons, Inc., 1962. Pp. v, 1084. $10.00.

To the busy college professor, administrator, or trustee, the reading of a book of 1035 pages on the state of higher education would have to prove highly rewarding. For the most part, *The American College* is. Its distinctive contribution is indicated in the subtitle, *A Psychological and Social Interpretation of the Higher Learning.* The editor and authors ". . . regard higher education—its policies and processes and the determinents of them—as a field of intellectual inquiry" (p. 1). The liberal education of the college undergraduate is subjected to the scrutiny of social scientists as they turn their attention to the social-psychological phenomena of the American college. The subject matter of the 29 chapters covers a broad range as the book moves from a study of the entering student, through the college environment and the behavior of the college student, to the effects of the college experience upon the students.

Editor Nevitt Sanford, Director of the Institute for the Study of Human Problems at Stanford University, has done a noble job in attempting to unite diverse contributions into a consistent approach through his emphasis upon the college student as a developing psychological being. His chapters and introductions show him to be a sophisticated social scientist and a concerned educator. Nevertheless, since few will find time to read everything, more rigorous editing might have improved the total offering.

The goal of the editor was, ". . . to show that the field is open to a variety of approaches, and that it may benefit at this time from being viewed in different perspectives" (p. 3). With this avowed emphasis on the desirability of different perspectives, one questions the failure to identify the positions and fields of specializations of the 30 authors who are, ". . . different among (themselves) in background, experience, and field of specialization . . ." (p. 3). It would be most helpful to the reader to know the perspectives from which the authors write, analyze and recommend.

In general, the book strikes a nice balance between research reports and clinical impressions. The research chapters range from initial reports of research programs (*e.g.* those conducted at Vassar by the Mellon Foundation) to careful summaries of existing research (*e.g.* Summerskill's "Dropouts from College," with almost 200 references).

Readers will have their own ideas about the appropriateness of the proportion of space allotted to the various aspects of the American college. This reviewer found the treatment of motivational factors in college entrance especially disappointing, since the only chapter devoted to this topic was narrowly dependent upon psychoanalytic theory for the interpretation of some interesting data.

Part III on Academic Procedures, on the other hand, is unusually thorough and one of the unique contributions of the book. Part VIII, "Higher Education and the Social Context" will be of interest to all. Campbell Stuart's chapter on the history of education and his interpretation in the light of modern sociology is a scholarly job; and Christian Bay's, "Social Theory of Higher Education" presents a challenge to concerned educators everywhere. Frank Pinner's, "The Crisis of the State Universities: Analysis and Remedies" contains interesting and sometimes astute observations. His work might be more objective and constructive, however, if he were not bound by the conviction that the source of the difficulties is the widening gap between the genius of the faculty and the stupidity of almost everyone else—students, the public, administrators, and even older faculty members.

On the whole, the authors have performed a valuable service for higher education in their reports of new research, summaries of existing research, insightful analyses of problems, raising of thought-provoking questions, and suggestions for further research.

K. PATRICIA CROSS

Cornell University

*Computer Applications in the Behavioral Sciences.* Edited by HAROLD BORKO. New York, Prentice-Hall, 1962. Pp. 633. $11.65 (Trade), $8.75 (Text).

The book under review consists of an initial seven chapters that introduce the reader to computers and their programming, followed by seventeen chapters describing various applications of computers to behavioral science. Organizationally, the book represents an interesting attempt to solve the problem of writing in an interdisciplinary area. It is not just another collection of articles; rather, the editor, Harold Borko, wrote the seven introductory chapters and a final summary one, thus preparing the way for the specialized chapters written by experts in the different areas of application. Four of these chapters cover numerical procedures in psychology (factor analysis, multiple regression, etc.); the remainder cover the gamut of exotic applications: simulation, automatic teaching, natural language processing, etc. All the material was written expressly for this book. Thus the book is the right shape to be a text for a course in computers for behavioral scientists. However, it fails to be a really good book because of the pedestrian handling of the introductory sections and because the organizational form still shows the difficulties of a collection of authors.

On the other hand, a number of chapters are very good. The one by Charles Wrigley on the University Computing Center I found to be delightful and full of sense and scholarship. The chapter by Lejaren Hiller and Robert Baker gives the only recent material available on their work into computer music. (Hiller is the composer-by-indirection of the *Illiac Suite* a noteworthy example of early exploration into exotic applications of computers.) In general the chapters that present considerable detail are quite good: Julian Feldman's chapter on simulation of cognitive processes, David Hays chapter on automatic language processes and a section in Robert Simmons chapter on Synthex, a natural language retrieval system, where he goes through a sample calculation. The chapters on numerical applications are adequate, although they fail to be convincing that there is any other interaction between the computer and the applications except pure speed (maybe there isn't any). The other areas of application draw heavily on concepts of information processing suggested by the development of the computer. Those chapters are least good which stick to generalities and hortatory descriptions, e.g., the one on automated teaching or Ross Ashby's essay on simulation of a brain. Again, the chapter by Robert Ledley on advances in biomedical science deals with so many (interesting) examples that it fails to bring the reader to grips with any of the real problems in these applications. The two chapters dealing with the simulation of formal organization, one by Sydney and Beatrice Rome, the other by Clay Sprowls, deserve a special worry. Although both of them are quite explicit about what is going on in the technique of simulation, neither gives any adequate hint about how simulations at this level can be used to generate new scientific knowledge.

There are some gaps and missed opportunities. The area of programming languages is hardly developed at all, not even in the introductory material. Also one of the most technically impressive uses of computers to generate controlled complex training environments is going on in the Systems Development Corporation (where Borko and several of the other authors are employed). This book would have been *the* place for a description of these efforts, but it is not even mentioned.

In summary, the book is worth while because of some of the chapters. It could receive some use as a text, but will probably be superseded as more integrated efforts appear. It will introduce any psychologist to the wide range of ways in which the computer is currently being used in the behavioral sciences.

Rand Corporation

ALLEN NEWELL

*Training in Business and Industry.* By WILLIAM MCGEHEE and PAUL W. THAYER. New York, John Wiley & Sons, 1961. Pp. xi, 305. $7.50.

Although training is one of the traditional subjects of concern to industrial psychologists, there are very few books dealing with industrial training from the standpoint of scientific psychology. This circumstance makes particularly welcome a competent treatment, such as this one generally is. After a stage-setting chapter, the book takes up the analysis of training needs, principles of learning, methods and techniques in industrial training, the responsibilities and qualifications of trainers, and evaluation of training.

The often slighted subject of training-need analysis receives an excellent and thorough treatment in three chapters. Two chapters on learning principles cover much the same ground as may be found in many textbooks of general psychology, with the addition of comments seeking to point up the possible relevance of these principles to industrial training. In view of the avowed importance of attitude modification through training, it is unfortunate that scant attention is paid in this section to the substantial body of recent research and theory on the subject of attitude development and change. The authors are no more successful than are those of introductory psychology texts in bridging the gap between theory and the management of learning in practical situations. They take a sound way out by recommending that training programs be designed in the light of what is known about learning, while accompanying the execution with evaluative research. Good suggestions are made concerning the implementation of this strategy.

A general conceptual weakness is the primacy which the authors ascribe to helping to attain the organization's ultimate goals, defined essentially as productive efficiency or profitability. Not only social scientists but many management specialists would take exception to this narrow view of corporate objectives. Its espousal rules out as ultimate criteria measures of job satisfaction, mental health, innovation, or even organizational growth, unless they can be shown to be highly correlated with profitability (which they probably are not). It also may operate to discourage desirable evaluative research using more available intermediate and immediate criteria, such as turnover or skill mastery, by challenging their relevance unless proven to contribute to profitability; the virtual impossibility of establishing such proofs is well known both to psychologists and to management analysts. Furthermore, this position contains the assumption that training should always operate to bring employees' attitudes and behavior into line with management's goals, so that the possible use of training for the mutual accommodation of the goals of superiors and subordinates is not even considered.

There is also a problem in the general focus of the book. Written by psychologists, it is expressly addressed to managers and trainers. It therefore skirts some theoretical and methodological subtleties which are of concern to psychologists, while discussing a program that will surely exceed the technical capabilities of nearly all managers. Nevertheless, both of these potential audiences will find much of value in this book.

RAYMOND A. KATZELL

New York University

*Advances in Psychosomatic Medicine: Symposium of the Fourth European Conference on Psychosomatic Research.* Edited by ARTHUR JORES and HELMUTH FREYBERGER. New York, Robert Brunner, 1961. Pp. 334. $8.50.

This volume is the published version of the fourth in a series of symposia on psychosomatic research in Western European nations. It appears as a supplement to the *International Journal of Psychotherapy and Psychosomatics.* Like all such symposia, there was considerable variability among the speakers' points of views, and among their substantive contributions, nevertheless, the organization was good and the transactions well worth publishing.

The meeting was divided into four sections. Part I, "Methods and Principles of

Research on Psychosomatic Fundamentals," contained the descriptions and guiding principles of research of scientists representing a wide range of disciplines, yet there were general points of agreement on procedure. Each of the speakers cited the complexities of psychosomatic research and the importance of multidisciplinary approaches.

One loudly dissonant note was introduced by Mitscherlich (a psychoanalyst) who argued that analysts should withdraw from multidisciplinary research teams and form teams of their own. One of the reasons he gave was, "that those psychoanalysts who have joined such teams have increasingly abandoned their own viewpoint and become more and more undifferentiated in their own special field of observation" (p. 34). I leave the reader to draw his own conclusions about the implications of this observation.

Section II, "Concept of Constitution in Psychosomatic Medicine," brought forth many ideas which one rarely hears at American symposia. What emerged, beginning with the interesting reviews by Freyberger and by Rees, was a formulation of constitution which was so broad that it took on the ubiquitous meanings that psychosomatic has. Nevertheless, there was a clear emphasis on correlations of anatomical and physiological characteristics with behavioral and temperamental indices.

Part III, "Training for Psychosomatic Medicine," dealt with the pragmatic problems of teaching physicians and medical students the techniques and skills necessary for diagnosing and treating psychosomatic disorders. There was general agreement that the most successful training methods to date have been developed by Balint at the Tavistock Clinic in London. It was unfortunate that each of the speakers at this session was an analyst. The symposium would have benefited from hearing the experiences of non-analysts.

Part IV, free papers, was a collection of original research and theoretical papers covering a wide range of psychophysiological problems—one of the papers included a critique of structural psychology!

Several factors distracted from the overall quality of the program. Many of the translations were quite poor, and one paper was reproduced in German; the discussions, which are often the most interesting parts of such symposia, were not published; and in a few papers the bibliographies were not included although there were numerous citations in the text.

San Francisco Medical Center
University of California

BERNARD T. ENGEL

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*Personality and Decision Processes: Studies in the Social Psychology of Thinking.* (Stanford Studies in Sociology, II.) By ORVILLE G. BRIM, Jr., DAVID C. GLASS, DAVID E. LAVIN, and NORMAN GOODMAN. Stanford, Stanford University Press, 1962. Pp. ix, 336. $8.00.

The title should not mislead the reader that this is a further book concerned with a derived psychological application of economic theories of choice. There is only a tangential connection with the arguments of such people as Knight, Shackle and Marshak, and there is little emphasis on the purely cognitive aspects of decision-making, analogous to the studies of Bruner or Festinger. There is certainly a final section of the book referring to the expected utility hypothesis, but the majority of the subject matter is related to considerations of personality and social class. What we have here is rather an index-orientated research as contrasted to a problem-orientated research. The indices employed are various factors related to the structure of personality, differences between social classes, differences between the sexes in the middle and lower classes, relationship between husbands and wives regarded again from the class aspect, types of situation, and individual versus group decision processes. The content of decision-making is exclusive to the single problem of various aspects of child-rearing.

The authors disarmingly admit that this is an exploratory investigation, more concerned with conceptual mapping of the problem rather than putting to the test specific hypotheses of decision-making. Certain interesting facts emerge. There is a relationship between the personality factor which is labelled autonomy-dependency and certain decision characteristics. People at the dependency end of this factor are likely to be more optimistic over the outcome of their actions than people at the autonomy end, and are likely to consider fewer outcomes in evaluating the alternatives. Lower class people veer towards the dependency pole, and there is less difference between lower-class spouses on this characteristic than in middle-class spouses. Certain kinds of child-rearing difficulties exert a situational influence on decision characteristics. Thus concern with masturbation problems affects the number of decision outcomes. The classical expected utility maximisation hypothesis appeared to fit the data on child-rearing decisions. This is a complex and intricate book. There are 108 Appendix Tables and at the end of one chapter the authors themselves admit that the results of that chapter are too complex to be summarised in detail. Evaluation may be aimed at the possible generalizations of the research. We may note the specific nature of the area of decisions, namely child-rearing, and the distinctiveness of the population tested, namely that it was predominantly Jewish. Traditional Jewish ideas of child-rearing appeared to have less influence than was thought, but it would be of value to have similar data with a more heterogenous sample. The book is a useful bridge between the concerns of the sociologist and the psychologist. For both, the result is likely to introduce more complexity than has been previously appreciated.

DOUGLAS PRICE-WILLIAMS

London School of Economics.

*The Analysis of Behavior.* By JAMES G. HOLLAND and B. F. SKINNER. New York, McGraw-Hill, 1961, pp. xvi, 337. $3.50.

As is generally known by now, this book, if it may be called a book, is presented in a self-instructional format consisting of 1800 information or teaching frames each of which requires that the student respond with an appropriate term or concept. In turn, the correct answer to each frame is provided for the student after he has responded.

It is difficult for a reviewer to be critical of a teaching machine program of the linear form when that program was written by the originator of linear programming for self-instruction, B. F. Skinner. The program is divided into 14 major sections, each section consisting of from two to six sets of frames; for a total of 53 sets. Unlike most of the teaching machine programs which have been published, this one contains a detailed table of contents with page references; unfortunately, it contains no index, a serious oversight for the reader who would like to refer to specific topics in the text. As a further aid to the student, estimated reading time for each set is provided as are self-tests and review. A number of the sets contain a panel of information in text or tabular form for use with the frames of that set. For the student who wants to write his response to each frame, it is rather awkward, at times, to leaf back and forth from panel to frame and still manage the act of writing without losing one's place. Undoubtedly, publishers will develop improved means of presenting panels of information without increasing the cost significantly.

The content of *The Analysis of Behavior* is familiar to readers of Skinner's *Science and Human Learning* and some of his more recent articles. For the beginning student in psychology or for the general reader who desires to learn about Skinner's positivistic psychology, the book is "guaranteed" to lead the student to achieve an understanding of its contents providing that he is willing to work through the material step by step, response by response—1800 of them. The published evidence of the self-instructional value of the program provides strong support for the "guarantee." Further, students' attitudes toward the book generally have been favorable; more so probably than the attitudes of professional psychologists, particularly those who write conventional texts.

In summary, *The Analysis of Behavior* serves as a fine example of linear programming for self-instruction and it serves, also, as a good introduction to Skinnerian psychology for the layman or the beginning student in psychology. It is a novel experience, indeed, to work through a book of this type.

Bucknell University — WENDELL I. SMITH

*Studying Personality Cross-Culturally*. Edited by BERT KAPLAN. Evanston, Illinois, Row, Peterson and Company, 1961. Pp. ix, 687. $8.50.

An extremely varied volume is this, more diverse than would be expected simply from its having 30 different authors. It begins with an excellent 60-page survey, by Milton Singer, of theory and research on culture and personality, accompanied by a 22-page bibliography. It ends with an epilogue by the editor. Here Kaplan looks over the contributions to the book, and points out what he sees as a major convergence in many of the writers: a shift in emphasis in recent years, from what might over-simply be labeled study of personality-culture coherence to principal study instead of mechanisms of conformity.

In between these two over-views are four groups of papers, gathered under headings of theory, methods, problems and approaches. As might be expected, this classification seems largely arbitrary, but it does indicate the nature of the purer extremes. On the one hand, there are theoretical articles dealing with such general and fundamental problems as the choice of constructs for use in understanding culture and personality—articles such as those by Sears, Spiro, and Wallace. At the other extreme, there are articles restricted to considering a single method of study; of these I would cite as especially useful Dorothy Eggan's article on dream analysis and George De Vos' article on content analysis of Rorschach responses.

Between these extremes falls many a chapter which is highly varied within itself. A chapter by Dell Hymes, for example, reviews several major points or interaction between linguistics and culture-personality study. Elkins' chapter on slavery and personality may be taken as representative of several articles which focus on a specific problem and bring to bear on it whatever methods and theories seem to be most fruitful. There may be value in listing here some of the other problems which receive extended treatment in the volume: acculturation, adjustment to the Soviet political system, the psychological significance of art and mythology (on the latter, La Barre provides a lengthy bibliography), mental disorders in other cultures, a Maori community and Israeli *kibbutzim* as sites for enquiry into culture and personality.

Out of all this diversity comes a highly useful volume, since so many of the separate articles are in themselves valuable contributions. The editor is to be congratulated for having instigated their writing and arranged for their joint publication.

IRVIN L. CHILD

Yale University

*Studies in Individual Differences: The Search for Intelligence.* Edited by JAMES J. JENKINS and DONALD G. PATERSON. New York, Appleton-Century-Crofts, Inc., 1961. Pp. xiv, 774. $8.50.

It is gratifying to have a volume of original sources available for serious students of individual differences in intelligence. Too often, even graduate students are acquainted only with secondary sources; and these, of necessity, are either seriously thinned down or too highly distilled. This volume of readings contains 66 articles or excerpts from books, arranged in chronological order, beginning with a Galton publication of 1869 and ending with a 1959 publication. As the subtitle indicates, the publications deal only with various aspects of the measurement and characteristics of intelligence; but the range of subject matter is quite wide.

Among the names of authors whose publications are included are those of well known psychologists such as: Spearman, Binet, E. L. Thorndike, Terman, Thurstone, Hollingworth, Boring. But there are also surprising omissions. There are no readings, for example, from the important publications of Otis, W. F. Dearborn, Stoddard, J. McK. Cattell, Psyche Cattell, Stern, Klineberg, Burtt, Yerkes, Herring, Sherman, Kohs, Pintner, Healy and Bronner. On the other hand, I believe that some of the readings included are of minor significance. Also, it appears that the book is unduly weighted with publications by the Thurstones, and with materials on racial differences. With the exception of the Binet-Simon articles and one from Sweden, the readings are from publications in the United States and Great Britain. Restricting the materials to those that originally appeared in English is probably necessary and wise. It would be well, however, if our graduate students could learn about work that has been done in other countries.

The selection of materials to be included in a volume of readings is a difficult one and a matter of personal preference. The exercise of one's preferences is limited by restrictions placed on the size of the book. It is probable that all editors would agree on a few of the articles to be included; but they would disagree on many others; and there is, no doubt, much to be said for each editor's selections. Jenkins and Paterson have provided a valuable collection of materials, with most of which advanced students should become acquainted. Perhaps several more similar volumes will be edited for publication, to provide a series of comprehensive and fully representative readings.

Cornell University FRANK S. FREEMAN

*The Practice of Clinical Psychology.* By MOLLY HARROWER. Springfield, Illinois, Charles C Thomas, 1961. Pp. xiv, 321. $12.00.

This book, devoted to private practice, should be read by all graduate students in clinical psychology; for it was written by a widely experienced, versatile, and expert clinician whose *early* preparation and work in psychology were distinctly non-clinical. And for the same reason, it should be read by graduate students in *non-clinical* areas of psychology, few of whom have an adequate conception of the scope of the work done by clinicians, their responsibilities, their procedures, their associates among non-psychologists, their concern for research, and their interest in furthering the development of the science.

The materials in the book include very practical considerations of how to get started in private practice; types of cases referred by physicians, industrial organizations, schools, and colleges; special contributions that can be made by psychologists; research opportunities and responsibilities; communicating with educators, physicians, and parents; and work patterns of established practitioners. An appreciable portion of the book is devoted to very readable presentations of specific cases from Dr. Harrower's own files, presented not for their own sake but to illustrate particular aspects of the clinician's work. She also regards the private practitioner's files as a rich source of materials for research purposes; and to support this view, she cites studies derived from her own files and offers suggestions for others as well; for she would encourage clinical psychologists to devote some of their efforts to research.

This book is an admirable piece of work. It is practical. It indicates how different professional groups can cooperate—a type of cooperation in which Dr. Harrower has been eminently successful. Some psychologists will disagree with some of her views and interpretations of certain research findings; but a degree of disagreement is inevitable. There is no doubt, however, that here is a distinguished book, written by a psychologist who has had long and varied experience with theory and research and in the applications of the science. Very important, too, is the fact that the book is written in a highly readable style.

Cornell University FRANK S. FREEMAN

*Experience in Groups.* By W. R. BION. New York, Basic Books, 1961. Pp. 198. $3.95.

This book consists of articles written by W. R. Bion and published in journals during the past two decades. Chapter One was written with the collaboration of John Rickman and dates from 1943, when it appeared in *Lancet.* The next seven chapters are reprinted from Volumes I through IV of *Human Relations,* and the final chapter originally appeared in Volume 33 of the *International Journal of Psycho-Analysis.*

Bion, who is a psychiatrist and psychoanalyst, has worked extensively with small groups as the setting in which therapy can be conducted. This book reports observations and "psycho-analytically developed intuitions" gained through years of experience with groups consisting of six or eight neurotic members whose desire to receive therapy is the manifest reason for their participation in the groups. Characteristically, Bion refuses to play the role of all-knowing leader which the members initially ascribe to him. Deprived of the messiah they crave, members tend to be guided by any of three irrational "assumptions": they seek to establish dependency relationships with a substitute leader, they form emotionally laden pair relationships with one another, or they pursue a hostile course of fight-flight actions. Any single group may shift rapidly from one "assumption" to another. Finally, after much travail, many groups become "work groups" which are reality-oriented and which seek to develop through learning. However, even at this advanced stage, behaviors will tend to manifest the somewhat irrational effects of the three guiding assumptions.

The reader with sufficient ingenuity and persistence can translate much of Bion's argument into the somewhat more familiar and quantifiable language of group dynamics as it prevails in this country. But the task of translation is not a very rewarding one because the few insights which are gained are only weakly supported by Bion's unsystematic observation. Without translation, much of Bion's discussion of groups is too abstract and too permeated with poorly defined psychoanalytic concepts to generate many testable insights. Psychologists may find this book to be thought-provoking, but few will conclude that it offers very convincing answers to the problems which are of most significance to students of collective action.

University of Illinois I. D. STEINER

*Classics in Psychology.* Edited by THORNE SHIPLEY. New York, Philosophical Library, 1961. Pp. xx, 1342. $20.00.

Thirty-six historically important selections, ranging in time from 1800 to 1938, appear here with an introduction, a biographical note for each, and a 6-page index of names. Three of the papers are given in English translation for the first time: sections of Herbart's "Psychology as a science, newly founded on experience, metaphysics and mathematics"; of Wundt's "Contributions to the theory of sensory perception"; and Wertheimer's "Experimental studies on the seeing of motion." I am glad to have these, although the translation of the Wertheimer paper (in . . . "an attempt . . . to catch the spirit of the original text") is frequently awkward to the point of unintelligibility.

Selections of readings are always subject to debate, and the most serviceable review is to list the contents. In addition to the three noted above, papers have been selected which present early systematic formulations, influential programmatic manifestos or characteristic analyses by Helmholtz, Mach, Titchener, Stern, Pinel, Esquirol, Charcot, Bleuler, Kraepelin, Rush, Prince, Wagner-Jauregg, Sakel, Jackson, Sherrington, Breuer and Freud, Adler, Jung, Pavlov, Watson, Hull, Cattell, Binet and Simon, Rorschach, Aichhorn, Hall, Piaget, Köhler, Koffka, Ray, Lewin, and McDougall.

The order of the readings is not a particularly useful one, to my view, nor is a clear rationale apparent for the editor's 21-page rhapsodic introduction. On the other hand, each selection is followed by a useful biographical and bibliographical section. The table of contents contains excellent abstracts appended to each title, which would have been exceedingly handy if the subtopics listed had been paginated; unfortunately, this generates a 14-page table of contents which, since it is in neither chronological nor alphabetical order, makes location of any particular selection difficult. This is aggravated by the omission of page headings to identify the reading at which one is looking; in a book of 1335 pages of text, such headings would be particularly desirable. This omission will not, however, affect specific page assignments, and the collection will be a welcome addition to the departmental or college library.

Cornell University JULIAN HOCHBERG

*The Methods and Findings of Experiments on the Visual Discrimination of Shape by Animals.* By N. S. SUTHERLAND. (Experimental Psychology Society Monograph No. 1.) Cambridge, England: Heffer & Sons Ltd. 1961, Pp. iv, 68.

A systematic analysis of the methodology of studying shape discrimination is followed by an equally intensive review of the results of such studies. The methodology section is well worth reading for anyone starting work on animal discrimination as it includes many necessary considerations arrived at ordinarily through long experience. This section includes such topics as: indices of learning, experimental apparatus, nature of the response, and experimental control. The absence of any reference to operant techniques and a lack of consideration of the use of automation to overcome such problems as the presence of the experimeter are minor failings.

The analysis of the results on shape discrimination is impressive and provocative. By juxtaposing results of a large number of studies, Sutherland is able to compare discriminability of shape on the basis of a large number of factors. Such analysis leads to the conclusion, for example, that rats, chickens and fish often discriminate in terms of parts of figures but the rat's discrimination is usually based on the baseline of the figures and the chicken's and fish's on the most differentiating parts of the figures. As the author points out, it is indeed unfortunate that the stimuli used in the various experiments with the various species have been so different that comparison is often difficult, but in this, there is also the challenge. Is there, for example, any phylogenetic regularity in ability to transfer a form discrimination to a reversed figure ground brightness relationship? The evidence cited in the book indicates that carp, hens, and rats don't, but minnows, cats, and primates do.

Human data are excluded from this monograph on the basis that humans are "capable of discriminations of such complexity that results are difficult to interpret." This certainly is the author's right, but inclusion of data from studies of children would have been interesting without being too complex.

This is a small book with much for thought.

University of Minnesota HERBERT PICK

*Contribution à la Théorie de la Causalité Perceptive: Nouvelles Recherches sur l'Effet-Entraînement.* By G. THINÈS. Louvain, Editions Nauwelaerts, 1962. Pp. 124. FB. 140. ($2.80.)

This research is an extension of Michotte's work on *entraînement,* the perceptual experience of one object (A) causing the motion of another (B) by carrying it along. In Michotte's work with extended objects, the static presence of B before its encounter with A is necessary to produce *entraînement*. Otherwise, screen-effect or gamma movement interferes and *entraînement* does not occur. Thinès avoids these difficulties by using dimensionless objects, *i.e.* points of light. (A consists of three points in the form of a triangle and B is a single point.)

In a series of experiments Thinès studies the types of perceptual experience evoked by this new technique, with emphasis on two specific problems: (1) the effect of variations in the duration of the static presence of B before encounter with A and (2) the effect of variations in the duration and extent of the common path of A and B.

The results confirm Michotte's analysis *entraînement* and place it between two fundamental types of perceptual organization: pre-integration and segregation. In pre-integration, B appears as if it already were part of Group A, while in segregation, B seems entirely independent. *Entraînement* falls midway between these since B is dependent on A for its displacement while remaining an independent entity.

As to the specific problems, in (1) Thinès is able to produce entraînement without the prior static presence of B, but only if there is fixation of the place at which B appears. This expectation seems to confer upon B a phenomenal object status, thus acting as a force against pre-integration. With regard to (2), Thinès finds that there is a minimum common path necessary for *entraînement* and that within certain upper and lower limits, the extent of this path varies positively with the speed of the objects. It should be pointed out, however, that in this latter study the experimental groups are very small and composed largely of experienced subjects. This is unfortunate in an area in which subjective factors are so important.

Cornell University
VIVIAN OLUM

*Creative Management.* By NORMAN R. F. MAIER and JOHN J. HAYES. New York, John Wiley & Sons, 1962. Pp. vii, 226. $5.95.

This is primarily an account of three conferences, "The Problem-Solving Conference," "The Pseudodemocratic Conference" and "The Informational Conference." The conferees in each of the three conferences thought that they were "for real." They were not. The conferees accepted the results of the problem-solving conference better than the results of the pseudodemocratic conference which in turn had results that were better accepted than those of the informational conference. Some conferees could tell that the pseudodemocratic conference was not truly democratic. The topic was the same in each of the three conferences, i.e., how to best spend a certain amount of money budgeted to make a follow-up on the results of a management training program. The conference leader was the same. The conferees were different. In each instance the participants were supervisors or managers—presumably for United Air Lines, the full time employer of Mr. Hayes, and the part time employer of Professor Maier. The reactions of conferees on a questionnaire showed results to be decidedly inferior for the informational conference. The democratic method was most effective, but the pseudodemocratic had distinct positive reactions, suggesting that even some participation is much more desirable than none.

There is an answer in this book to Whyte's attack in *Organizational Man* on group thinking. Maier and Hayes point out that Whyte has not committed himself to tell what he favors—only what he opposes. What is the alternative to democratic leadership in large organizations? It is either the autocratic, which is most common, or anarchy.

This is an interesting book in the series of Norman Maier books. It is somewhat similar to the *Appraisal Interview* in consisting mainly of the analysis of a role-playing situation, although here the participants, except for the conference leader, did not know that they were role-playing.

Stanford University THOMAS W. HARRELL

*Vocational Interests of Nonprofessional Men.* By KENNETH E. CLARK. Minneapolis, University of Minnesota Press, 1961. Pp. xi, 129. $3.75.

This book is a report of a research program of more than ten years' duration. The research has been concerned with the measurement of the vocational interests of persons employed in skilled and technical level occupations. Specifically, the report is the "biography of the Minnesota Vocational Interest Inventory" (p. 9).

In at least two respects this is an important book. In the first place, it helps to fill the void in knowledge concerning the vocational interests of the large proportion of our population who do not attend college. Even though Dr. Clark's findings are based largely on Naval personnel and are still quite incomplete with respect to demonstrating that they are relevant to civilian populations, he has demonstrated rather conclusively that the interests of nonprofessional men can be measured reliably and that these interests have significance for career planning.

This monograph is also important in that the author discusses, in a sophisticated manner, many basic issues in the measurement of interests, issues which are equally applicable to the measurement of other non-cognitive characteristics. Treated are topics such as homogeneous vs. heterogeneous scales, differed vs. unit weighting, and the number of items necessary to secure reliable scales.

Within the context of the technical aspects of the report, the researcher will find many insightful observations concerning the interest domain. Of special interest is the final chapter entitled "Toward improved interest measures." In this chapter, the author indicates some necessary next steps in the process of exploring the meaning and measurement of vocational interests.

This report is one that can be read with profit by a wide variety of individuals; by researchers who are interested in devising better measures of interests, by theorists who are interested in the development and meaning of vocational interests, and by counselors who are concerned with vocational interests as a major aspect of career choice.

University of California, Berkeley LAWRENCE H. STEWART

*Investigation of Carbon Dioxide Therapy.* By G. K. YACORZINSKI, ARTHUR J. ATKINSON, JEROME COHEN, and FORREST G. SHUFFLEBARGER. New York, Charles C Thomas, 1962. pp. viii, 313. $9.75.

The technique of carbon dioxide therapy of the neuroses considered in this book was introduced by L. J. Meduna about fifteen years ago. On repeated occasions the patient is made to inhale a mixture of 30% carbon dioxide and 70% oxygen until he loses consciousness. Reports of results obtained hitherto have been variable and contradictory.

This book records an investigation comparing the effects of this technique on 17 spastic colitis patients with the effects of nitrous oxide similarly administered on 18 similar patients. In each group 60 treatments were given. Both groups were reassessed after the treatment and again half-yearly for 2 years.

Only 2 patients in each group were accorded a "markedly improved" psychiatric rating. Generally there was slightly, but not significantly, more clinical improvement with carbon dioxide. On the Jackman-Schorr Neurotic Index significantly more change was found immediately in the carbon dioxide group, and only after a year did the nitrous oxide group catch up. The authors devote much space to the results of a battery of muddy and meaningless projective tests, including the Rorschach.

The Jackmann-Schorr findings indicate that in some cases carbon dioxide reduces the general level of anxiety. This may well be through deconditioning anxiety responses to pervasive stimuli, as I have argued (*Psychotherapy by Reciprocal Inhibition*, 1958). Unfortunately, the authors have a "psychodynamic" background and impose the usual unvalidated assumptions upon their facts. Nevertheless they are to be commended for their attempt at a controlled study—so rare in this field.

JOSEPH WOLPE

Department of Neurology and Psychiatry
University of Virginia

*The Growth of Basic Mathematical and Scientific Concepts in Children.* By K. LOVELL, New York, Philosophical Library, 1962. Pp. 154. $7.50.

This small book undertakes a summary and critical review of findings on the development of scientific concepts in young children, with primary emphasis on British and European work. The concepts considered are those of number, substance, weight, time, space, length and measurement, area and volume, and number systems. Naturally enough, considerable attention is paid to the work of Piaget and Inhelder, but this is supplemented by consideration of other investigations of these problems, including those of the author. Each chapter summarizes lucidly what can be concluded about the capabilities of children in each of these conceptual areas, based upon observations of what they can or cannot do when presented with representative problems. This is followed in each case by a statement of implications for children's learning, and often by some useful examples.

This work provides a convenient and highly readable summary of much that is known about basic conceptual skills that appear to underlie science and mathematics education. The identification of these skills is itself an accomplishment, and it is a convenience to have them considered systematically in one volume. Although Lovell emphasizes the role of learning in concept formation to a greater extent than does Piaget, many American readers will still find this factor to be inadequately considered and accounted for; they will find themselves wishing that "learning" of scientific concepts had been studied, as opposed to their "growth." For teachers and curriculum planners, as well as for investigators of child development, this book should have considerable value.

ROBERT M. GAGNÉ

American Institute for Research

*Criminal Psychology.* Edited by RICHARD W. NICE. New York, Philosophical Library, 1962. Pp. 284. $6.00.

The title of this book, as well as its jacket illustration of two highly magnified fingerprints, is misleading. Actually, the book is a symposium mainly on forensic psychiatry and particularly on the issue of criminal responsibility as affected by mental illness or personality deviation. Only one of the contributors, the Editor, is a psychologist; of the others, five are psychiatrists, two are professors of law, and one a psychiatric social worker.

Ten years ago this volume might have interested a dozen psychologists in the United States. In the past decade, however, psychological applications to court and legal settings have multiplied. An evidence of this development is the recent questionnaire on forensic psychology sent out by the A.P.A. Board of Professional Affairs. While few psychologists are specifically involved in court tests of the Durham decision on criminal responsibility, many of them now have a direct interest in the widely ramified area of psychology and the law.

Mr. Nice, Clinical Psychologist at the New Jersey State Reformatory for Men, has assembled here some well chosen articles in a relatively new and specialized field. The authors represent a Who's Who in forensic psychiatry, including Henry A. Davidson, Manfred Guttmacher, and William Haines. Mr. Nice's own theoretical approach to the field is not especially clear from his introduction; nevertheless, he has performed an editorial service in making these contributions readily accessible. Of widest interest to psychologists will be the two articles on treatment of offenders, and David W. Louisell's sophisticated guide to "The Psychologist in Today's Legal World."

Psychiatric Institute, Municipal Court of Chicago A. ARTHUR HARTMAN

*The Q-Sort Method in Personality Assessment and Psychiatric Research.* By JACK BLOCK. Springfield, Illinois. Charles C Thomas, 1961. Pp. viii, 161. $6.75.

The monograph offers a new language instrument, the California *Q*-set. Block's goal is to provide a tool that will permit a comprehensive description in psychodynamic terms of an individual's personality, suitable for quantitative comparison and analysis. The instrument, the product of seven years of effort, consists of 100 descriptive statements together with instructions for arranging or sorting them in order of representativeness for the individual. The construction of the *CQ* items is presented and there is an excellent chapter on the methodology of *Q*-sort. Whether sorts should be free or forced-choice and the form of the distribution are set forth in careful detail. There is also a chapter on research application.

While recognizing the many merits of the *CQ* set and its potential for exploratory studies the reviewer believes the instrument lacks a certain type of precision. No theoretically formulated or statistically established set of personality concepts is assessed. A standard vocabulary is certainly needed but when it is constructed it will assess specified dimensions and it will not be person-centered. Perhaps the final judgment rests on what *CQ* set can generate that is of genuine general psychological import.

Block has a refreshing zestful style. Readers interested in the clinical and research application of personality measures will find the monograph not only easy reading but a fascinating source of information. For those who believe in the ipsative approach to personality research this is a "must" book.

Outpatient Psychiatric Research Laboratory, MAURICE LORR
Veteran's Administration

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(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review.)

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# THE EFFECTS OF FIXATION UPON THE APPARENT MAGNITUDE OF BOUNDED HORIZONTAL EXTENT

By ROBERT H. POLLACK, Institute for Juvenile Research,
Chicago, Illinois

The orientation of the boundaries of figures as a possible determinant of post-fixation effects has not been studied systematically. There are numerous passing references to this variable in the literature on figural after-effects which suggest that it might play a significant role in determining the direction and size of such after-effects. The research reported here was designed to provide a possible theoretical explanation for the operation of boundary-orientation, as well as to demonstrate its effects both empirically and systematically.

In his investigation of the Delboeuf illusion, Ogasawara demonstrated not only the usual illusory effect of the apparent growth of the circumscribed circle, but the apparent shrinkage of the circumscribing circle as well.[1] The results were interpreted as showing a mutual attraction of concentric circles. Earlier, Köhler and Wallach, Walthall, and more recently Day and Logan, showed that prolonged fixation of a small circle resulted in its diminution in apparent size.[2] Köhler and Wallach also demonstrated such shrinkage in a fixated square accompanied by distortion in the form of the bowing-in of the sides. George demonstrated a mutual attraction of more closely placed parallel vertical lines following fixation, even to the extent of a merger.[3] At the same time, Day's work on the Müller-Lyer illusion

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[1] Jiei Ogasawara, Displacement after-effect of concentric circles, *Jap. J. Psychol.*, 22, 1952, 224-234.

[2] Wolfgang Köhler, and Hans Wallach, Figural after-effects; an investigation of visual processes, *Proc. Amer. phil. Soc.*, 88, 1944, 269-357, esp. 271-351; W. J. Walthall, Jr., The Köhler effect, this JOURNAL, 59, 1946, 152-155; R. H. Day and J. A. Logan, A further investigation of apparent size and retinal size as determinants of the figural after-effect, *Quart. J. exp. Psychol.*, 13, 1961, 193-203, esp. 200-202.

[3] F. H. George, Acuity and the statistical theory of figural after-effects, *J. exp. Psychol.*, 63, 1962, 423-425.

showed that horizontal extents bounded by bent line or angular contours did not diminish, but in some cases tended to expand.[4] Thus, over-all, there appears to be considerable evidence that parallel or at least non-intersecting contours, separated by visual angle distances of up to 1° or more, tend to attract each other—especially following fixation. Angular contours separated by similar distances tend not to be so attracted. These two phenomena are illustrated in sharp contrast in the shape-distortion of the fixated square in the Köhler and Wallach study. Here, as was stated above, the parallel sides bowed in, while the angles tended to hold their positions.

There appears to be a parallel to these phenomena in the study reported by Fry and Bartley and interpreted by Bartley.[5] The findings were that the presence of a contour inhibited the appearance of a new neighboring parallel contour as given by brightness-contrast, while the appearance of an adjacent intersecting contour was facilitated. Just what the determinants of these opposing interactions are was not stated by Bartley, who made an empirical generalization only; but the relationship to contour-displacement is striking. It is possible, however, that the terms 'facilitation' and 'inhibition' are appropriate, but not in the sense intended by Bartley. An examination of the hypothetical neurological mechanism posited by Osgood and Heyer in their statistical theory of figural after-effects may afford a plausible explanation.[6] If, as they state, a line-contour is represented by a ridge of neural activity on the receptor- or projection-surface, with activity spreading through time at right angles to the peak of the ridge, then new, weak, neighboring activity would be expected to be recruited into the ongoing established distribution of firing. The new activity would tend to spread the distribution on its side, flatten the peak and, perhaps, displace the peak somewhat toward it. As the new contour is intensified physically, its own neural activity also increases, ultimately producing a distinct peaked ridge, and thus it appears as a perceptual response. When parallel contours of equal length are presented simultaneously, as was the case in George's work, the firing of the tissue between the peaks is facilitated, gradually resulting in the formation of a single distribution with its blunt peak between the original ridges or, perceptually, as a gradual merger of the two contours. Even without the merger, the displacement of the peaks toward each other would be expected along with the flattening, which is another name for adaptation. Evidence for the flattening of the distribution is given by the reported loss of contrast of figures with their grounds following prolonged fixation.[7] When contours intersect at right angles, the spread of activity for each will meet at right angles. Perhaps such circumstances tend to inhibit rather than to facilitate further spread. Indeed, it may be that the only place where expansion of activity could take place under such conditions would be along the peak of the ridge of each distribution, thus lengthening and, perhaps, displacing the contours away from each other. Ghoneim has reported the over-estimation of the angles of a diamond.[8] The existence of these facilitatory and inhibitory mechanisms, depending upon the orientation of contours, is purely speculative but it does serve to account for both the findings of Fry and Bartley, and for those of contour-displacement. Inhibition would either concentrate the activity of the new contour along its peak, making its appearance more likely, or prevent attraction displacements.

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[4] Day, The effects of repeated trials and prolonged fixation on error in the Müller-Lyer figure, *Psychol. Monogr.*, 76, 1962, 1-19, esp. 4-6.

[5] G. A. Fry and S. H. Bartley, The effect of one border in the visual field upon the threshold of another, *Amer. J. Physiol.*, 112, 1935, 414-421; Bartley, *Vision: A Study of Its Basis*, 1941, 232-235.

[6] C. E. Osgood and A. W. Heyer, Jr., A new interpretation of figural after-effects, *Psychol. Rev.*, 59, 1952, 98-118.

[7] Köhler and Wallach, *op. cit.*, esp. 271-351.

If the explanation offered is valid, it would predict the effect of fixation upon horizontal extents as a continuous function of the orientation of contours. These would range from no effect when the contours are aligned, through maximal repulsion when they form an angle of 90°, to a maximal attraction when they are parallel. It should also account for special effects with boundaries which are made up of more than a single contour or are made up of arcs. Finally, it should predict changes in the size of small figures as a function of shape. All these predictions are investigated below.

## EXPERIMENT I: ORIENTATION OF SINGLE-LINE BOUNDARIES

In Experiment I, a study was made of the effects of the orientation of single-line boundaries upon apparent horizontal extent following fixation. It was predicted that, as the angle between the boundaries changed from alignment to 90°, fixation would produce an enhancement of horizontal extent; but as the angle was varied further toward parallelness, the enhancement would decrease and become diminution. This diminution would be maximal when the boundaries were parallel.

*Subjects.* The *S*s were 90 Columbia undergraduate volunteers divided at random into groups of 10, each of which viewed the standard at 1 of the 9 orientations.

*Apparatus.* The apparatus (see Fig. 1) consisted of a black anodized aluminum sheet 35 cm. square, mounted vertically at its center on a vertical supporting column fixed in a horizontal base. The mounting itself was a locking pivot to permit 180° rotations of the sheet. At the center of the sheet, a white dot, 2 mm. in diameter, was placed which served as a fixation-point (*FP*). Five millimeters to one side of *FP* was a 2-mm. square socket, and 27 mm. farther away from this socket was another, identical, socket. Flat metal rods, 6 x 3 mm., fixed to a 2-mm. square plug at one end and painted white served, when plugged into the sockets, as boundaries of a fixed standard distance. By using a set of such rods mounted at different angles to the plugs, the boundaries of the standard distance were presented at nine different orientations to each other: 180°, 160°, 140°, 120°, 90°, 60°, 40°, 20°, 0°. The lower edges of the rods at all settings were separated by approximately 1° of visual angle; 180° rotation of the sheet permitted presentation of the standard to the left or right of the fixation-point (*FP*).

To the other side of *FP* and 5 mm. away, was a third 2-mm. square socket. Beginning 15 mm. from this socket, and farther away, was a 15-cm. track on which was mounted a movable socket. A variable distance was provided by rods similar to those described above, mounted in these sockets at the same orientation as the standard. The movable socket was activated by a screw device connected to a rod 180 cm. long. When *S* rotated the rod, this socket moved right or left 1 mm. per three complete turns. Direct readings of variable distance were given by a pointer fixed to the rear of the movable socket which passed over a millimeter scale fixed to the back of the aluminum sheet. A screen attached by clamps to the sheet could be fitted to hide the variable distance completely.

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[8] Sayed Ghoneim, Recherches sur le développement des perceptions; les déformations perceptives du losange de l'enfant à l'adulte, *Arch. Psychol., Genève*, 37, 1959, 1-99, esp. 35-40.

*Procedure.* The apparatus was mounted on a table at *S*'s eye-height and with his eyes 180 cm. from the *FP*. In a fore-test, *S* was presented with the standard and variable distances set at objective equality; he was then requested to fixate the *FP* and to set the variable or comparison-figure (*CF*) equal to the standard by rotating the control rod appropriately. The *CF* was then covered by the screen. The new total display comprised the inspection-figure (*IF*). *S* was then asked to fixate the *FP* for 2 min. At the end of the fixation-time, the *CF* was reëxposed and *S* was requested to reset it, making it equal to the standard. This was the after-test. The difference between fore- and after-test was taken as a measure of contour-interaction, or fixation-effect. Each *S* underwent only one such sequence.

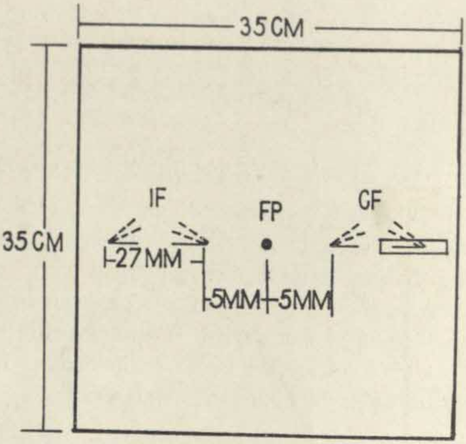

FIG. 1. VISUAL DISPLAY FOR HORIZONTAL EXTENTS WITH SINGLE-LINE BOUNDARIES

*Results.* The data were examined statistically and graphically. A single classificatory analysis of variance for 9 independent groups of 10 *S*s each was carried out for both the fore-test and the fixation-effect, *i.e.* the difference between fore-test and after-test. Prior to such analysis, the variances were first found to be homogeneous. The analysis of the fore-test yielded an *F*-ratio with a probability greater than 0.05, while the analysis of the after-effect produced an *F*-ratio of 10.24, which for 8 and 81 *df* has a probability of less than 0.01, demonstrating the differential effects of the orientations (see Table I). Fig. 2 presents the data graphically. The

TABLE I

Means and $t$-Test of Differences between Mean Fixation-Effects (mm.) and Zero

| | Orientation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 180° | 160° | 140° | 120° | 90° | 60° | 40° | 20° | 0° |
| Mean* | −0.10 | 0.35 | 0.65 | 0.90 | 1.00 | 0.70 | 0.25 | −0.25 | −1.00 |
| *SD* | 0.89 | 0.55 | 0.45 | 0.58 | 0.55 | 0.51 | 0.60 | 0.60 | 0.50 |
| $t$ | 0.36 | 2.01 | 4.58 | 4.92 | 5.75 | 4.35 | 1.32 | 1.16 | 6.33 |
| $p$† | >0.05 | <0.05 | <0.005 | <0.005 | <0.005 | <0.005 | >0.05 | >0.05 | <0.005 |

* Positive values indicate contour-repulsion; negative values attraction.

† One-tailed tests.

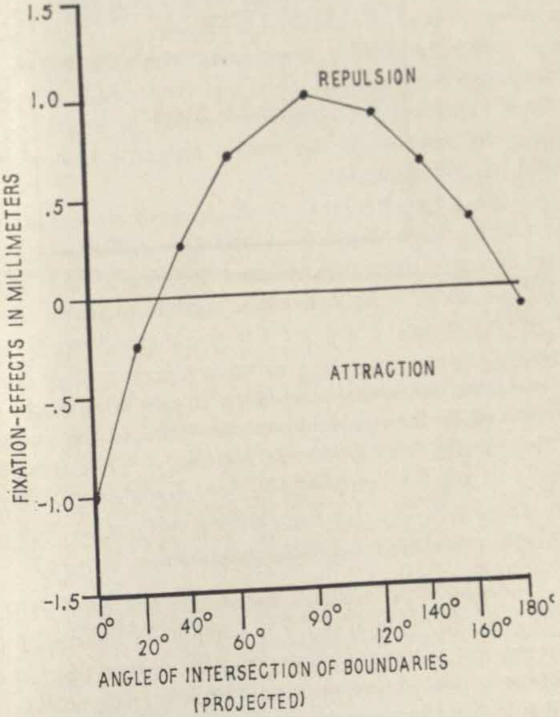

Fig. 2. Fixation-Effects as a Function of the Mutual Orientation of Single-Line Boundaries of a Horizontal Distance

abscissa is the angle of mutual orientation and the ordinate is the magnitude of displacement following fixation. Positive displacements represent repulsion of boundaries, and negative displacements represent attraction. For six of the nine orientations the deviation from zero was found to be significant by *t*-test. It can be seen clearly that repulsion increases to a maximum at 90°, then decreases and changes to attraction which is maximal when the boundaries are parallel. The prediction was thus borne out.

## EXPERIMENT II: ORIENTATION OF BENT LINE OR ANGULAR BOUNDARIES

In Experiment II, two types of boundary-configurations were used. In one case the apexes of angles pointed away from each other, thus appearing like the arrowhead or closed component of the Müller-Lyer figure. In the other case, the apexes pointed toward each other, appearing like the open component of the Müller-Lyer figure.

The first case represented a situation in which the angular opposition of contours across the horizontal space is accompanied by the opposition of parallel lines. This parallel opposition varied in extent as a function of angle-size. In other words, for certain orientations an ambivalence of forces could be said to obtain.

It was predicted, therefore, that, as the angle of orientation varied from alignment to 90°, there would be initial appreciable repulsion reaching a maximum at angles larger than 90° and declining toward attraction with angles smaller than 90°. The angles here referred to are the same as those described in Experiment I, and are not the angles between the arms of the arrowheads.

The second case represented a situation in which the angle of orientation is reinforced by the opposition of the angles of the arrowhead, and in which the parallel lines are not in opposition. Thus, it was predicted that the shape of the function would be very similar to that obtained with single-line boundaries, but that the enhancement-portion would be more leptokurtic and would have a greater maximum.

*Subjects.* The *S*s were another sample of 180 undergraduate students of Columbia and Barnard colleges, naïve with respect to the phenomenon under study, and divided at random into 18 groups of 10 *S*s each.

*Apparatus.* The apparatus was the same as that used in Experiment I, except that the boundaries were each made of two white rods 6 x 3 mm., pivoted where they joined the plug. Each rod was connected to a pointer, so centered in a calibrated circle at the back of the screen that correct angles could be set for each condition.

*Procedure.* The procedure was the same as in Experiment I.

*Results.* The data again were examined statistically and graphically (see Table II and Fig. 3). After demonstrating homogeneity of variance, the

TABLE II

MEANS AND *t*-TEST OF DIFFERENCES BETWEEN MEAN FIXATION-EFFECTS (MM.) AND ZERO

| | | Orientation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | 180° | 160° | 140° | 120° | 90° | 60° | 40° | 20° | 0° |
| Open component | Mean* | 0.00 | 0.25 | 0.55 | 1.05 | 1.30 | 0.70 | 0.20 | −0.35 | −1.05 |
| | *SD* | 0.81 | 0.75 | 0.57 | 1.04 | 0.82 | 0.64 | 0.51 | 0.45 | 0.65 |
| | *t* | 0.00 | 1.05 | 3.06 | 3.19 | 5.02 | 3.47 | 1.24 | 2.46 | 5.10 |
| | *p*† | >0.05 | >0.05 | <0.01 | <0.01 | <0.0005 | <0.005 | >0.05 | <0.025 | <0.0005 |
| Closed component | Mean* | 0.00 | 0.60 | 0.40 | 0.20 | −0.10 | −0.50 | −0.65 | −0.85 | −1.05 |
| | *SD* | 0.50 | 0.73 | 0.80 | 0.87 | 0.83 | 0.67 | 0.78 | 0.63 | 0.57 |
| | *t* | 0.00 | 2.60 | 1.58 | 0.73 | 0.38 | 2.36 | 2.64 | 4.27 | 5.83 |
| | *p*† | >0.05 | <0.025 | >0.05 | >0.05 | >0.05 | <0.025 | <0.025 | <0.005 | <0.000 |

* Positive values indicate contour-repulsion; negative values attraction.
† One-tailed tests.

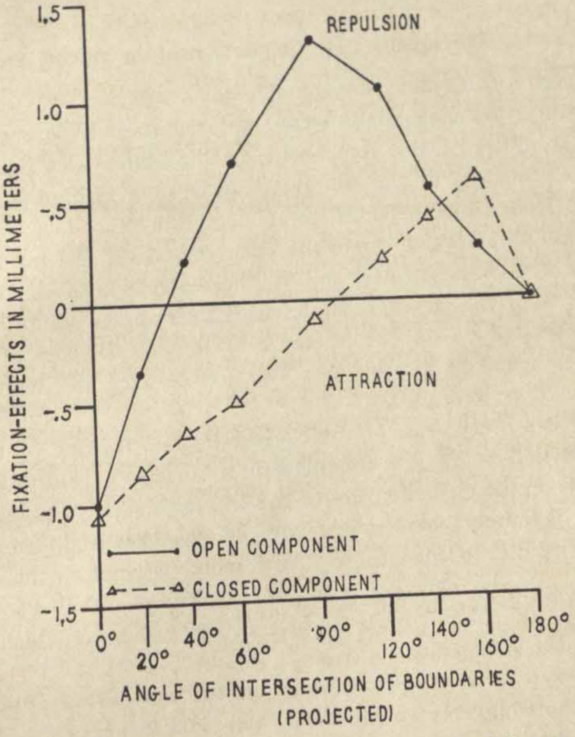

FIG. 3. FIXATION-EFFECTS AS A FUNCTION OF THE MUTUAL ORIENTATION OF ANGULAR BOUNDARIES OF A HORIZONTAL DISTANCE

fore-test and fixation-effects for each configuration were examined in double classification analyses of variance. As in Experiment I, the fore-test yielded *F*s with probabilities $> 0.05$; while the fixation-effects produced *F*s with probabilities $< 0.001$ for both main effects of configuration and angular orientation. The probability for the interaction of these variables was $< 0.01$. The nature of the interaction can be seen clearly in Fig. 3. Both fixation effects were graphed on the same axes and showed distinct functions. The closed component of the Müller-Lyer figure yielded a straight line between 160° and 0°, showing little displacement at 90°, but showing considerable attraction beyond. The open component produced a function much like that in Experiment I, except that the 90° maximal repulsion is slightly greater and the slopes of the rise and fall are steeper. Of the 18 orientation-conditions, *t*-tests showed that 11 of the mean displacements were significantly different from zero. Again, the predictions were borne out.

## EXPERIMENT III: ORIENTATION OF ARCS

The effect of bounding horizontal extents with arcs was investigated next. Again, two configurations were employed. As in Experiment II, they could be compared to the components of the Müller-Lyer figure. When the convex surfaces of the arcs faced away from each other, the configuration looked like the closed component; when they faced each other, the configuration was similar to the open component. To maintain comparability with Experiments I and II, the designation of degrees of arc was made as if the arcs were angles with their apexes pointing horizontally and located on the circumference of the arcs. Because of technical difficulties in making variable arcs, two sets of fixed arcs with different angular settings were used.

An arc is a special case of angle-orientation. In the case of the closed component, if innumerable, infinitely short tangents were drawn to adjacent points on the circumferences of the arcs, each adjacent tangent would be at an infinitely obtuse angle with its neighbor and, across space, would be parallel to a tangent of a corresponding point on the other arc. Thus, the closed component represents a situation of almost maximal parallelness and should always produce an attraction-effect which becomes greater as the arcs flatten.

Conversely, in the open configuration the drawing of innumerable, infinitely short tangents would produce only intersection of these tangents across the horizontal extent, except for the one vertical tangent. Thus, repulsion would usually be expected, ceasing when the arcs became very flat.

*Subjects*. The *Ss* were a new sample of 40 undergraduate students of Columbia and Barnard Colleges, all naïve with respect to the phenomenon under study, and randomly divided into groups of 10 each.

*Apparatus*. The apparatus was the same as in Experiment I, except that the boundaries were arcs. They were 3 mm. thick, and had circumference-lengths of approximately 9 mm. There were two sets of arcs for each configuration, one corresponding to an orientation of 120° and the other corresponding to 60°.

*Procedure*. The procedure was the same as in Experiments I and II.

*Results*. Once more, the data were treated statistically and graphically. Following establishment of homogeneity of variance, a double classification analysis of variance was performed, comparing configurations and angular orientations as in Experiment II. As before, the fore-tests showed no significant effects of orientation, angle, or configuration. The analysis of the fixation-effect yielded an $F$ for the main effect of configuration which had a probability $< 0.001$; the main effect of angular orientation had a probability $< 0.05$; but the probability of the interaction between these factors was $> 0.05$ (see Table III and Fig. 4). The deviation of each of the fixation-effect means as compared with zero yielded significant values in three of the four separately performed $t$-tests. Thus, the predictions were once again confirmed.

TABLE III

MEANS AND $t$-TEST OF DIFFERENCES BETWEEN MEAN FIXATION-EFFECTS (MM.) AND ZERO

| | Orientation | | | |
|---|---|---|---|---|
| | *open component* | | *closed component* | |
| Degrees of | 120° | 60° | 120° | 60° |
| Mean* | 0.99 | 0.25 | −0.30 | −0.60 |
| *SD* | 0.77 | 0.68 | 0.51 | 0.49 |
| $t$ | 3.69 | 1.18 | 1.86 | 3.87 |
| $p$† | <0.005 | >0.05 | <0.05 | <0.005 |

* Positive values indicate contour-repulsion; negative values, attraction.

† One-tailed tests.

## EXPERIMENT IV: CHANGES IN MAGNITUDE OF MÜLLER-LYER ILLUSION AS A FUNCTION OF BOUNDARY ORIENTATION

By algebraically combining the effects of fixation on each component of the Müller-Lyer illusion at each orientation as found in Experiment II, it was possible to produce an empirical function of the prediction of the effects of fixation on the magnitude of illusion (see Table IV and Fig. 5). Similar prediction was made for the arcs (see Table V and Fig. 5). The initial occurrence of the illusion itself could be predicted from the orientation of the boundaries of the components. It could be expected that the open component would produce the bulk of the illusion due to the ambivalent character of the closed component. This fact was noted by Binet.[9] As can be seen for all but one setting in which an illusion was expected, the prediction was that enhancement of magnitude of the illusion would follow fixation. Such prediction runs contrary to the findings of Eysenck and Slater, and of Day, who employed fixation with figures of comparable size.[10] For the most part, both investigations revealed no significant change with repeated short fixations, but it must be pointed

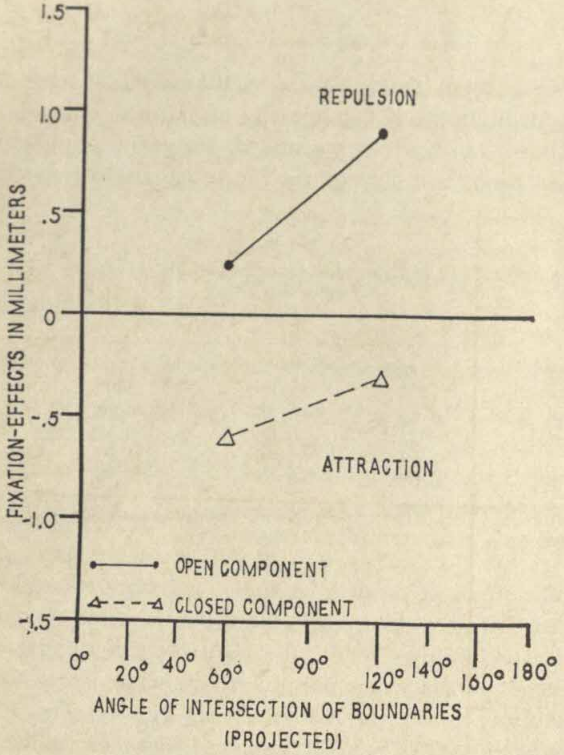

FIG. 4. FIXATION-EFFECTS AS A FUNCTION OF THE MUTUAL ORIENTATION OF ARC BOUNDARIES OF A HORIZONTAL DISTANCE

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[9] Alfred Binet, La mesure des illusions visuelles chez les enfants, *Rev. Philos.*, 40, 1895, 11-25, esp. 19-20.

[10] H. J. Eysenck and P. Slater, Effects of practice and rest on fluctuations in the Müller-Lyer illusion, *Brit. J. Psychol.*, 49, 1958, 246-256; Day, *op. cit*, 4-6.

TABLE IV

Predicted and Obtained Changes in the Magnitude (mm.) of the Müller-Lyer Illusion as a Function of Orientation of Angular Boundaries

| | Orientation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 180° | 160° | 140° | 120° | 90° | 60° | 40° | 20° | 0° |
| Predicted value* | 0.00 | −0.35 | 0.15 | 0.85 | 1.40 | 1.20 | 0.85 | 0.50 | 0.05 |
| Obtained means* | 0.00 | −0.40 | 0.20 | 0.95 | 1.50 | 1.15 | 0.70 | 0.55 | 0.10 |
| *SD* | 0.55 | 0.77 | 1.29 | 0.65 | 0.63 | 0.55 | 0.71 | 0.54 | 0.62 |
| *t* | 0.00 | 1.64 | 0.49 | 4.61 | 7.54 | 6.61 | 3.11 | 3.22 | 0.51 |
| *p*† | >0.05 | >0.05 | >0.05 | <0.005 | <0.0005 | <0.0005 | <0.01 | <0.01 | >0.05 |

* Positive values indicate increase in the magnitude of the illusion following fixation; negative values indicate decrease.
† One-tailed tests.

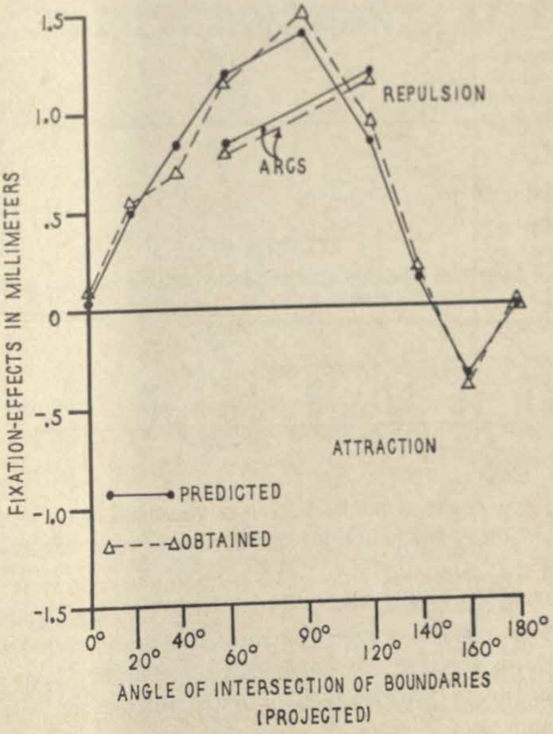

Fig. 5. Predicted and Obtained Changes Following Fixation in the Magnitude of the Müller-Lyer Illusion as a Function of the Mutual Orientation of the Angular Boundaries of the Figure

out that the Brentano version of the figure was used in those experiments where the effect of the central arrowhead belonging to both components has not yet been ascertained. In the present study, the components were on opposite sides of the central fixation-point and were separated in space.

*Subjects.* The *Ss* were another distinct group of 110 undergraduate students of Columbia and Barnard colleges, naïve with respect to the phenomenon under study. These were divided at random into 11 groups of 10 *Ss* each.

*Apparatus.* The apparatus was the same as in Experiments II and III, except that the open component was to one side of *FP* and the closed component to the other.

TABLE V

PREDICTED AND OBTAINED CHANGES IN THE MAGNITUDE (MM.) OF THE MÜLLER-LYER ILLUSION AS A FUNCTION OF ORIENTATION OF ARC BOUNDARIES

| Orientation | 120° | 60° |
|---|---|---|
| Predicted value | 1.20 | 0.85 |
| Obtained mean | 1.15 | 0.80 |
| *SD* | 0.45 | 0.71 |
| *t* | 10.36 | 3.56 |
| *p** | <0.0005 | <0.005 |

* One-tailed tests.

TABLE VI

MÜLLER-LYER ILLUSION (ANGULAR VERSION): MEANS AND *t*-TEST RESULTS OF INITIAL ILLUSION MAGNITUDE

| | Orientation | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 180° | 160° | 140° | 120° | 90° | 60° | 40° | 20° | 0° |
| Mean | 0.30 | 1.95 | 6.30 | 6.00 | 4.35 | 3.65 | 3.05 | 2.00 | −0.10 |
| *SD* | 1.10 | 1.29 | 1.07 | 1.22 | 0.93 | 1.01 | 1.52 | 1.34 | 0.66 |
| *t* | 0.86 | 4.81 | 18.64 | 15.54 | 14.03 | 11.44 | 6.34 | 4.72 | 0.48 |
| *p** | >0.05 | <0.005 | <0.00005 | <0.00005 | <0.00005 | <0.00005 | <0.00005 | <0.005 | >0.05 |

* One-tailed tests.

*Procedure.* The procedure was the same as in Experiments I, II, and III, except that no part of the display was covered during the sequence of fore-test, inspection, and after-test.

*Results.* The data were again treated statistically and graphically. A glance at Fig. 5 shows a close agreement between predicted values and observed values for each orientation. After establishing homogeneity of variance, significant variation both of initial illusion and magnitude of illusion-change as a function of orientation was found for the angular configuration. In addition, all seven settings where initial illusion was anticipated produced significant deviations from physical equality, as expected. Significant deviation was discovered in five out of nine fixation-effects (see Table VI).

In the case of the arcs, analyses of variance, following establishment of homogeneity of variance, showed significant variability of initial illusion as a function of orientation, but not of changes in the magnitude of illusion following fixation. All individual *t*-tests of initial illusion-magnitude, different from physical equality and amount of change due to fixation, yielded significant *t*-values (see Table VII). All predictions were confirmed.

TABLE VII

Summary of Analyses of Variance of Two Orientations of the Müller Lyer Illusion (Arc Version): Means and *t*-Test Results of Initial Magnitude of the Illusion

| Orientation | 120° | 60° |
|---|---|---|
| Mean | 1.35 | 1.70 |
| *SD* | 1.22 | 1.58 |
| *t* | 3.50 | 3.40 |
| *p** | <0.005 | <0.005 |

* One-tailed tests.

## Experiment V: The Influence of Shape upon Size-Changes Following Fixation of Small Geometric Figures

The postulated hypothetical mechanisms and the establishment of differential contour-displacements following fixation as a function of contour-orientation led to the prediction that enclosing boundaries of extent would not change fixation-effects. Thus it was predicted that circles which represent the ultimate in non-intersecting opposed contour would shrink following fixation, a fact already established by other investigators; while triangles, representing intersecting contours only, would not shrink and perhaps might even expand.[11]

*Subjects.* The *S*s were 60 undergraduate students of Barnard College, naïve with respect to the phenomenon under study, and divided at random into 4 groups of 15 *S*s each.

*Apparatus.* The apparatus consisted of eight white cards, 8 x 6 in., on which the stimulus-figures were drawn in India ink. Each card had a black dot at its center, which served as the *FP*. One card contained a pair of filled-in circles, 0.75 in. in diameter, placed with centers 1 in. to the right and left of *FP*. These circles served as test-figure (*TF*) and comparison-figure (*CF*). The second card had one such circle and served as inspection-figure (*IF*). A similar pair of cards was made containing outline circle; a third pair with filled-in equilateral triangles, 0.75 in. on each side; and a fourth pair with similar outline triangles. The cards were placed vertically against a stand, 36 in. from *S*'s eyes.

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[11] Köhler and Wallach, *op. cit.*, 351; Walthall, *op. cit.*, 152-155; Day and Logan, *op. cit.*, 102-108.

*Procedure.* The procedure was the same as in Experiments I—IV, except that the *Ss* now reported verbally and qualitatively on the relative sizes of *TF* and *CF*.

*Results.* The fixation-effects for the four sets of figures are given (see Table VIII). Since by inspection the results for outline and filled-in figures did not differ very markedly, and since *N* was small for a 3 × 4 Chi-square analysis, these were combined.

Analysis of circles versus triangles provided a Chi-square value of 27.80, including the correction for continuity, which for 2 *df* has a probability of $< 0.01$. The coefficient of contingency was 0.56, showing a marked correlation between shape and direction of fixation-effect. Thus, the final prediction was also confirmed.

TABLE VIII

FREQUENCIES OF RESPONSE-CHANGES FOLLOWING FIXATION OF SMALL GEOMETRIC FIGURES WITH VALUES OF CHI SQUARE AND *C* OBTAINED

| Figures | Response changes* | | | |
|---|---|---|---|---|
| | larger | equal | smaller | totals |
| Triangles | 14 | 11 | 5 | 30 |
| Circles | 0 | 4 | 26 | 30 |
| Totals | 14 | 15 | 31 | 60 |

* Chi square=27.80; $df=2$; $p<0.001$; $C=0.56$.

## DISCUSSION

The results of the experiments have clearly demonstrated three salient facts: First, the type of adaptation-theory proposed by Köhler and Wallach, with its emphasis on the inevitable repulsion of neighboring contours, is untenable as it stands.[12] Such a finding is not new. They, however, never explained the shrinkage of circles and squares they themselves discovered. Secondly, the Osgood-Heyer theory with certain modifications can, as George and the present investigator have shown, account for fixation-effects.[13] Thirdly, as Fry and Bartley discovered in their work on contour-thresholds, the mutual orientation of two contours is of vital importance, both the direction and magnitude of displacements following fixation being determined by it.[14] Whether the extremely sketchy and extremely speculative account of underlying mechanisms presented in the introduction to this article has any validity is debatable, but it has served the purpose of generating testable expectations which were confirmed.

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[12] Köhler and Wallach, *op. cit.*, 348-356.

[13] Osgood and Heyer, *op. cit.*, 98-118; George, *op. cit.*, 423-425.

[14] Fry and Bartley, *op. cit.*, 414-421.

The results of all earlier work on figural after-effects in which the inspection-figure was varied in some way—such as duration, contrast, and size—have shown indirectly that fixation-effects probably have some relatively simple peripheral basis.[15] It has been shown, also, that fixation-effects or simultaneous effects, and after-effects or successive effects, using the same stimulus-configuration, produce displacements in opposite directions.[16] These findings indicate that the formulation of Gibson, which describes such phenomena as adaptational with negative after-effect or contrast, may have something to recommend it.[17] That is, adaptation in the receptor system takes place during fixation and produces an observable behavioral change. A contrast or negative after-effect occurs following the withdrawal of the inspection-stimulus and its replacement by a discriminably different new stimulus. The previous adaptation with its behavior-correlate is necessary to the appearance of the after-effect, but it is probably not the only determinant. The sequence of withdrawal of the first stimulus followed by the presentation of a second one is introduced, resulting in some kind of integration of these two events through time. Such a sequence may require more than the mere persistence of activity resulting from the inspection-stimulus. An indication of such a possibility is the opposition of developmental trends of simultaneous and successive illusory effects using the same stimuli as shown by Piaget *et al.*[18] In 1958, Spitz and Blackman showed that a simultaneous effect is not affected by intelligence; while the same authors demonstrated in 1959 intellectual influence upon a successive effect.[19]

All these data, it would seem, tend to show the relative binding of simultaneous or fixation-effects to the adaptation-potential of the visual

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[15] E. R. Hammer, Temporal factors in figural after-effects, this JOURNAL, 62, 1949, 337-354, esp. 348-351; E. H. Graham, Figural after-effects as functions of contrast, area, and luminance of the inspection-figure, *Psychologia,* 4, 1961, 201-208.

[16] Jean Piaget, Marc Lambercier, Ernest Boesch, and Barbara von Albertini, Recherches sur le développement des perceptions; I. Introduction à l'ètude des perceptions chez l'enfant et analyse d'une illusion relative à la perception visuelle de cercles concentriques (Delboeuf), *Arch. Psychol., Genève,* 29, 1942, 1-107, esp. 18-19; Piaget and Lambercier, Recherches sur le développement des perceptions: V. Essai sur un effet d'"Einstellung" survenant au cours de perceptions visuelles, successives (effet Usnadze), *ibid.,* 30, 1944, 139-196, esp. 158-163; Hisako Ikeda and Torao Obonai, Studies in figural after-effects: IV. The contrast-confluence illusion of concentric circles and the figural after-effect, *Jap. psychol. Res.,* 2, 1955, 17-23.

[17] J. J. Gibson, Adaptation with negative after-effect, *Psychol. Rev.,* 44, 1937, 222-244, esp. 222-226 and 240-243.

[18] Piaget *et al., op. cit.,* 18-19; Piaget and Lambercier, *op. cit.,* 158-163.

[19] H. H. Spitz and L. S. Blackman, Studies in mental retardation: I. A comparison of mental retardates and normals on visual figural after-effects and reversible figures, *J. abnorm. soc. Psychol.,* 58, 1959, 105-110; The Müller-Lyer illusion in retardates and normals, *Percept. mot. Skills,* 8, 1958, 219-225.

receptor, as compared with the possible links after-effects might have with higher activity. It has been shown by Ohwaki, for example, that the measured sizes of after-images of illusory figures conform to the verbal reports of illusion rather than to physical size, implying the operation of a local retinal mechanism.[20] The physical correlates of fixation- or simultaneous effects are those which enhance or weaken perceptual behavior, indicating the presence of contour. Such effects, at least those represented by illusions, tend to decline through childhood; they also seem independent of intellectual capacity. Perhaps the adaptation-potential of the receptor-system declines with chronological age. Perhaps there is an increasing level of permanent adaptation built up, dependent only upon prolonged and continuous pattern-stimulation. A hint of such a possibility has been given in studies of pattern-deprivation, in that color-saturation and brightness appear enhanced following deprivation.[21] It would seem, therefore, that investigations might profitably be turned to studies of contour-thresholds, based on contrast as a function of chronological age and intelligence. A finding of rising thresholds with age, independent of intelligence, would serve to support the reasoning in this paper, and would provide the foundation of a single and, perhaps, simple account for a considerable segment of perceptual phenomena. It would, as well, segregate them from apparently similar phenomena which probably require the postulation of additional, more complex mechanisms for their operation.

## SUMMARY

A highly speculative, hypothetical, physiological mechanism was proposed to account for the following perceptual phenomena: (a) the effect of one contour on the appearance of another; (b) geometrical illusions involving contour displacement; and (c) fixation-effects of contour displacement. The postulation of this mechanism led to predictions concerning the effects of systematic variation of contour orientations in a variety of bounded horizontal extents on the magnitude of these extents following prolonged fixation. Predictions were made as well, concerning the effect of fixation and orientation upon the Müller-Lyer illusion, and on the interaction of shape and fixation in changing the apparent size of small geometrical figures. All the predictions were confirmed, and the results were discussed in relation to a possible extension of the theory to problems of perceptual development.

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[20] Sonoko Ohwaki, On the after-image of geometrical illusion, *Percept. mot. Skills*, 13, 1961, 326.

[21] Woodburn Heron, B. H. Doane, and T. H. Scott, Visual disturbances after prolonged perceptual isolation, *Canad. J. Psychol.*, 10, 1956, 13-18.

# SUPPRESSION AND FUSION IN VIEWING COMPLEX STEREOGRAMS

By LLOYD KAUFMAN, Sudbury, Massachusetts

Julesz has demonstrated an interesting stereoscopic effect with computer-generated dot-patterns.[1] One member of the stereo-pair contains a random array of dots in a rectangular matrix. The second member is identical to the first except that an inner rectangular portion of the array is shifted toward one side. An inner rectangular form is not recognizable or visible within either monocular array. One does become visible when the patterns are viewed in a stereoscope.

Form-perception in these patterns is based not only upon the fact that the binocularly perceived form is at a different level of depth than the background, but also occurs when the displays are rotated 90° in the stereoscope so that the dots exhibit vertical disparities. It is well known that vertical disparity does not produce a depth-effect, yet the rectangular form is seen. Also, when the inner portions of both patterns are comprised of independently selected dots, thereby eliminating any consistent transverse or vertical disparities, a form is still seen. We must, therefore, distinguish between form in depth and form without depth.

Perception of binocular form without depth may be the result of either vertical or transvere disparities, or some combination of these. A possible explanation of binocular form-perception without depth is that it is due to a differential effect of binocular rivalry in the superposed patterns. There is some evidence to support this proposal. First, Wallach and Adams have shown that rivalry will occur when commensurable areas of different surface-color are superposed.[2] Since dots of different surface-color are superposed in disparate regions of these displays, the occurrence of rivalry may be predicted. Secondly, Julesz has shown that when one member of a stereo-pair is apparently blurred, the binocularly perceived

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[1] Bela Julesz, Binocular depth perception and pattern recognition. In Colin Cherry (ed.), *Information Theory,* Fourth London Symposium, 1961, 212. Also see B. W. White, Stimulus-conditions affecting a recently discovered stereoscopic effect, this JOURNAL, 75, 1962, 411.

[2] Hans Wallach, and Pauline Adams, Binocular rivalry of achromatic colors, this JOURNAL, 67, 1954, 513.

display usually corresponds to the sharper member of the pair.[3] Both form- and depth-perception are present in spite of the apparent suppression.

The purpose of this paper is to describe some experiments which tested this proposal and also revealed some interesting phenomena associated with stereoscopic vision. The relation of these phenomena to the classical concept of fusion will be considered.

## EXPERIMENT I

Experiment I was performed to test the proposition that suppression is, in fact, present in the binocularly-viewed dot-patterns. It was assumed that if substantial suppression were present in these patterns, then the apparent dot-numerosity in disparate regions would closely approximate the apparent numerosity in non-disparate regions. In the absence of suppression, the dot-numerosity or density in non-disparate regions would be less than that of disparate regions.

*Method and procedure.* The patterns used in this experiment consisted of random arrays of black dots on a white background.[4] The white background of one display was divided into a matrix 65 cells wide and 65 cells high. Seventy-five percent of the cells contained black dots in a random sequence. The other member of the stereo-pair differed from the first in that the upper half of the display was shifted horizontally two cell spaces toward one side. The displays were reduced photographically from an original 6 x 6-in. size to 1.7 x 1.7 in. When the displays are viewed in a stereoscope, a field of dots divided into two halves may be seen. One half appears closer than the other when the displays are so oriented that they have transverse disparities. This division is not visible in either display alone.

When the upper half of the stereoscopic field is fixated, its dots will be in register, while dots in the lower half will be out of register. In point of fact, there will be about 20% more dots in the lower than in the upper part of the field under this condition. This is illustrated in Fig. 1, which shows a representation of the two fields superposed physically. The objective of this experiment was to determine if the greater numerosity of dots in the disparate region of the display was reduced by the presence of suppression.

To obtain subjective impressions of the relative numerosities of dots in the disparate and non-disparate regions of the displays, comparison-stimuli were also prepared. These were exactly the same as the stimuli described above, except that they were positive photographic transparencies. The two transparencies were superposed between two glass slides and mounted on a rear illuminated frosted-glass screen. The comparison-stimulus was placed near a Keystone Telebinocular which was used by *O* to view the stereograms. *O* looked back and forth from the stereoscope to the comparison-slide to make relative dot-density judgments.

Two viewing conditions were employed. In one condition, the disparity was transverse and *O* saw the two dot-regions in depth. The two regions were separated by a horizontal subjective border. In the other condition, the patterns were so rotated 90° in the stereoscope that the disparity was vertical. The division between the two halves of the field was vertical and no depth was possible. In the transverse disparity case, the comparison-slide was oriented so that its dividing line was vertical. Also, the dividing line between the more numerous and less numerous portions of the comparison-stimulus was horizontal when the stereo-display exhibited vertical disparities. This was done to reduce the chances that matches would be based on the recognition of dot-configuration rather than on the basis of dot-density.

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[3] Julesz, *op. cit.*, 216.

[4] The patterns were provided by B. W. White of the MIT Lincoln Laboratories.

*O* was instructed to fixate one half of the field and then to compare the other half of the field with both halves of the comparison-stimulus. He was to judge which half of the comparison-field most closely resembled the non-fixated half of the stereo-display in terms of the numerosity of dots per unit-area. He was also instructed to make his judgment only when the lower half of the stereo-display was seen in depth in the transverse disparity-condition. In the case of vertical disparity, he was instructed to make his judgment only when the field appeared to be divided into two portions. The *O*s were given prior experience with other patterns so that they would be able to achieve good binocular register in the experimental patterns. In addition to this procedure, they were permitted to make comparisons with free regard of the stereo-display.

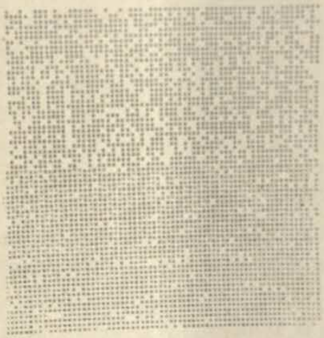

FIG. 1. THE TWO DOT-FIELDS OF EXPERIMENT I SUPERPOSED PHYSICALLY

Showing the increased density of dots in the disparate region.

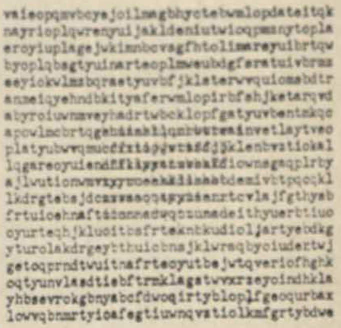

FIG. 2. HORIZONTALLY-CORRELATED LETTER-PATTERNS OF EXPERIMENT III

Superposed to illustrate the binocular alignment of dissimilar elements in the treated portion of the display.

*Observers.* Seven men were employed as *O*s. All were employees of the Sperry Rand Research Center. Some had previously served in experiments on binocular rivalry in line-patterns. All had normal or corrected-to-normal vision.

*Results.* Six of the seven *O*s always reported that the less dense half of the comparison-stimulus-field most closely resembled both halves of the stereo-display. This was true when the fixation was maintained and also in the case of, free regard. One *O* reported that one half of the field resembled the denser portion of the comparison-stimulus, while the other half resembled the less dense portion. In no case did an *O* report that the stereo-display looked exactly like the comparison-stimulus. Further comparisons by the author and another sophisticated *O* were in agreement with the six naïve *Os*. It must be concluded that dot-fields which are out of register do not appear to have substantially more dots than those which are in register.

A further observation may be made. It was apparent to all the *Os* that with vertical disparities one half of the field appeared busy or unstable. The suppression seemed unstable, as it would if binocular rivalry were present. This was not so in the presence of transverse disparities. Both halves of the field appeared stable. Some instability, however, was noticeable in one half of the field prior to the appearance of a depth-response.

## EXPERIMENT II

Experiment I showed that dot-numerosity was no greater in disparate regions of the displays than in non-disparate regions. It might be argued that this is due to fusion rather than to suppression. The disparate stimulus-representations might actually so shift as to come into register or better spatial correlation with or without a depth-effect. In spite of the fact that Ogle has shown that fusion is not necessary for stereopsis, this argument has a certain logical force.[5] This stems from the fact that disparity is the basis for stereopsis. If half-images are suppressed, then information upon which stereopsis is based may not be available to produce a depth-effect. Before a suppression-theory of binocular vision can have plausibility, it must be shown that stereopsis can, in fact, occur in the presence of patent rivalry. This need furnished the basis for Experiment II.

*Method and procedure.* The regular dot-pattern described by Julesz was copied photographically from one of his reports. When the dot-patterns are fused in a stereoscope, an inner square is seen in depth. Two Spectracoat Monopass Filters manufactured by Optics Technology Inc. were mounted on the Telebinocular. One of the filters peaked at 5330 Å and the other at 6660 Å. This caused one of the displays to reflect only green light to one eye and the second display to reflect only red light to the other eye. Binocular viewing through these filters of patterned stimuli illuminated by a tungsten lamp produces binocular rivalry. The hypothesis tested here was that even though marked color-induced rivalry is present, the stereoscopic form- and depth-perception can be perceived in the dot-patterns.

Eight men were used as *Os*; six of them were employees of the Research Center and two were visiting psychologists.

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[5] K. N. Ogle, Precision and validity of stereoscopic depth perception from double images, *J. opt. Soc. Amer.*, 43, 1953, 906.

*Results.* All of the *Os* reported the stereoscopic effect. The inner square was stable and clear in form. All also reported a marked rivalry in the sense that the apparent color of the display ranged from all red to all green. At intermediate levels, the display appeared to be comprised of red and green patches. This confirms the hypothesis that, in spite of marked and changing pattern of suppression, a stable binocular perception of form can be achieved. Since the array of light and dark elements in the display is not affected by the color-rivalry, the explanation of binocular perception of form by rivalry is not affected by this result. Where portions of the display are in register, the color-induced ocular alternation does not affect the apparent stability of light and dark elements. Where portions of the display are out of register, ocular alternation results in a changing pattern of light and dark elements.

## EXPERIMENT III

Stereoscopic effects like those obtained with dot-patterns can also be obtained with matrices of letters of the alphabet. Letter-patterns have an unique advantage over dot-patterns. The *Os* can read the letters of the patterns, thus allowing an evaluation of what is happening at each element of the display under different stimulus-conditions. The purpose of this experiment was to utilize this particular feature of the displays to verify the occurrence of suppression and to test the notion that binocular perception of form, in the absence of depth-perception, is due to a differential effect of rivalry in the binocular field. An ancillary purpose was to verify the observation that transverse disparities yield less alternation of suppression than do vertical disparities.

*Method and procedure.* Three basic patterns were used. The first consisted of a matrix of lower case letters selected haphazardly from the keyboard of a standard typewriter. The letters were arranged in a rectangular array 40 letters wide and 18 rows high. Two such matrices were constructed. They were identical, except that an inner portion of one matrix was shifted one letter space toward one side of the display and the displaced column of letters moved to the opposite side of the shifted array. The treated inner region was 16 letters wide and 6 rows high. The patterns were reduced to half-size photographically. The over-all dimensions were 1.8 in. wide and 1.6 in. high. The vertical separation between rows of letters was about 0.05 in. and the horizontal separation between letters was about 0.02 in. Letter-heights were in the range of from 0.03 in. to 0.05 in. Letter-widths were about 0.04 in. The patterns were viewed at the far point in the Telebinocular. When the displays were viewed in a stereoscope, an inner rectangular array of letters was seen in depth above a background of letters. Five sets of patterns containing independently selected letters were constructed in accord with this method. These are called the horizontally correlated displays.

The second pattern was similar to the first except that, instead of shifting the inner letters toward one side of the display, the letters of one display were shifted one row upward. Five sets containing the same letters as those used in the horizontally correlated displays were constructed. These are called the vertically correlated displays.

The third pattern was similar to the first two except that one member of every stereo-pair contained an inner region 16 letters wide and 6 rows high, which was comprised of an independent sample of letters. Hence, there was a random relation between the inner region letters of both members of the pair. This display contains neither transverse nor vertical disparities and is therefore called the uncorrelated display. Five uncorrelated-display pairs were constructed.

Fig. 2 shows a pair of horizontally correlated patterns superposed physically. It will be noted that the inner region contains pairs of superposed dissimilar letters. This is due to the fact that the shift of letters in one pattern causes similar letters to fall on noncorresponding places. These superposed letters of the inner region are unreadable. If the stimuli to corresponding points were to sum so that the letters would be seen as superposed in the stereoscope, they would also be unreadable. If, however, one of two stimuli to corresponding places were to be suppressed, then letters would be readable. Therefore, the mere fact of readability would be consistent with the hypothesis that suppression is present in these patterns.

The presence of suppression does not prove that binocular rivalry is the basis for binocular perception of form. In addition to the fact of superpositioning of dissimilar dots in the basic patterns, disparity is also present. Whether it be transverse or vertical, disparity rather than rivalry may be the basis for the binocular perception of form. The disparity may itself be the stimulus which somehow serves to delimit an area of the over-all display. From this point of view, the fact of superpositioned dissimilar objects is merely an accidental concomitant of the stimulus-situation. The uncorrelated displays offer an opportunity to test these alternatives. There is no disparity in these displays because the inner regions are comprised of dissimilar elements. Form-perception in the presence of rivalry in these patterns, is consistent with the rivalry-interpretation of the phenomenon. This cannot be done with dot-patterns, because independent samples of dots still present a combination of both vertical and transverse disparities.

To make a test of the rivalry-theory more decisive, it is desirable to obtain a measure of the amount of rivalry in the different patterns. Also, a comparison of the amount of rivalry in the different patterns will permit verification of the relatively greater apparent stability of the horizontally correlated displays.

The method of measuring the amount of rivalry requires *O* to read designated lines aloud while *E* notes changes in the sequences of letters. Suppose that *O* reads across a line comprised of two rows of superposed dissimilar letters in one of the uncorrelated stereo-displays. It is obvious that an ocular alternation has occurred if he reads a letter of one line and then a letter of the contralateral line. This is also true of vertically correlated displays, since dissimilar rows are superposed. The criterion for alternation must be more subtle in horizontally correlated displays because of the similarity of the letters contained in the monocular rows. Fig. 3 shows two such lines of letters.

If *O* should begin by reading 'a,' 'b' when these two rows are superposed in a stereoscope, and then read 'd,' it could be assumed that an alternation has occurred. This assumption would only be valid if convergence-variations are controlled. To control for these variations, *O* was given specific instructions to fixate. This can be described with the help of Fig. 4, which shows a portion of a binocularly-perceived letter-pattern. Since depth cannot be seen in this figure, a contour has been added to show the boundary of the inner rectangle.

*O* was required to read both Line 1 and 2 in the pattern. He was instructed to maintain fixation on Line 1, but to read the letters of Lines 1 and 2 in alternating order. Thus, he would read "a,e,b,f,c,g. . . ." Outside the region of the inner square, the binocularly perceived letters are comprised of superposed identical letters. Inside the inner rectangle, the superposed letters are dissimilar. It is here that alternation would be expected. If changes of sequence occur outside the inner rectangle, then a convergence-variation would be indicated. To prevent convergence variations from contaminating the results, a record was also kept of *O*'s responses to the letters outside the critical area. He was required to find the lines in the stimuli that corresponded to Lines 1 and 2 of Fig. 4. His ability to do this ensured that he was seeing the inner square.

| | |
|---|---|
| Left eye | abcdefghi··· |
| Right eye | bcdefghij··· |

FIG. 3. HORIZONTALLY CORRELATED LETTERS SHIFTED ONE LETTER SPACE

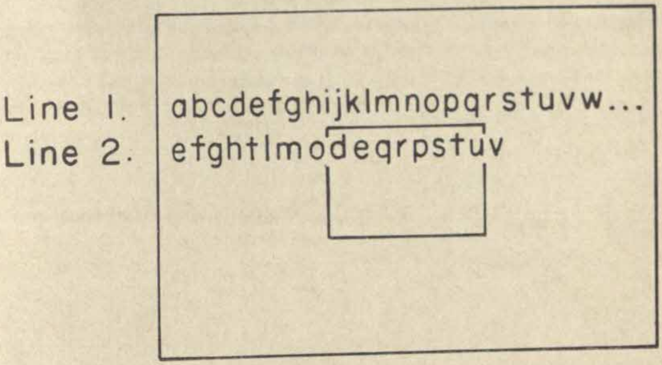

FIG. 4. SCHEMATIC PATTERN OF BINOCULARLY PERCEIVED LETTERS

There were three basic displays. Five representations of each type were prepared, therefore affording a total of 15 displays for use in the experiment. The procedure described above was employed both with the top line of the inner rectangle and with the bottom line of the inner rectangle, using the line below it as the fixation-line. This means that two lines of material were used in each display. Hence, 30 lines were used for each *O* in this experiment. The number of alternations in each line was tabulated. The average percentage of these alternations per line was calculated for each of the three displays.

Seven *O*s, one woman and six men, served in this experiment. All had normal or corrected-to-normal visual acuity. None had difficulty in achieving stereopsis.

*Results.* All *O*s were able to read the lines of letters of the stimuli. This is a further verification of the occurrence of suppression in these patterns, since dissimilar letters were superposed on corresponding points in all of the patterns.

The number of alternations observed in each of the 10 lines of each type of pattern was tabulated and the average percentage of letters in a line to alternate computed. These average percentages, classified according to display-type, are shown in Table I.

The average numbers of alternations in the vertically correlated and uncorrelated displays do not differ significantly. The average percentage of alternations in the horizontally correlated displays, however, differs significantly from both of the other display-types. The difference between the horizontally-correlated mean-percentage alternations and the mean of the vertically-correlated display-percentage data is significant at the 2% level, with $t = 3.56$. The difference between the means of the horizontally-correlated display-data and the uncorrelated display-data is significant at the 1% level, with $t = 3.74$. Therefore, the frequency of alternation is less in the horizontally-correlated display than in either of the other types of display.

TABLE I

AVERAGE PERCENTAGE OF ALTERNATIONS OCCURRING PER 16-LETTER LINE IN EACH DISPLAY-TYPE

| 0 | Type of display<br>Horizontally correlated | <br>Vertically correlated | <br>Uncorrelated |
|---|---|---|---|
| 1 | 1.3 | 14.0 | 21.3 |
| 2 | 16.6 | 35.3 | 24.7 |
| 3 | 6.6 | 20.7 | 20.0 |
| 4 | 9.0 | 38.0 | 37.3 |
| 5 | 2.6 | 3.3 | 4.7 |
| 6 | 13.3 | 14.7 | 10.7 |
| 7 | 0.7 | 22.0 | 21.0 |
| Mean | 7.2 | 21.1 | 18.9 |

The fact that form, the inner rectangle, was perceived in the uncorrelated displays indicates that disparity, either vertical or horizontal, is not necessary to the perception of form. The presence of rivalry seems to be a sufficient condition to produce binocular form-perception.

*The displacement phenomenon.* An accidental observation was made in the course of this experiment. Two of the *O*s reported spontaneously that the letters corresponding to Line 1 of Fig. 4 in their patterns did not appear to line up vertically with the letters corresponding to Line 2. The letters in the two lines were displaced by as much as one-half a letter-space. This is an odd phenomenon, since the geometry of both patterns is such as to demand a vertical alignment of letters. Since this effect occurred uniformly across the sixteen letters of the inner rectangle in the horizontally correlated displays, it suggests that a fusional process may have been tapped. In the case of horizontally-correlated letters in the two displays, images of similar letter-stimuli are seen at non-corresponding places. If these letters fall within Panum's fusional areas, it may be assumed that the contour-processes associated with the letter-images fuse or summate to yield the perception of a single image in depth. The place of fusion could be intermediate between the positions of the half images in phenomenal space, thereby producing the displacement-phenomenon. This theory would account for the differences in the amount of rivalry encountered in the displays, since fusion would supplant rivalry in the horizontally-correlated displays.

Apart from the fact that the postulation of a fusional process is not so parsimonious as an explanation that would rely solely upon a suppression-model, it does present some substantive problems. For one thing, only two of the *O*s yielded a marked effect. If fusion occurs at an intermediate place in phenomenal space, this should be a more general effect. Secondly, although they were relatively few, instances of rivalry did occur in the horizontally-correlated displays.

The results of Experiment III suggest an explanation of the displacement-phenomenon which is based upon suppression. One assumption required by this explanation is that whenever corresponding points are stimulated simultaneously, only one of the two inputs will come to awareness. Secondly, the results obtained with the vertically correlated displays suggest the proposition that within the limits of ocular dominance, the suppression of one element at one horizontal meridian has nothing to do with the suppression of another element at another horizontal meridian. To be more explicit, suppose that rows of letters are imaged at corresponding horizontal meridians in the two eyes. Also suppose that most of the horizontally distributed inputs to one of the eyes at that meridian are suppressed. Now, if another pair of stimuli is imaged at another pair of corresponding horizontal retinal meridians, the suppressions of one of the former stimulus-representations will not influence that member of the second pair of representations which would be suppressed. Hence, correlated letters arranged in a vertical column will suppress independently of each other. This is clearly not true, however, of the stimuli to the two eyes along corresponding retinal meridians. As the experiment has shown, when these stimuli are redundant, markedly less alternation of individual letters is observed. It may therefore be inferred that when the stimuli to the two eyes are similar and arranged horizontally, they tend to behave as a unit. Thus, an entire row of letters imaged in one eye may be suppressed, provided that they are similar to the letters in the contralateral eye. If these assumptions are correct, then binocular variations in convergence or disparity of fixation can explain the displacement-phenomenon. To make this clear, suppose that one of the rows of letters which serves as an input to form the binocularly perceived row immediately above the inner rectangle, is suppressed by the left eye. Suppose also that one of the rows which make up the binocular top row of the inner rectangle is suppressed by the right eye. This means that the row experienced by *O* above the inner rectangle corresponds to the input to the right eye and the row experienced by him at the upper edge of the rectangle corresponds to the input to the left eye. Now, if the eyes shift convergence, the two rows will shift with respect to each other. Also, in the presence of a fixation-disparity, this independence of row-suppression can produce a constant offset.

The foregoing explanation was reinforced by a number of informal observations and a small experiment designed specifically to test it. The observations were made by the writer, who found that he could induce the effect reported by the two *O*s in the experiment by simply moving one of the stereograms slightly to the left and right. Before convergence could catch up to the displacements of the stimuli, the phenomenon could be observed. Other experienced *O*s could also make this discrimination. These observations led to Experiment IV.

## EXPERIMENT IV

The purpose of Experiment IV was to test the hypothesis that the displacement-phenomenon was due to variations in convergence of the eyes.

*Method and procedure.* It was first determined empirically that letters typed with a black ribbon will dominate similar letters typed with a red ribbon when the letters are superposed in a stereoscope. Based upon this fact, two stimulus-cards were prepared. One card contained the letters ABCDE typed in black. The other card contained the same letters (ABCDE) typed in red. One space below the red letters was another row of red letters: namely, FGHIJ. Both cards were mounted in the Telebinocular on a holder which permitted *O* to turn a knob which caused the black letters to slide to the left or right. Initially, *O* was asked to report the relative vertical positions of the letters in the stereoscope. After reporting their positions, he so turned the knob that the black letters would slide slightly to the left and right relative to the red letters. He was to report if he saw the red ABCDE letters at any time during the excursion and also if the letters of the top and bottom rows displaced relative to each other.

Five *Os* were used in this experiment. Two of them were the *Os* who had reported the displacement-phenomenon in Experiment III. The other three were *E* and two sophisticated *Os*.

*Results.* The two *Os* who first reported the displacement-phenomenon also observed it in these stimuli. When they turned the knob to displace the black letters, they reported changes in the magnitude of the displacement. Also, they did not report seeing the red letters in the upper line, even when they were displaced from superposition with the black letters. The three remaining *Os* did not at first see the displacement-phenomenon. They reported seeing it readily, however, when they turned the knob. Moreover, the magnitude of the displacement seemed to vary with the magnitude of knob-rotation. The red letters in the upper line also remained suppressed, even after displacements by as much as one letter-space.

The results of this experiment are consistent with the explanation of the displacement-phenomenon by suppression. It is clear that suppression occurred in viewing the superposed black and red letters because the binocularly perceived letters were black and not a dark red. The letters perceived corresponded, therefore, to the black input and not to some fused image. As these black letters moved laterally, they displaced relative to the lower line of red letters. This relative displacement was consistent with the geometry of a situation in which one set of letters was presented to one eye and another set to the other eye at another horizontal meridian. Also, in an earlier paper, it was shown that suppression spreads from the place of overlap of rivaling stimuli in the binocular field. It was also shown that, where two black bars are so superposed as to form a cross in the binocular field, moving one of these bars, so that it no longer overlaps the other, will leave a wake of suppression in its path.[6] This indicates that there is either a time-lag involved in recovery from suppression or that even though two contours do not directly overlap in the binocular field, they can still be rivaling. This is consistent with the fact that the red letters of the upper line remain suppressed, even when they are no longer superposed on the contralateral black letters.

In addition to the foregoing, the two *Os* who spontaneously reported the displacement-phenomenon did so in this experiment, too. If the initial displacement is due to fusion, then it must be postulated that these

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[6] Lloyd Kaufman, On the spread of suppression and binocular rivalry, *Vision Research,* 3, 1963, 401.

*O*s experienced a fixation-disparity in this experiment. It is only errors of fixation that produce disparity between the letters of the top line and the bottom line. The bottom line will always be single because the stimulus is monocular. The top line will display diplopia only if the letters of the left eye do not fall on places that correspond to the places where the letters of the right eye are imaged. If the diplopia exists because of the disparity of fixation, then it would be predicted that the displacement-phenomenon will vary with changes in convergence. This, however, can only happen in the patterns of this experiment because there is no contralateral stimulus for the bottom line. Moreover, it cannot occur in the patterns of Experiment III because the disparity will not change with changes in convergence. In the latter patterns, there are binocular stimuli for all lines of letters. As convergence is altered, the relative positions of all elements in one eye remains invariant. Therefore, the relative disparity remains constant. Fusion would therefore lead to a constant displacement independent of disparity of fixation. Since the displacement-phenomenon changes magnitude with lateral displacement of one of the original patterns, it cannot be due to fusion.

## Summary and Conclusions

This research started with the hypothesis that form-perception in Julesz's patterns is due to a differential effect of binocular rivalry in the fused image. As a first step, it was shown that suppression is, in fact, present in superposed dot-patterns. It was also shown that the stereoscopic effect can be obtained in the presence of marked color-induced rivalry. This is particularly important for theories which ascribe a role to suppression in stereoscopic vision. The result refutes the criticism that if an input is suppressed it cannot function to produce stereopsis which, after all, depends upon binocular disparity. The fact that stereopsis occurs in the presence of clear suppression indicates that though the disparity may not be directly available to awareness, it may still function at a neural level to produce stereopsis.

An experiment was also performed with letter-analogues to the dot-patterns. These permitted an assessment of the amount of rivalry in various types of patterns. In patterns which display horizontal disparity, no more than about 7% of the letters in a 16-letter line of the matrix will display rivalry. This was measured in the presence of a depth-effect. About 20% of the letters in lines which are horizontally-uncorrelated will, however, display binocular rivalry. When the inner regions of the letter matrices are totally unrelated, rivalry and form-perception both occur. This means that the form-perception does not depend upon disparity, since there is no disparity in the display. Rivalry between individual elements in the displays appears to be a sufficient condition for the form-perception.

A misalignment of letters in different rows of the binocularly perceived patterns was reported spontaneously by two *O*s. This misalignment, termed the displacement-phenomenon, seemed interesting because it might represent an instance of summative fusion of representations of disparate stimuli. It was found, though, that the displacement-phenomenon could be explained in terms of independent suppression of the monocular rows of letters. An experiment which yielded results consistent with this explanation was described.

It is concluded that the existence of depth-perception in displays with horizontal disparity explains the sharp form-perception in the presence of consistent monocular suppression across the horizontal meridians of the two eyes. The dots, or letters, in these patterns are at a different plane of depth than the background and, therefore, take on a segregated organization. In the absence of a depth-effect, these and similar patterns contain forms because of a differential effect of binocular rivalry.

The results of this study suggest some interesting properties of binocular vision. First, it seems clear that suppression plays an important role in stereopsis. It is at least related to the problem of the singleness of binocular vision; but the way in which suppressive phenomena operate is not so simple as is suggested in the writings of Verhoeff and of Asher.[7] While it is true that one or the other representation of stimuli to corresponding places will be suppressed, the stability of the suppression depends upon other stimuli on the same horizontal meridians. Two letters alone will rival. These same letters will not rival if they are contained within horizontally-correlated but displaced rows of letters. This is not simply a matter of informational redundancy, since the same degree of redundancy in the vertical dimensions leads to different results. It would seem, therefore, that there is lateral communication between sets of corresponding places at higher nervous centers which allows them to coördinate their functions when the stimulus-configuration has certain properties. Coördinated suppression may be the basis for apparent binocular fusion which seems to occur because ordinary stereoscopic scenes are so stable. It must be emphasized once again that though suppression occurs, the suppressed elements must still function in the nervous system to produce the perception of depth. Parametric studies are needed to clarify the precise nature of the factors which influence coördinated suppression.

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[7] F. H. Verhoeff, A new theory of binocular vision, *Arch. Opthal.*, 13, 1935, 151; Harry Asher, Suppression theory of binocular vision, *Brit. J. Opthal.*, 37, 1953, 37.

# VERBAL CUES, LANGUAGE, AND MEANING IN SELECTIVE ATTENTION

By ANNE M. TREISMAN, Oxford University, England

In 1953, Cherry did a series of experiments requiring a selective response to one of two verbal messages presented either binaurally or dichotically.[1] While the irrelevant message caused negligible interference with the repeating back or 'shadowing' in the dichotic condition, it made selection extremely difficult when both messages were presented binaurally, at equal intensities and in the same voice. The *S*s needed many attempts at the same message and pieced it together only gradually, making errors and transpositions which were influenced by the probability-structure of English. They did eventually, however, succeed in separating out one of the two messages when these were normal prose, but failed when presented with strings of *clichés* with transitional probabilities which were high within phrases but fell to zero between them. This performance seems qualitatively different from that in the dichotic condition, and may not involve selective attention at all. The *S*s might have heard both messages and retrospectively put together what made sense (in fact they did much better if allowed to write), whereas in the dichotic condition they succeeded on the first trial and could report none of the verbal content of the irrelevant message, nor even that in one case it changed to a foreign language.

Broadbent put forward a general theory of selective attention which accounts for the finding with dichotic presentation by postulating a filter which selects signals arriving on one of two or more input channels and rejects others before they are fully analyzed.[2] The aim of the present experiment was to investigate selective attention when it depends solely on the identification of verbal or linguistic features, using a task where the messages are presented only once and require an immediate and continuous response. In more detail, it was hoped:

(1) To find out how efficient selective attention could be with competing mes-

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[1] E. C. Cherry, Some experiments on the recognition of speech with one and with two ears, *J. acoust. Soc. Amer.*, 25, 1953, 975-979.

[2] D. E. Broadbent, *Perception and Communication,* 1958, 36-45.

sages in different languages. Broadbent suggested that "classes of words may behave in the same way as sensory channels do," that is, they might be passed on or discarded by a selective filter in the same way as messages differing in localization or voice.[3] A difference in language is probably the most extreme case of a verbal cue's distinguishing two messages, and it might be possible to 'filter out' one language as such, in the absence of any general differences in physical characteristics.

(2) To compare the interference produced by irrelevant messages differing only in verbal characteristics, to determine the roles of familiarity, redundancy, meaningfulness, and similarity, either of phonetic structure or of meaning. In an experiment in which the messages arrived on different channels distinguished by localization, verbal differences between the irrelevant messages (in similarity, informational content, and meaningfulness) had no effect on the efficiency of selective attention.[4] This suggests that when an effective cue for filtering out irrelevant messages at an early stage is available, little of their verbal content is normally analyzed. If differences between classes of words—languages—could be used for the same type of filtering, a similar result would be expected here, but if messages arriving on the same channel are rejected at a later stage, familiarity, similarity, and redundancy should have some effect on performance.

(3) To discover how far messages could be selected on the basis of transitional probabilities between words alone. The aim was to clarify the level of selection in Cherry's experiment with binaural presentation by showing whether irrelevant messages were discarded before full analysis or only in piecing together the over responses.

In the present investigation Cherry's binaural situation and his shadowing technique were used, except that the *Ss* were given only one trial with each pair of messages to restrict their performance as far as possible to one of selective attention. Unfortunately, binaural presentation introduces the complication of peripheral auditory masking which is less important in the dichotic condition. Miller investigated the intelligibility of speech with different types of interference—noise, tones, and speech—and concluded that the crucial factors in masking were the frequency-spectrum and the continuity.[5] Thus, masking by speech was more effective with several voices than with one alone, presumably because there were fewer gaps both in time and in frequency-spectrum. A single voice at the same intensity as the test-words allowed about 78% intelligibility of single words, as compared to 95% without it. This would give almost 100% intelligibility for coherent prose, so that masking alone is unlikely to be the main source of any impaired performance in the present experiments. Moreover, in the present investigation masking effects were equalized by using one voice (with a constant long-term frequency-spectrum) for all but two conditions, so that differences in the results could be attributed to differences in interference at the level of word or meaning.

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[3] *Ibid.*, 54.

[4] A. M. Treisman, Attention and speech. Unpublished Doctoral dissertation, Oxford University, 1961, 54.

[5] G. A. Miller, The masking of speech, *Psychol. Bull.*, 44, 1947, 105-129.

*Method: (1) Apparatus.* The passages of prose were recorded in a woman's voice on one track of a two-channel Brenell Mark 5 tape-recorder. They were monitored on a microammeter and repeated until the mean intensity remained approximately constant with no long or marked fluctuations. They were read as regularly and monotonously as possible, with pauses kept to a minimum, at an over-all speed of 150 words per min. The irrelevant messages were recorded on the other track of the same tape-recorder, starting a few words later than the first, and with the same precautions to control for variations in intensity, pitch, and timing. For the experiment, the two outputs were joined and fed together at the same intensity (about 60 db. SL) to both earphones of a pair of Brown moving-coil headphones. The *Ss'* responses were recorded on a Grundig Stenorette recorder.

*(2) Speech material:* (*a*) *Part I.* The messages to be shadowed were narrative or descriptive extracts from the novel, *Lord Jim,* by Conrad. They were 150 words long, lasted 1 min., and were in a woman's voice.

The irrelevant messages were: (1) English prose from the *same novel,* in a *man's voice;* (2) English prose with a long interpolated passage of *Latin,* also in the *man's voice;* (3) English prose from the *same novel,* in the *same woman's voice* as the shadowed message; (4) English prose from a *technical* discussion of biochemistry, in the *same voice;* (5) *French* prose from a novel, in the *same voice;* (6) *German* prose from a novel, in the *same voice;* (7) *Italian* prose from a novel, in the *same voice;* (8) Czech, simplified and spoken with a deliberately English accent (equivalent to *nonsense using the same phonemes* as English), in the *same voice;* (9) English played backwards, in the *same voice,* and (10) A *French translation* of the English shadowed message, in the *same voice.*

*(b) Part II.* Here the messages to be shadowed were a set of statistical approximations to English,[6] constructed by the method described by Miller and Selfridge.[7] Each was 100 words long and lasted 40 sec.; 1st, 2nd, 4th, 6th, 8th and 12th order passages were used. The competing irrelevant messages were extracts from Conrad's novel, *Lord Jim,* read in the same voice.

*Subjects.* The *Ss* were undergraduates or research students at Oxford University who volunteered for the experiment. Many of them were studying modern languages at the University. Each *S* was asked to give an estimate of how well, if at all, he knew each of the foreign languages. The scale used was A, B, and C; A, indicated bilingual or fluent—either studying it full-time at the University or having lived for some time in the country; B, some knowledge of the language—learned at school or on a short visit; and C, no knowledge.

*Procedure.* The *Ss* were first given some three or four practice trials at shadowing with dichotic presentation, until they could do this quite fluently, and then a further three trials with binaural presentation of selected and irrelevant messages. They were then presented with the experimental messages, both to both ears, and were asked to repeat back continuously, as they listened to it, as much as they could of the message which started first. They were told to try as far as possible to keep to this message and not transfer to the other, but they were encouraged not to refrain from speaking altogether when in doubt, but to repeat as much as they could of the one they believed to be correct. Their responses were later analyzed and the percentages of words correct, intrusions from the wrong message, errors and omissions were noted.

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[6] A. M. Taylor and Neville Moray, Statistical approximations to English and French, *Lang. and Speech,* 3, 1960, 7-10.

[7] G. A. Miller and J. A. Selfridge, Verbal context and the recall of meaningful material, this JOURNAL, 63, 1950, 167-185.

Sixteen *Ss* did both Part I and Part II of the experiment in one session, starting with Part I; 14 did Part I only, and 12 did Part II only. (These last two groups were unable to spare more time, so the *Ss* who knew one or more foreign languages did Part I and others did Part II only.) Three different recordings of each set of messages were made to cancel out the effects of practice, fatigue, and relative difficulty of shadowed and irrelevant messages. In each recording, the messages were presented in a different order, and were differently paired in such a way that the irrelevant messages which had been associated with low scores in the previous recording were paired with test messages which had given high scores. The *Ss* were assigned their recording in the order in which they volunteered—effectively at random.

TABLE I

PERCENTAGES OF WORDS REPEATED FROM THE CORRECT MESSAGE AND OF INTRUSIONS FROM THE IRRELEVANT MESSAGE

Condition

| Irrelevant message | 1 Man's voice, same novel | 2 Man's voice, with Latin | 3 Same voice, same novel | 4 Same voice, technical English | 5 to 7 Same voice foreign languages | | | 8 Same voice, Czech nonsense | 9 Same voice, reversed English |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | A | B | C | | |
| Mean % correct | 73.7 | 73.9 | 31.0 | 39.8 | 42.2 | 47.9 | 55.5 | 49.6 | 45.9 |
| *SE* | 2.5 | 3.1 | 2.4 | 3.3 | 2.8 | 1.7 | 2.1 | 2.0 | 2.3 |
| Mean intrusions | <1.0 | <1.0 | 19.5 | 12.3 | <1.0 | <1.0 | <1.0 | <1.0 | <1.0 |
| *SE* | | | 2.1 | 2.1 | | | | | |
| No. *Ss* | 20 | 20 | 30 | 29 | 20 | 27 | 23 | 30 | 27 |

In Part I of the experiment, one group of *Ss* (chosen at random for each recording), was told in general terms what the irrelevant message would be (*e.g.* "a passage in French") and another group was not; none was told that in one case the French was a translation of the shadowed English. In those cases where they were not told in advance, they were asked after each passage whether they had incidentally noticed what the irrelevant message had been. In Part II, none of the *Ss* was told the nature of the messages to be given, nor interrupted between individual statistical approximations. At the end of the series, all the *Ss* were asked whether they had noticed what the rejected message was about and how confident they were that they had kept to the correct one.

*Results.* The results of Part I are summarized in Table I, which gives the mean percentages of words correctly repeated from the selected message and the mean percentages of intrusions from the irrelevant message.

For the foreign languages, the results are classified according to how familiar the language was to each *S*; thus, for example, the scores grouped under A are those obtained by all *S*s with the irrelevant message in the language they knew fluently, whether this was French, German, or Italian. If two languages were equally familiar to *S*, his score was taken as the mean of the two results. The results of the first 10 *S*s with the two irrelevant messages in the man's voice had to be discarded as the recording proved unsatisfactory (pauses were too long and the voice not sufficiently monotonous). Three results with the reversed English and one with the technical English were also omitted, as the *S*s were distracted and stopped shadowing to comment on the interference. Student's *t* was calculated for pairs of scores from the same *S*s, to compare the effects of different types of interference. Table II shows the number of *S*s who noticed the nature of the irrelevant messages when they had not been told in advance. The main differences that emerged will first be summarized and then their theoretical implications will be discussed in more detail.

TABLE II

NUMBER OF *S*S WHO NOTICED THE NATURE OF THE IRRELEVANT MESSAGE

| | Condition | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Irrelevant message | 2 Man's voice, with Latin | 5 to 7 Same voice, foreign languages | | | 8 Same voice, Czech nonsense | 9 Same voice, reversed speech | 10 Same voice, French translation | |
| | | A | B | C | | | A | B |
| Number of *S*s who noticed nature of irrelevant message | 6 | 9 | 18 | 11 | 4 | 2 | 11 | 2 |
| Number who noticed only that it was not English | 3 | 1 | 0 | 5 | 5 | 8 | 8 | 9 |
| Total *S*s | 20 | 10 | 18 | 16 | 22 | 17 | 19 | 11 |

(1) *Comparison of voice- with language-difference.* There is a marked contrast between the interference by the man's voice and the interference produced by the same woman's voice as read the selected message. Condition 1 was significantly easier than both Conditions 3 and 4 ($t = 13.0$ and 7.9, $df = 19$, $p < 0.001$). Moreover, far fewer *S*s noticed that the competing message in the man's voice in Condition 2 was not in English; only 6 out of 20 identified it as Latin, while with the two messages in the same voice, 38 out of 44 identified the foreign languages. With the Czech or nonsense message (which, like Latin, was phonetically similar to Eng-

lish), however, only 9 out of 22 noticed that the interference was not in English, which suggests that the *S*s were identifying the foreign languages chiefly by general features of pronunciation rather than by analyzing fully all their verbal content. On the other hand, there was no difference in the interference produced by Latin and by English when the man's voice was used, while with the same voice, differences in the language of the irrelevant message were important.

(2) *The effects of familiarity, phonetic differences, and meaning.* The familiarity or meaningfulness to the *S* of the foreign language did affect the degree of interference it produced; Condition C was significantly better shadowed than Condition B, which, in turn, was better than Condition A ($t = 3.7$ and 3.5, $df = 16$ and 19, $p < 0.01$ in each case). The Czech was also easier than the technical English ($t = 3.5$, $df = 28$, $p < 0.01$), but the reversed speech was not significantly different from the technical English.

To some extent, the method used controls for possible effects of phonetic differences between the foreign languages, since different *S*s were familiar with different languages. The *S*s classed A, B, and C were unfortunately, however, not equal for each language; for example most *S*s knew French better than Italian. The decrement in performance, when a well-known language interferes, cannot, however, be accounted for by increased interference from the French, as such, for within each class of familiarity, A, B, and C, there were no significant differences between the languages at all. The mean difference between pairs of languages equated for familiarity was only 1.5%. Thus, phonetic differences between the irrelevant messages had little effect, although their phonetic dissimilarity to the shadowed English may have been important.

The effect of this phonetic cue's distinguishing the irrelevant message from the selected one can be seen in a comparison of Conditions C and 8, the unknown foreign language and the Czech nonsense which was phonetically similar to the relevant English: the phonetic difference made the foreign language a little easier to reject ($t = 2.2$, $df = 22$, $p < 0.05$). The reversed speech also caused considerably more interference than the unknown foreign language ($t = 4.2$, $df = 20$, $p < 0.001$). It may be that most *S*s had never heard anything similar and that its unfamiliarity was distracting. From their comments, however, it seems to have been more than this: Many *S*s said that it seemed to "swallow up the English," "to cancel it out," "to come forward," "to fuse with it."

The French translation of the selected English message was neither easier nor harder to reject than the irrelevant French message (a mean difference of 1.8%). Not all the *Ss* who were fluent in the language noticed it was a translation (11 out of 19). There were, however, a few overt intrusions which suggested that contextual probabilities were facilitating identification of occasional French words: "fits of *vertige*" and "floating *dans l'eau*" are examples. Moreover, comments from those *Ss* who noticed it was a translation varied considerably: Some said it was very disturbing and seemed to "claim their attention," while others said they found it a help and actually translated back a few words of the French when they had heard that and missed the English. (One showed this by saying "nearly" instead of "all but," and another said "a piece of wood" ("*un morceau de bois*") instead of the English "a log of wood.")

(3) *The effects of subject matter and of transitional probabilities between words.* When both messages were in the same voice and equally intelligible to *S*, there was no significant difference between the number of words correct for messages distinguished by language and for messages distinguished only by subject matter (Conditions A and 4), although there was a significant difference in the number of intrusions from the irrelevant message. When, on the other hand, the two messages were from the same novel, Condition 3, there was a decrease in the number of words correct as well as an increase in the number of intrusions from the irrelevant message compared to the technical English, Condition 4, both significant ($t = 2.4$ and $2.2$, $df = 28$, $p < 0.05$). Thus, the important factor with messages in the same voice seems to be not a language-difference, as such, but the presence of a cue to distinguish the two messages; two passages taken from the same novel would have much more in common than a passage from a novel and a passage of technical biochemistry.

The effect of verbal context is shown more clearly in the results of Part II of the experiment, given in Table III and Fig. 1. There is a trend downward with order of approximation in the number of words correctly repeated and an opposite increase in the number of intrusions from the incorrect message, the extracts from the novel. The regressions of words correctly repeated and of intrusions from the irrelevant message are plotted against the information-rate, based on estimates of the mean bits per word in each passage, made from the guesses of 100 *Ss* at 20 missing words.[8] Both regressions were significant and the departures from linearity insignificant. The total number of words repeated stays almost constant, the increase in intrusions compensating almost exactly for the decrease in words correct. Very few *Ss* realized that the messages they were supposed to be shadowing were not coherent prose, but more than half thought they had probably switched to the wrong message for part of the time.

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[8] Treisman, *op. cit.,* 153.

The slope of the regression of words correct is similar to that found in

TABLE III

PERCENTAGES OF WORDS REPEATED FROM THE CORRECT MESSAGE AND OF INTRUSIONS FROM THE IRRELEVANT MESSAGE

| | Order of approximation | | | | | |
|---|---|---|---|---|---|---|
| | 12th | 8th | 6th | 4th | 2nd | 1st |
| Mean % correct | 28.5 | 26.0 | 26.3 | 19.7 | 19.6 | 10.6 |
| *SE* | 2.3 | 2.3 | 2.5 | 2.0 | 2.5 | 1.2 |
| Mean % intrusions | 24.8 | 26.8 | 26.4 | 29.6 | 27.6 | 40.6 |
| *SE* | 2.2 | 3.0 | 2.9 | 2.4 | 2.5 | 2.6 |
| Totals | 53.3 | 52.8 | 52.7 | 49.3 | 47.2 | 51.2 |

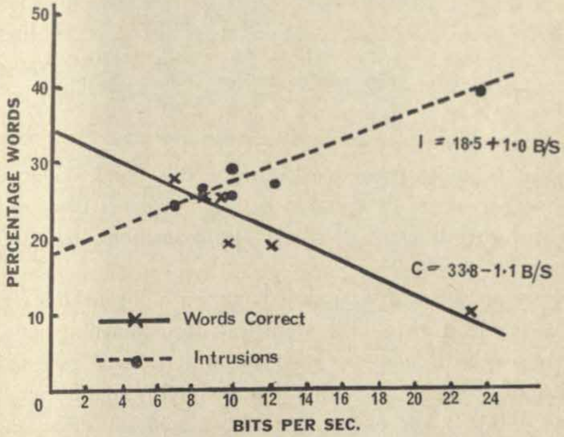

FIG. 1. REGRESSIONS OF WORDS CORRECTLY REPEATED AND OF INTRUSIONS FROM IRRELEVANT MESSAGE ON INFORMATION-RATE OF SELECTED MESSAGE

an earlier experiment for shadowing the same messages with only dichotic interference (33.8 − 1.1 bits per sec. compared with 101.9 − 1.3 bits per sec.), although the intercept is very much smaller.[9] This suggests that the same system is operative, but under more difficult conditions. The fact, however, that the regression for the intrusions, which were all from the novel, also has a similar slope (18.5 + 1.0 bits per sec., where bits-per-sec. refers to the competing statistical approximation) shows that the probability of words' being repeated depends not only on their own information-content, but also on the competing message. Thus the extracts from the Conrad novel, which probably all averaged about 6.5 bits per sec., (the value obtained for one extract) became progressively more likely to be repeated as the competing words became less contextually constrained.

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[9] *Ibid.,* 182.

This result could have been reached in two ways: The *S*s might have switched more often to the prose when repeating the lower orders of approximation, without repeating more consecutive words from the prose on each occasion; or they might have switched equally often for all orders of approximation, but repeated more consecutive words from the prose when it was competing with the less coherent passages. The responses were analyzed in more detail to determine this point. Table IV shows the mean number of words repeated between switches from one message to another for the different statistical approximations, for the technical English, and for the novel.

TABLE IV

MEAN NUMBER OF WORDS REPEATED FROM EACH MESSAGE BETWEEN SWITCHES FROM ONE MESSAGE TO THE OTHER

| | Selected passage | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 1st order | 2nd order | 4th order | 6th order | 8th order | 12th order | Novel | Tech. Eng. |
| Correct words | 2.6 | 4.7 | 5.2 | 6.2 | 7.0 | 9.8 | 11.2 | 11.8 |
| Intrusions | 13.2 | 7.5 | 8.1 | 8.0 | 6.5 | 8.3 | 6.8 | 3.5 |

An analysis of variance was carried out on a logarithmic transformation of the results for (a) the statistical approximations and (b) the prose passages which competed with them. It showed that for (a) the variance due to order of approximation was highly significant ($F = 19.2$, $df = 5$, $p < 0.001$). For (b) it was also significant ($F = 3.2$, $df = 5$, $p < 0.01$), but here the effect was almost entirely attributable to the first-order passage and disappeared completely ($F < 1$) when the first order was omitted from the calculation. For (a), the variance when the first order was omitted remained highly significant ($p < 0.001$). This suggests that with the two exceptions of the first-order passage and the technical English, the number of words repeated from the 'irrelevant' prose remained approximately constant. Thus, the switches between relevant and irrelevant message were determined primarily by transitional probabilities within the message *S* was repeating at the time, and not by the probabilities of competing words.

*Discussion.* It may be helpful at this point to summarize the main findings: (1) It was shown that a difference in voice (male vs. female) and a difference in language have quite different effects on tasks requiring selective response to one of two messages. The difference in voice allows the irrelevant message to be rejected much more efficiently, and this probably takes place at an earlier stage in the perceptual analysis of inputs. (2) When two messages share the same general physical characteristics, a difference in language allows some selection between them; however this seems more similar to selection between two English messages on the basis of subject matter than to the efficient performance obtained with different voices. Thus, complete rejection of one language as such appears to be impossible. (3) Phonetic cues make an unknown foreign language less distracting than a message which is phonetically similar to English (the Czech nonsense), and allow the *Ss* to name the irrelevant foreign language. Reversed speech however, causes a relatively high degree of interference. (4) The *Ss'* knowledge of the language affects the amount of interference it produces. In most cases, however, little of the content of the rejected message can be reported. Many *Ss* failed to notice that the Czech was not normal English and not all the *Ss*, even among those who knew the language fluently, realized that a rejected French message was a translation of the selected English one. While similarity of the languages at the phonetic level makes selection more difficult (as with the Czech), similarity of meaning (with the French translation) produced no general increase in interference. (5) Finally, when both messages are in the same language and same voice, selection is based chiefly on transitional probabilities between words and its efficiency varies with the degree of contextual constraint within both the selected and irrelevant messages. This is the only condition in which a considerable number of overt intrusions from the irrelevant message is made.

These findings are relevant to the following problems: (1) Are both messages fully analyzed before one is selected to determine the responses? (2) If not, at what stage in the analysis is one of the two discarded? (3) What determines selection and switching between messages? How does this depend on the familiarity and phonetic structure of the languages and on the transitional probabilities between words?

*(1) Analysis of irrelevant message.* For the following reasons, it seems unlikely that both messages are always fully analyzed and that selection takes place only for the overt responses: (a) less than half the *Ss* recognized either the French translation or the Czech nonsense, although this would have added no more load to response or memory than noticing the male voice. Those who did identify these messages may have done so by switching to the irrelevant message. (b) When the statistical

approximations were competing with normal English prose, the points at which the *S*s switched between the messages were determined primarily by the contextual constraints between words that they repeated and not by the probabilities of competing words.

*(2) Stage at which selection is made.* The question then arises: at what stage is one of the two messages rejected from further analysis? It does not seem possible to reject or filter out irrelevant messages which differ only in verbal characteristics in the same efficient way as those which differ in general physical features. The marked contrast is unlikely to be due to differences in peripheral auditory masking (as might have been the case in Cherry's dichotic vs. binaural conditions), for low frequencies are usually more effective in masking, but here the man's voice caused much less interference than the same woman's voice. The efficiency of selection between messages differing in physical features is probably due to the fact that the *S*s can reject them at an early stage in perceptual analysis, so economizing the limited capacity available for the more complex discriminations necessary in identifying words. If this early selection were possible between messages in the same voice but different languages, known and unknown languages should cause equal interference and both should be easier to reject than a message in the same language as the selected message. Neither of these predictions was confirmed by the results.

The results thus suggest that features of incoming messages are analyzed successively by the nervous system, starting with general physical features and proceeding to the identification of words and meaning, and that selection between messages in the same voice, intensity, and localization takes place during, rather than before or after, the analysis which results in the identification of their verbal content. It seems to be at this stage that the information-handling capacity becomes limited and can handle only one input at a time, either keeping to one message where possible, or switching between the two. Broadbent's suggestion that one may think of classes of words as constituting separate "input channels" which can be rejected, as such, is not supported by these results.

*(3) Factors determining selection and switching between messages.* What, then, determines selection and switching when both messages arrive on one input channel? The irrelevant messages seem to fall into two main classes: *(a)* those which the *S*s potentially could identify; and *(b)* those whose verbal content they could never identify at all, because the language was unknown or the tape played backwards.

(*a*) When the irrelevant message was in a known foreign language or in English, the interference often took the form of making the *S*s shift their attention to the wrong message and lose the correct message altogether for a time. The results with the statistical approximations suggest that the *S*s repeat the correct message until its transitional probabilities fall to a low value. Differences in the competing messages do not affect the point at which they switch, but do affect their subsequent performance. Having switched, the *S*s have two decisions to make: whether to repeat aloud what they hear, or to switch back. The instructions were to repeat as much as possible, rather than remaining silent when in doubt. This would encourage the *S*s to make overt responses until they switched back, except when these were obvious errors, such as words in a different language.

To examine in more detail the results with the different messages actually used: *(i) Known foreign language and technical English.* It seems that the *S*s switch back to the correct message after the same mean interval with the known foreign language and with the technical English (since the number of words correct was the same for both conditions). This is likely to be the minimal time possible, since words in a foreign language, once identified, are immediately recognizable as errors. They make, however, more overt errors of intrusion before switching back from the technical English than from the foreign language. *(ii) Same novel.* When both messages are from the same novel and thus have a similar verbal content, the *Ss* take longer to decide to switch back, and so identify less of the correct message as well as making more overt intrusions. *(iii) When the irrelevant message is a translation of the selected message.* Under these conditions, bilingual *Ss* may switch between the two more often, as suggested by some of their comments quoted in the results. Transitional probabilities between words of the same meaning in the two languages will be high for such *Ss*, and may even cause a few overt intrusions. *(iv) Statistical approximations.* Since the statistical approximations have no distinctive or continuous subject matter, there is no means of recognizing intrusions from the competing prose as errors. Switch-backs from the prose are therefore caused only by its transitional probabilities falling to a low value. The first-order approximation is probably an exception. This passage is easily recognizable as different from the prose, through its absence of any sequential dependencies between words, and perhaps also by differences in intonation, which are difficult to avoid completely in reading the random words. This might explain why the *Ss* apparently switched back to it less often than to the other approximations; they may in fact have switched as often, but recognized the change and avoided any overt responses, just as they did with the known foreign language.

(*b*) With messages in an unknown language, complete shifts of attention did not seem to occur as they did with languages the *Ss* had learned to understand, *e.g.* "Sometimes, it's just like atmopherics, very easy to ignore; at other times, the two make sense and the mind wanders." The interference with these unidentifiable messages was more like a continual masking at the phonetic level. It was particularly marked with the reversed speech, as many *Ss* spontaneously remarked.

*Need for an hypothesis.* The account given so far has been oversimplified, since it assumes that the individual words or phrases of each message arrive as separate units to be identified as such. One needs some hypothesis about the way in which verbal messages are analyzed, to explain how one message is selected from the complex sound-wave arriving at the ears. We might propose that a series of tests is carried out on incoming messages, each examining for a criterial attribute such as the presence or absence of formants, the syllabic pattern, etc. As a result of each test the signal is passed down one of two or more alternative paths until the series of tests has been completed. The tests can be represented as a branching tree or hierarchy whose terminals are particular words or other verbal units. Each test might be thought of as a signal detection problem (see Tanner and Swets);[10] a certain adjustable criterion point is adopted on the dimension being discriminated above which signals are accepted and below which they are rejected as 'noise.' Transitional probabilities between words would then have the effect of lowering criteria in favour of certain outcomes at each test-point. This model has been put forward to account for the occasional identification of contextually probable words arriving on a 'rejected' channel, with the additional hypothesis that the filter decreased the S/N ratio of irrelevant messages rather than blocked them completely.[11]

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[10] W. P. Tanner, Jr., and J. A. Swets, A decision-making theory of visual detection, *Psychol. Rev.*, 61, 1954, 401-409.

In the present experiment, selection will be determined only by changes in test-criteria and not by the earlier filter. The two messages will arrive as one complex stimulus from which one of two different verbal sequences has to be extracted. At each test-point, certain features will be identified and found incompatible with others; those for which the test-criteria are lowest will be selected and subjected to further tests, while the remainder are discarded. Those features which are still ambivalent will accompany the selected set until some further test determines whether they belong with it or not.

Interference from a competing message can take two forms: (1) The *S*s may switch from analyzing the correct message to analyzing the irrelevant one when this is potentially intelligible to them; (2) features of the irrelevant message, whether intelligible or not, will act as noice masking features of the selected message until they have been found incompatible and discarded.

(1) The first type of interference will apply only to known languages; here the contextual probabilities will alter test-criteria and determine selection as outlined above. When the irrelevant message is in an unknown language, few of its words will fit any classifications in the hierarchy of tests. The *S*s will therefore be unable to switch to it, even when the transitional probabilities in the correct message fall to a low value. If the irrelevant message consists of nonsense pronounced as English, it will tend to fail tests at a later stage or sometimes not at all, since nonsense-sequences of English phonemes may include English words unless special care is taken to avoid them. The Czech message used in this experiment began "Te*roo* *zavalli* lesk *lid* *mok*ny *pan*dolu *a* *sak*et *arch*ivo*day*," where the italicized portions could all be English words when spoken. This would explain why the interference was greater than with an unknown foreign language and why half the *S*s thought the message might actually have been English.

(2) Interference in the form of central masking at the test-points dis-

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[11] Treisman, *op. cit.*, 212.

criminating criterial attributes of words will be produced by all types of competing message in the same voice and localization as the selected message. Its extent, however, may vary with the stage at which the decisions that separate the two messages can be made. If the irrelevant message is in a known language, there will be tests in the hierarchy adapted to identifying its features; these can therefore be recognized at an early stage, separated from those of the selected message and discarded without further analysis. If, on the other hand, the irrelevant message is totally unidentifiable, as was the reversed speech, it will fit no available phonemic or verbal classifications in the hierarchy of tests. It will therefore remain indistinguishable from a noisy component of the selected message, masking it for longer and at later stages of analysis making identification of the words less likely. This might happen also with unknown foreign languages, but to a lesser degree, since the three languages used in this experiment share many of their phonemes with English. They should thus be more easily separable from the correct message, without, on the other hand, running the risk of complete switches of attention, as did the known languages and occasionally the 'anglicized' Czech nonsense.

## SUMMARY

An experiment is reported on selective attention to one of two competing messages, presented on the same channel and differing only in verbal characteristics. A difference between two equally known languages allowed no more efficient selection than a difference in subject-matter between two messages in the same language. Unknown foreign languages, however, did produce less interference. Some selection was shown to be possible on the basis of transitional probabilities between words; the evidence suggested that irrelevant words were discarded during their identification rather than before or after. The 'filter' hypothesized by Broadbent does not seem able to select classes of words, as such, in the same way as input-channels.[12] A suggested basis for selection in the present experiments is a change in the criteria for decisions in the series of tests performed on the sounds and resulting in the identification of words.

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[12] Broadbent, *op. cit.*, 54.

# APPARENT MOVEMENT OF A NECKER CUBE

By PAUL A. KOLERS, Harvard University

Many of the figural displacements that we know of have been attributed to one underlying mechanism. Apparent movement, figural after-effects, perspective reversals of ambiguous figures, and even certain kinds of masking effect have been attributed to "satiation," the purported change a local region of the visual cortex undergoes following protracted responding to a visual stimulus.[1] Support for this assertion of a common mechanism has been derived from the apparent ability of some of these effects to affect each other: used as Inspection-Figure, an object in apparent movement is reported to produce a stronger figural after-effect than a stationary object; the path of apparent movement is reported to curve in depth around a "satiated" region; and perspective reversals are facilitated by prior inspection of a like form, as for a figural after-effect.[2] The possibility that these interactions may be due to processes other than "satiation" is always present; indeed, it has recently been shown that the intermittency of stimulation required to produce a perception of apparent movement itself has strong influences upon other figural properties.[3] Extending this work, we inquired whether the rate and number of apparent

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[1] Wolfgang Köhler, *Dynamics in Psychology*, 1940, 67-82; Wolfgang Köhler and Hans Wallach, Figural after-effects, *Proc. Amer. Philos. Soc.*, 88, 1944, 269-357; P. G. Cheatham, Visual perceptual latency as a function of stimulus brightness and contour shape, *J. exp. Psychol.*, 43, 1952, 369-380; K. T. Brown, Rate of apparent change in a dynamic ambiguous figure as a function of observation-time, this JOURNAL, 68, 1955, 358-371; H. H. Spitz, The present status of the Köhler-Wallach theory of satiation, *Psychol. Bull.*, 55, 1958, 1-28; I. P. Howard, An investigation of a satiation process in the reversible perspective of revolving skeletal shapes, *Quart. J. exp. Psychol.*, 13, 1961, 19-33.

[2] B. H. Deatherage and M. E. Bitterman, The effect of satiation on stroboscopic movement, this JOURNAL, 65, 1952, 108-109; R. J. Christman, Figural after-effects utilizing apparent movement as inspection-figure, this JOURNAL, 66, 1953, 66-72; M. B. Shapiro, A preliminary investigation of the effects of continuous stimulation on the perception of 'apparent motion,' *Brit. J. Psychol.*, 45, 1954, 58-67; Leon Cohen, Rate of apparent change of a Necker cube as a function of prior stimulation, this JOURNAL, 72, 1959, 327-344.

[3] P. A. Kolers, Some differences between real and apparent visual movement, *Vision Res.*, 3, 1963, 191-206.

changes of a Necker cube were affected by putting the cube into apparent movement.

If *S* looks fixedly at a stationary Necker cube, he soon reports that its perspective changes, and sometimes, also, that its depth has disappeared, leaving a plane figure.[4] These changes in appearance occur only after some seconds of observing, hence the interpretation that some region of the visual system must become 'satiated' for the effect to occur. If, in fact, continued stimulation were prerequisite for the effect, we would anticipate that fewer changes would be reported for a cube moved physically in and out of a target-region, since a moved cube would produce less stimulation in the target-region than a stationary one. Since a cube in *apparent* lateral movement also would be in the target-region less than a continuously presented cube, we would anticipate here also that fewer changes would be reported for such a cube than for a stationary one presented continuously. It will be shown in this paper that both of these predictions are wrong, and that an intermittently presented cube changes much more than a continuously presented one.

*Method. (1) Apparatus.* One apparatus was used to present a Necker cube intermittently or in apparent movement (Experiments I and II), another to present it in real movement (Experiment III). The latter is described below. The apparatus used in the first two experiments was a multi-chambered box in which the duration, intensity, and sequence of four visual presentations were separately controlled. The details of this device have been described fully elsewhere.[5] To produce a perception of a Necker cube in apparent movement, one condition of the experiment, two separate cubes were lighted sequentially with appropriate pauses between the offset of each and the onset of the other.

Some experiments were performed on the light-adapted eye, and others on the dark-adapted eye. Viewing distance in the first case was 100 cm., in the second, 75 cm. In both cases, *S* sat in a light-tight booth, looking through a 3-mm. artificial pupil at a small red light ($< 2'$ visual angle) with his right eye.

*(2) Materials.* The forms presented to the light-adapted eye were high-contrast photographs on matte paper; those to the dark-adapted eye were photographic negatives. Details of the apparatus made the angular subtense of the photographs one-quarter less than that of the negatives. The arrangement and angular subtense of the negatives are shown in Fig. 1. Their luminance was about 3 ft.-L. against a background of $2 \times 10^{-3}$-ft.-L. For the light-adapted eye, the field was approximately the same size, but of 4-ft.-L. luminance.

*(3) Subjects.* The *S*s for the experiments on apparent movement were Naval enlisted men, aged 18-24 yr. Three men took part in the first experiment and 14 in

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[4] R. M. Pritchard, Visual illusions viewed as stabilized retinal images, *Quart. J. exp. Psychol.* 10, 1958, 77-81; Cohen, *op. cit.*, 327-344.

[5] Kolers, Multi-field electronic apparatus for studies of visual perception, MRL-TDR-62-33, Wright-Patterson Air Force Base, 1962.

the second. Ten Harvard undergraduates were the *S*s in Experiment III, on real movement.

*(4) Procedure.* Three stimulus-conditions were used in the first experiment: (1) two cubes presented alternately at a rate that produced a perception of a single cube in movement; (2) a single cube (A in Fig. 1), presented continuously, and (3) Cube A presented intermittently at the same rate as in Condition 1. These three conditions will be referred to as $F_2$, $C_1$, and $F_1$, respectively, the initial standing for flickering or continuous, and the number showing whether one or two cubes were presented.

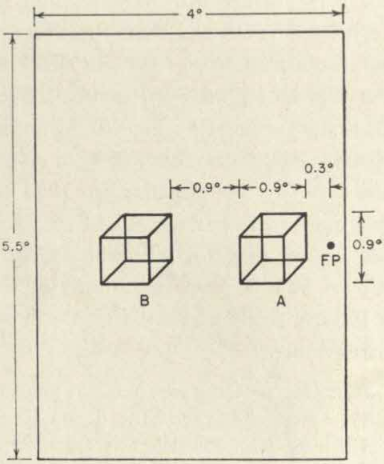

FIG. 1. ARRANGEMENT OF TEST-FORMS IN CONDITION $C_2$

Cube A only was presented for Conditions $C_1$ and $F_1$, while both A and B were presented for $C_2$ and $F_2$.

In Condition $F_2$, each cube was presented for 50 m.sec., and the interval between the offset of each and the onset of the other was 100 m.sec. In Condition $F_1$, Cube A was presented for 50 m.sec. on and 250 m.sec. off (the sum of B and the two intervals between A and B), a blank card being substituted for B, making a rate of 3.3 cps. In $C_1$, Cube A was illuminated continuously.

In each condition, two 90-sec. trials were given, separated by a 3-min. rest (4 min. in Experiments II and III). *S* signalled with a buzzer each time during a trial that he saw the cube change perspective or gain or lose depth. Fixation was restricted to the fixation-point; and for the cube in apparent movement, *S* was instructed to report on its condition only when it was near FP and to ignore its fluctuations when it was farther off. (The *S*s were not told that the movement of the cube was illusory.) All testing sessions began with a 3-min. period of dark-adaptation (10 min. in Experiments II and III) and a 1-min. period of light-adaptation to a blank field in the viewing apparatus.

In Experiment I, each one of the three *S*s observed twice in each of three test-conditions on each of six consecutive working days, according to a different random order for each *S*. In Experiment II, two groups of seven *S*s each were each tested with two 90-sec. observing periods on each of two days. On the first day, seven *S*s were tested on $F_1$ and another seven on $F_2$; on the second day, the first group of seven was tested on $C_1$, and the other seven on a fourth condition, $C_2$, with the two cubes, A and B, presented continuously.

*Results. (1) Black cubes to the light-adapted eye.* The data are plotted to show the average number of figural transformations during each 15 sec. of the 90-sec. observational period. Each plotted point in Fig. 2 is based on 36 observations, 12 from each of 3 *S*s. In all three test-conditions, the number of reversals in each 15-sec. of observing tends to increase through the first 60-75 sec. Comparison among the conditions shows, however, that the cube transforms fewer times when it is presented steadily ($C_1$) than when it is alternated; and when it is alternated, fewer times when it is seen in apparent movement ($F_2$) than when it is only alternated in place ($F_1$).

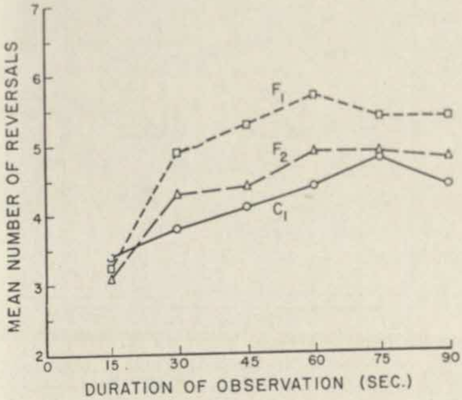

FIG. 2. PERCEIVED FIGURAL CHANGES DURING SUCCESSIVE OBSERVATIONAL PERIODS

Stimuli are black forms presented to the light-adapted eye.

These data were collected from the same three *S*s on six consecutive working days. Inspection of the raw data revealed a marked carry-over effect: rate and number of reversals on the last two days were greater than on the first, so that the data of Fig. 2 confound the effects of rate with those of level. This carry-over effect can be seen in Fig. 3, in which the total number of reversals on each condition, averaged for the three *S*s, is plotted on the ordinate, and the successive testing days on the abscissa. The figure shows that the number of reversals in each condition tends to increase with successive days. As the order in which the *S*s were presented the conditions was not balanced across days, this imbalance may account for much of the irregularity of the curves of Fig. 3.

Figs. 2 and 3, considered together, show that rate of transformation increases within a testing session, and level or number of transformations increases across testing days. Our data, however, were collected on only three *S*s, tested in all the permutations of order; hence it is not possible to isolate quantitatively the relative contributions of these two effects.

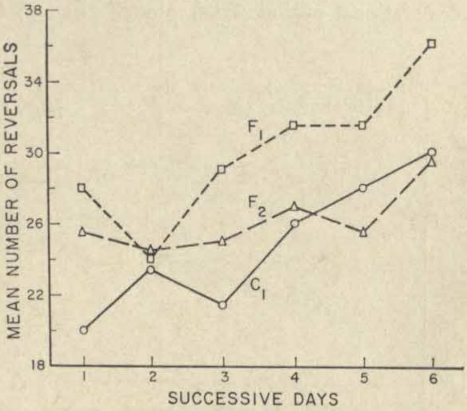

FIG. 3. AVERAGE NUMBER OF FIGURAL CHANGES ON SUCCESSIVE DAYS OF TESTING

*(2) Light cubes to the dark-adapted eye.* Because of the long-term effects shown in Fig. 3, the *S*s in Experiment II were tested much less, but a fourth condition, $C_2$, was added. In this condition, Cubes A and B were presented for continuous observation, with reports again restricted to the condition of Cube A. The other three conditions were the same as in Experiment I. Every point in Fig. 4 is based on 14 measurements, 2 from each of 7 *S*s. The curves indicate (1) that flickering a cube either alone or in apparent motion results in many more reversals than presenting one or two cubes continuously ($F > C$); and (2) that a neighboring cube does not cause a continuously presented target-cube to reverse more than the target-cube reverses alone ($C_1 = C_2$). Further, although the curve for $F_1$ shows more reversals than $F_2$, this difference may very well be an artifact of method, since the *S*s found it very difficult to report, during

$F_2$, only when the cube was precisely in the target-region. The strict criterion they were instructed to take may have acted to reduce the number of reports in $F_2$. In any case, as noted, both F-conditions show more reversals than the C-condition. Finally, it will be noted that if some carry-over effect between days existed with these *S*s, as was found in the first group (cf. Fig. 3), it would have acted to elevate performance on the $C_1$- and $C_2$-conditions in comparison to $F_1$ and $F_2$, so that the basic imbalance of the design of the experiment (all the *S*s tested first with alternated cubes) cannot be responsible for the greater number of reversals found with flickered cubes.

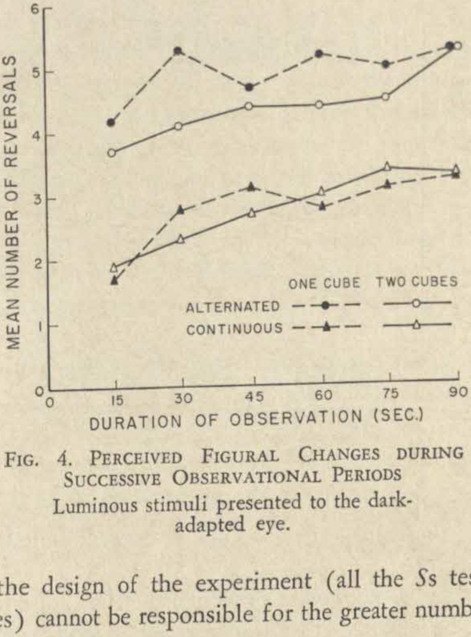

FIG. 4. PERCEIVED FIGURAL CHANGES DURING SUCCESSIVE OBSERVATIONAL PERIODS

Luminous stimuli presented to the dark-adapted eye.

*(3) Comparison of physically moved and stationary cubes.* Five *S*s observed a stationary cube for two 90-sec. trials and then a physically moving cube for two 90-sec. trials. Another five *S*s observed the moving cube first, and then the stationary cube. All trials were separated by a 4-min. rest-period. The stationary cube was 130 cm. from the right eye. The cube was moved by means of an arm attached to a General Radio Co.-type 1750-A Sweep Drive, in such a way that plotting rate of movement against time yields triangular waves, the cube accelerating linearly to the mid-point of the traverse, then decelerating linearly, and so on.[6]

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[6] I am grateful to Georg von Békésy for suggesting this method and lending me the instrument.

The *Ss* were instructed to limit their reports to the cube when it was in the target-region, an instruction identical to that given the *Ss* seeing a cube in illusory movement. The data shown in Fig. 5 indicate that a cube in the target-region about 3 cps. is reported to undergo more changes than a continuously presented cube. Regrettably, a comparison between the changes reported for real and for apparent movement cannot be made with these data because different apparatuses, different target-sizes, and different *Ss* were used for the two experiments. For the present purpose, it is sufficient to show that a moving cube underwent more transformations than a stationary one.

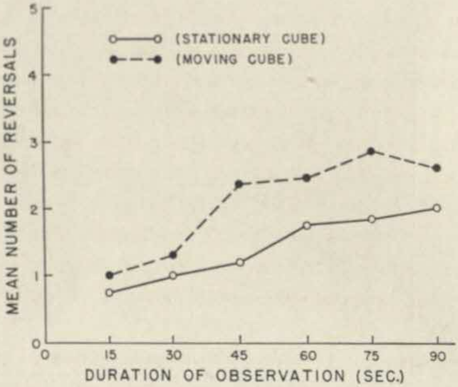

FIG. 5. FIGURAL CHANGES FOR STATIONARY AND MOVED CUBES

*(4) Qualitative observations.* (*a*) When the cube appeared in apparent movement, the *Ss* reported it as transforming at both termini of movement and also, many times, in the interspace between the termini. That is, not only is the presence of a cube reported in the interspace between the termini—a place where it never physically occurs—the cube is also reported to undergo a spatial transformation in that region.

(*b*) Although the data shown in Figs. 2 and 4 are fairly regular, reversals during a single observational period are not. A saw-toothed curve more accurately describes those fluctuations than the averages shown. Reversals tend to come in bunches, with intervals often several seconds long between them. During the stable periods between the bursts of change, the cube frequently maintains a fixed position in depth, without reversing. More often, however, when the cube is seen in a fixed position, it is reported as two-dimensional, a fact suggesting that the depth of the cube is processed as a different perceptual event from its contour.

(*c*) Recent experiments with optically stopped stimulus-objects have revealed that a Necker cube disappears in parts: apparently co-planar surfaces seem to disappear together.[7] One may then ask whether apparent movement of a configuration can be made selective to a single surface of a 3-D figure such as a cube, or even selective to one part of a complex embedded figure (Gottschaldt figure). In test-experiments, one surface of a Necker cube (that is, a square) was alternated with a whole cube, or the complex Gottschaldt figure was alternated with the simple basic figure. Under these conditions, the *S*s always reported that it was the whole form that was seen in motion, not the single surface: the cube transformed into a square in one direction of movement, and the square grew into a cube in the other. Analogous results occurred with the Gottschaldt figures. Apparent movement never acted to isolate parts of the complex figure. These observations seem to be similar to Orlansky's and to Squires's, who also found that relatively large differences are tolerable between the shapes of alternated configurations without serious impairment of the perception of movement.[8]

(*d*) When presented for 90 sec. of observation, the alternated cubes did not always appear to be in apparent movement. Rather, the *S*s often reported that apparent movement had disappeared, to reappear later, leaving in the meantime the perception of simultaneity or succession. Quantitative experiments on this rate of breakdown of apparent movement are now in progress. Observations made to date indicate that the periods for these changes seem to be much longer than those reported for reversals of perspective with cubes, Rubin vases, and other ambiguous figures, so that many fewer changes of movement occur.[9] In this breakdown of the perception of movement, it was the movement itself that disappeared and reappeared; the forms that were alternated were always clearly visible. The disappearance of apparent movement in continued observation constitutes a significant difference between that perception and the perception of real movement: 90-sec. observation of an oscillating pendulum-bob or metronome was never found to result in the disappearance of a perception of movement, nor was a like breakdown ever found when the cube was presented in real movement for equivalent durations (Experiment III).

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[7] Pritchard, *op. cit.*, 77-81.

[8] Jesse Orlansky, The effect of similarity and difference in form on apparent visual movement, *Arch. Psychol.* 35, 1940, 246; P. C. Squires, Topological aspects of apparent visual motion, *Psychol. Forsch.*, 26, 1959, 1-12.

[9] Cohen, *op. cit.*, 327-344.

*Discussion.* A Necker cube presented about three times per second in a target-region undergoes more figural changes than a continuously presented cube. This is the case irrespective of whether the cube is in apparent movement, is in real movement, or is presented in one place but intermittently. If 'satiation' of some region of the nervous system were the basis of the figural changes of a Necker cube, the continuously presented cube would have undergone the greatest number of changes. The results obtained reveal, instead, a powerful effect due to intermittency of presentation. Apparently, transients in the visual nervous system associated with the onset and termination of stimulation also affect the perceived orientation of a form. This effect of intermittency probably accounts for the influence found with apparent movement as an inspection-figure in studies of figural displacements.[10] The effects attributed to apparent movement in those experiments seem now to be due to the intermittent stimulation required to produce movement, but not to the apparent movement itself.

There is, however, at least one puzzling feature about these transients. Slow, intermittent presentations of a line tend to prevent the disappearance that otherwise occurs when forms are optically stopped. When they oscillated an optically stopped form, Krauskopf, and Burns *et al.* found maximal detection in humans and maximal cortical response in cats occurring to oscillations in the region of 3 cps.[11] But 3 cps. is about the same rate as that used here and which was found to produce a large number of figural *instabilities,* the cube perceived to be reversing many more times when presented at that rate than when presented for continuous observation (Figs. 2 and 4). Since Pritchard has already reported that an optically stopped Necker cube is seen to distort and to reverse in depth, and at about the same rate as when normally viewed,[12] the basis of this differential effect of intermittency is not clear. The difference could lie in the apparent dimensionality of the target, maximal detection being found with two-dimensional forms and maximal instability with three. The question remains open.

The observations above show that at least four different aspects of an alternated cube can be identified, each with its own binary state: (1) the cube may be faced left or right; (2) it may be seen in movement or flickering in place; (3) it may be seen in depth or in two dimensions, and (4) it may be present or altogether absent, the latter when optically stopped. Each of these four aspects seems to have its own reversal-rate. Since events encoded together cannot, logically, have separate treatments, the different rates of change imply that these aspects are the results of operations performed in different parts of the visual system. These different operations occur in a step-wise fashion, an order or hierarchy of operations characterizing the process.[13]

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[10] Christman, *op. cit.*, 66-72.

[11] John Krauskopf, Effect of retinal image motion on contrast threshold for maintained vision, *J. opt. Soc. Amer.*, 47, 1957, 740-744; B. D. Burns, Woodburn Heron, and R. M. Pritchard, Physiological excitation of visual cortex in cat's unanesthetized isolated forebrain, *J. Neurophysiol.*, 25, 1962, 165-181. Adriana Fiorentini and A. M. Ercoles (Vision with stabilized images and intermittent illumination, *Pubblicazione Istituto Nazional de Ottica*, 15, 1962, 618-633) found a slightly slower rate to be maximally effective.

[12] Pritchard, *op. cit.*, 77-81.

Finally, as noted in the introduction, many of these perceptual effects—perspective reversals, apparent movement, figural after-effects—have been attributed to the same explanatory base; namely, 'satiation.' Some form of long-term effect seems to be behind the data shown in Fig. 3, affecting the level of performance; but the data on rate of performance, in Figs. 2 and 4, comparing alternated with steadily presented stimuli, are exactly contrary to what satiational theory would seem to predict: we found that a cube with an on-time approximately one-third that of a continuously-presented cube reverses many more times than the latter. Cohen has pointed out some other ways in which perspective reversals of a Necker cube respond differently to certain stimuli from what is found with the spatial displacements of figural after-effects.[14] Particularly, perspective reversals seem to be less sensitive to spatial inductions, while being more sensitive to 'similarity' between neighboring forms. In fact, the kinds of long-term processes discussed in their great monograph by Köhler and Wallach probably have little to do with the formation of contours in *brief* presentations. Nothing like the "distance-paradox" occurs with briefly presented stimuli; the inhibitory influence of one form upon another in the latter case tends to fall off monotonically with distance between their borders.[15] A further difference is the role of contrast in the latter case: the *kind* of formation-process characterizing a figure seems to vary in a non-linear way with contrast.[16] Even though figural after-effects have been reported for moderately bright forms presented for 250 m.sec.,[17] these spatial displacements seem to involve still a different set of transformations from those discussed above. Thus, at least four factors characterize the formation of 'simple' figures such as Necker cubes, and distinguish their behavior from that of other figures; and at least two measurements—the masking test and the breakdown described in Section (4d) above[18]—distinguish real from apparent movement. Rather than attribute all the figural displacements to one mechanism, it is worth noting differences between them, and between real and illusory perception.

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[13] Other illusions may also have separable components. For example, the after-effect of looking at a rotating Archimedes spiral is a change in the apparent size of the object looked at. Since luminance-threshold for a small spot of light changes with a change in size of the spot (Ricco's Law), and if the small spot were observed after looking at an Archimedes spiral, would its luminance-threshold vary with its apparent size, or remain an unchanged function of its real size?

[14] Cohen, *op. cit.,* 327-344.

[15] Mathew Alpern, The effect of luminance of the contrast-inducing flashes on the spatial range of metacontrast, *Amer. J. Optom.,* 31, 1954, 363-369; P. A. Kolers and B. S. Rosner, On visual masking (metacontrast) dichoptic observation, this JOURNAL, 73, 1960, 2-21.

[16] Kolers, Intensity and contour effects in visual masking. *Vision Res.,* 2, 1962, 277-294.

## SUMMARY

The rate of reversal of a Necker cube was studied under three conditions: a stationary cube presented continuously; the cube moved physically; and the cube alternated in place. The last condition was studied when a single cube was alternated in place, and also when it was alternated with another to give a perception of apparent movement.

The findings were that alternating a cube, either alone or in apparent movement, or moving it physically in and out of a target-region, caused many more reversals of its perspective to be seen than presenting the cube continuously. The rate of intermittency was about 3 cps. Qualitative data on the disappearance of apparent movement in continued observation are reported, as well as the observation that apparent movement of a section of a figure does not produce the same isolation of parts that is found when the figure is optically stopped.

The findings are used to point to differences between various forms of figural displacements, such as apparent movement, figural after-effects, and perspective reversals; to differences between the perception of real and apparent movement, and to suggest the sequential nature of the processing of visual information.

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[17] Allen Parducci and Kenneth Brookshire, Figural after-effects with tachistoscopic presentations, this JOURNAL, 69, 1956, 635-639.

[18] Kolers, Some differences between real and apparent visual movement, *Vision Res.*, 3, 1963, 191-206.

# AVAILABILITY OF CATEGORIES AND CONTRAST-EFFECTS IN JUDGMENT

By B. JACK WHITE, University of Oklahoma

The typical study of contrast-effects in judgment reports that the introduction of a remote anchor to a previously judged series of stimuli produces a reduction in the frequency of use of those judgmental categories located nearest the stimulus-value of the anchor. As a result, the average category-values for the stimuli in the series are displaced toward the category most distant from the value of the anchor; thus, the range of judgments is constricted. Such effects have been demonstrated with a wide variety of stimuli, ranging from lifted-weights, visual inclinations, odors, time-intervals, and other relatively simple stimuli, to complex verbal stimuli such as the undesirability of social acts, attitude-scale items, and responses on clinical tests.[1]

The magnitude of the displacement of judgments which occurs in contrast has been repeatedly demonstrated to be a function of the degree of discrepancy of the anchor from the end of the stimulus-series. The possibility that the magnitude of displacement, as well as its form (whether constricted, uniform, or expanded), may also be a function of the number and location of judgmental categories available to *S* has not, however, been systematically investigated, although incidental observations reported in previous studies hint that such might be the case. For example, while judging a series of stimuli which included a remote anchor, McGarvey's *S*s complained about having too few categories and about being forced to put stimuli which "really don't belong together in the same category."[2] The complaints imply that if additional categories had been available beyond that end of the scale farthest removed from the value of the anchor, the *S*s would have used them; therefore, the magnitude of the contrast-effect would have been greater. The complaints

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[1] For a summary, see Muzafer Sherif and C. I. Hovland, *Social Judgment,* 1961, 44-72, 99-126.

[2] H. R. McGarvey, Anchoring effects in the absolute judgment of verbal materials, *Arch. Psychol.,* 39, 1942-43, (No. 281), 51.

also indicate that the piling up of judgments in the categories farthest removed from the anchor (*i.e.* constricted displacement of the scale) was not a function of a reduction of *S*'s ability to discriminate among stimuli in the presence of the anchor; thus, judgments might have been uniformly displaced to new categories which were farther removed from the anchor, had they been available.

The purpose of the present study was to assess the effects of a remote anchor under five variations in the number and location of categories available for judgment. The hypotheses were that when a remote anchor is interpolated into a previously judged series of stimuli, regardless of the location of the category used to define the anchor, (1) constricted displacement in judgments of the original series will occur when new categories are not available beyond that end of the judgmental scale which is farthest removed from the stimulus-value of the anchor, and (2) a greater magnitude of displacement in judgments will occur when new categories are available beyond that end of the judgmental scale which is farthest removed from the value of the anchor.

## METHOD

*Subjects.* Right-handed men and women from classes in introductory psychology who were not familiar with the problem being investigated served as *S*s. The 70 *S*s were randomly assigned to the 5 experimental conditions, 14 to each. Following a procedure used by Sherif, Taub, and Hovland,[3] the *S*s who were unable to attain at least a 50%-level of accuracy in the control-session were eliminated from the study in an effort to insure that effects found in the anchor-sessions were not a function of initial inability to discriminate among the weights. In the present study, the level of accuracy was determined by computing the percentage of the last 100 of the 300 judgments in the control-session which was in accordance with the instructions given. The probability of *S*'s reaching the criterion by chance, as determined by Chi-square, was less than 0.005.

*Apparatus.* One of the anchors and one of the series of stimuli used by Sherif, *et al.* were also duplicated, except for the size of the weight-containers.[4] The weights consisted of cylindrical containers 3.5 cm. in diameter and 9.6 cm. in height. The series in the control-session included weights of 97, 118, 137, 153, and 169 gm. The anchor weighed 41 gm. Two considerations were involved in the selection of these particular stimuli: (1) the anchor which was selected had been demonstrated to produce marked contrast-effects in the judgments of the other stimuli; and (2) the effects of the introduction of a remote anchor to the lighter end of a series of weights has been investigated in comparatively few studies.

In all of the anchor-sessions, the anchor (for which a judgment was not reported) was lifted immediately before any weight in the original series was lifted. The stimuli in the series were presented in random order, except for the restrictions that all stimuli appeared an equal number of times and that no stimulus for which a judgment was to be reported was presented twice in succession.

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[3] Muzafer Sherif, Daniel Taub, and C. I. Hovland, Assimilation and contrast effects of anchoring stimuli on judgments, *J. exp. Psychol.*, 55, 1958, 150-155.

[4] *Op. cit.*, 150-155.

The apparatus which presented the stimuli was so designed that seven *S*s could silently make judgments simultaneously. The *S*s were seated around a large circular table in such a way that each directly faced one side of a seven-sided enclosure which was mounted on the top of the table. A small opening aligned with a padded arm rest on each side of the enclosure allowed *S* to put his right hand into the enclosure which contained the weights. Elasticized cloth surrounding each opening fitted snugly around his wrist and prevented his use of visual cues for judgment. The weights were lifted directly with the hand in a uniform motion of the wrist. Judgments were reported by twisting a pointer across an 11-point dial with the left hand. A linear potentiometer attached to *S*'s pointer operated a pen on a Grass EEG recorder which produced a record of the response.

A circular platform within the enclosure held 40 weights. A timer regulated the interval between presentations of stimuli, turned on a green light when *S*s were to lift, turned the light off before the platform moved, and then turned on a motor which revolved the platform enough to bring the next weight under *S*'s hand. The apparatus operated on the following schedule: The green light which indicated that *S* should lift came on for 3.5 sec., 1 sec. elapsed after the light went off, the platform moved for 2 sec., the green light came on again, and the cycle was repeated. Thus, there was a total of 6.5 sec. between presentations of stimuli, a pace which *S*s reported was comfortable.

When the green light for the fiftieth judgment went off, a stepping switch shut off the moving parts of the apparatus and turned on a red light to indicate a 3.5-min. period of rest. Since there were 40 weights on the platform, the occurrence of the rest after the fiftieth lift insured that *S*s would not start in the same place in the order after each rest.[5] Including the time required for instructions, the 300 judgments in each session required 1 hr.

*Procedure.* The experiment took place in two sessions separated by an interval of 24 hr. In the control-session, the series of weights was presented for judgment by the method of single stimuli. The *S*s were instructed to judge in ounces, using Numbers 4-8 for the 5 weights. They were then shown the operation of the apparatus, were allowed to pass around and lift a 137-gm. weight (Stimulus 6) which, they were told, was "the middle weight in the series," and were given 20 practice-trials to insure that each understood what he or she was to do. In all sessions the *S*s were asked not to talk while judging and not to discuss their judgments during the rest-intervals.

After the control-session, the *S*s were divided into five groups which participated in separate anchor-sessions, each of which presented a different condition of availability of categories. In all conditions, the same extremely light anchor served as the standard in the method of constant stimuli.

In Condition I, a commonly used procedure was duplicated;[6] the anchor was, in effect, added to the original series of stimuli without an accompanying increase in the number of categories for judgment. The *S*s were told that the weights would be "presented in pairs," that the first member of the pair (the anchor) would always be the same weight, and that while they were not to report a judgment for the first member of the pair, they were to consider it Number 4. They were instructed to use Categories 4-8 for their judgments.

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[5] A 17-page report which gives details concerning construction of an improved version of the apparatus described here has been deposited with the Ameican Documentation Institute. Order Document No. 7439 from ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D.C.

[6] McGarvey, *op. cit.*, 1-86; Sherif, Taub, and Hovland, *op. cit.*, 150-155; Spaulding Rogers, The anchoring of absolute judgments, *Arch. Psychol.*, 37, 1941, (No. 261), 1-43.

In Condition II, to equate the number of categories and stimuli, an anchor-equivalent (A′) was substituted for the lightest weight in the original series. Thus, wherever Weight 4 appeared in the order used in Condition I, A′ appeared in that position in Condition II. The *S*s were given essentially the same instructions as those in Condition I, except for being told that whenever the first member of the pair appeared again as the second member of the pair, they were to record a 4 on their dials.

For the sake of comparability, A′ was also substituted for Stimulus 4 in Conditions III, IV, and V. The instructions in these three conditions were the same as those in Condition II, except for the number and location of new categories which *E* made available to the *S*s. In Condition III, the *S*s were allowed to judge the A′ with any number they chose between 1 and 4; however, they were to continue judging the four heavier weights in Categories 5-8. In Condition IV, the *S*s were instructed to judge A′ as 4, but were allowed to use Categories 5-11 for the four heavier weights. In Condition V, the *S*s were allowed to use Categories 1-11 in any way they chose.

## RESULTS

To increase the stability of the measures used, only the last 100 judgments (20 judgments of each stimulus) made in each session were treated in the analysis. Since responses in the end-categories were skewed, and since in some conditions (*e.g.* all control-sessions and the anchor-sessions of Conditions I and II) the instructions limited the *S*s to judgments of an ordinal nature, nonparametric statistics were used for all analyses—a treatment which has been recommended by Salzinger as more appropriate than parametric techniques for the analysis of anchor-effects.[7]

An overview of the results is presented in Fig. 1. A Kruskal-Wallis one-way analysis of variance on scores obtained by adding each *S*'s median judgments of the five stimuli showed that there was no significant difference among the groups in the control-session ($H = 1.58$; $df = 4$; $p > 0.80$). To simplify the figure, it therefore seemed appropriate to compute a grand median to represent the judgments of all the *S*s (70) in the control-session.

The distinction mentioned earlier between constricted and uniform displacement is clearly illustrated by a comparison of the line which represents judgments in the control-session with the lines which represent

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[7] Kurt Salzinger, Techniques for computing shift in a scale of absolute judgment, *Psychol. Bull.*, 53, 1956, 394-401.

Conditions II and IV. Constricted displacement is evidenced in Condition II as an increase in the steepness of the line which represents judgments in the anchor-session. Uniform displacement is illustrated in Condition IV wherein the line is almost parallel to the line for the control-session.

*Assessment of displacement in the anchor-sessions.* A score to represent the total displacement in each *S*'s judgment was obtained in the following manner. Since the A′ was not substituted for Stimulus 4 in Condition I, only the median judgments of Stimuli 5, 6, 7, and 8 were added to obtain a score for the control-session and a score for the anchor-session for each *S*. Corresponding scores from the two sessions were then subtracted to obtain total displacement-scores. A Kruskal-Wallis one-way analysis of variance on the resulting scores indicated significant differences among the five conditions ($H = 24.61$; $df = 4$; $p < 0.001$). Further analysis by the Mann-Whitney *U*-test indicated there were no significant differences among Conditions I, II, and III or between Conditions IV and V. Comparisons between the first three conditions and the last two indicated, however, that there was greater displacement in Conditions IV and V than in I, II, or III ($p < 0.01$ in each comparison). In both Condition IV and V there were a few *S*s who occasionally used a fifth category for the four heavier stimuli—an occurrence which could have produced a significant increase in the number of categories in the judgmental scale, had it been more frequent. One or more of the extra cate-

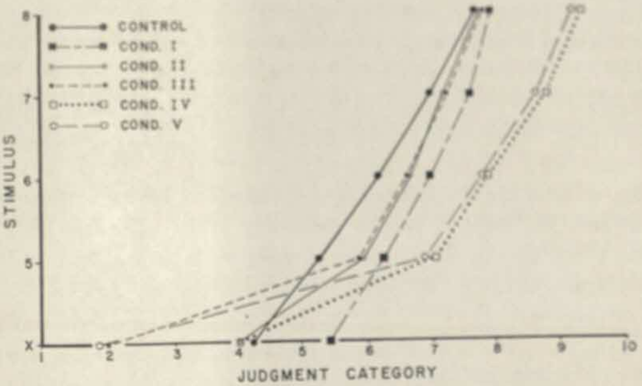

FIG. 1. MEDIAN JUDGMENTS IN THE COMBINED CONTROL-SESSIONS AND THE FIVE ANCHOR-CONDITIONS

The 'X' on the ordinate indicates that an anchor-equivalent was substituted for Stimulus 4 in Conditions II, III, IV, and V.

gories (evidenced by median judgments above 8.0) were used by 11 of the 14 *S*s in Condition IV, and by 12 of the 14 in Condition V.

The magnitude of displacement in the judgments of each stimulus (including Stimulus 4 in Condition I) was also evaluated. The *S*s' median judgments of a given stimulus in the control-session were compared with corresponding medians from the anchor-session by means of a Wilcoxon matched-pairs signed-ranks test. The various tests indicated that, with the exception of Stimulus 8 in Conditions II and III, judgments of each of the stimuli in the original series underwent significant displacement away from the anchor in all conditions ($p < 0.01$ for each comparison).

Though hypotheses concerning judgments of the A′ and the stimulus for which it was substituted, Stimulus 4, were not formulated, comparisons indicated that there was a significant difference ($p < 0.01$) between judgments of the two stimuli in each condition where both were used. In every case, the A′ received a lower median judgment than did Stimulus 4.

*Assessment of changes in the range of judgments in the anchor-sessions.* In view of the results of studies mentioned earlier, there was reason to expect constriction in the range of judgments in Conditions I, II, and III; therefore, one-tailed statistical tests were performed on the data from these conditions.

A Wilcoxon matched-pairs signed-ranks test indicated that a significant amount of constriction in the range encompassed by median judgments of Stimuli 4 and 8 occurred in Condition I ($p < 0.005$). Since Stimulus 4 was replaced by an A′ in Conditions II, III, IV, and V, the difference-scores compared in these conditions were based on the range between median judgments of Stimuli 5 and 8. For comparative purposes, similar scores were obtained for Condition I.

A significant amount of constriction in the range of judgments for the four heavier stimuli was found in Conditions I ($p < 0.005$), II ($p < 0.005$), and III ($p = 0.05$). A Kruskal-Wallis one-way analysis of variance indicated that there was no difference in the amount of constriction in the three conditions ($H = 2.93$; $df = 2$; $p > 0.20$).

Because there was as much reason to expect expansion as there was to expect constriction in Conditions IV and V (where Categories 9, 10, and 11 were available), two-tailed Wilcoxon matched-pairs signed-ranks tests were performed on the difference-scores for these conditions. No significant change in the range of judgments occurred in either condition. Thus, the significant amount of displacement which did occur in Conditions IV and V was uniform in nature.

*Effects of the location of the category assigned to the anchor-equivalent.* Mann-Whitney *U*-tests indicated there was no significant difference between median judgments of the A′ in Conditions II and IV (where the *S*s were instructed to use Category 4 for the A′) or between Conditions III and V (where they could use lower categories for the A′). In both comparisons, $p > 0.10$. Comparisons of Conditions III and V with Condition IV indicated, however, a difference significant beyond the 0.2% level in each case. Thus, when Categories 1, 2, and 3 were available for judgment of the A′ (as in Conditions III, and V), they were generally used in preference to higher categories.

If proximity of the category assigned to the A′ were a factor in the magnitude of displacement in judgments of the original series, then more displacement should have occurred in Condition II than in III, and in Condition IV than in V. As indicated earlier, such was not the case when total displacement-scores were analyzed. Since, however, these scores were gross measures which might conceal important differences among the conditions, a further analysis was carried out. Any effects of the A′ should have been most evident in judgments of those stimuli located nearest to it, *i.e.* Stimuli 5 and 6. Accordingly, individual comparisons of the displacement in judgments of each of the four heavier stimuli were made for Conditions II and III and for Conditions IV and V. Mann-Whitney *U*-tests indicated that there was no significant difference for any comparison. Thus, the category assigned to the A′ had no effect on the judgment of any stimulus.

## Discussion

Studies commonly cited as illustrative of contrast-effects, such as Rogers' study of judgments of visual inclinations and lifted-weights and McGarvey's study of judgments of the prestige of occupations and the desirability of social acts,[8] have clearly shown that constriction in the range of judgments of the original series increases as a function of the remoteness of the anchor-stimulus. At first glance, it would appear, however, that some degree of constriction was inevitable in these studies once the anchor became discriminably different from the original series. As an illustration, in Rogers' study of lifted-weights, the *S*s first judged six weights, using Numbers 1-6. They were then exposed to anchor-sessions in which they were instructed to assign the value *six* to weights (anchors) which were the same as, or heavier than, the heaviest weight in the original series. *S* continued using the same categories, though in the sessions where the heavier anchors were used, *S* was judging seven stimuli with six numbers. Under the circumstances, judgments of the six stimuli in the original series had to be compressed into Categories 1-5. Thus it appears that constricted displacement was an inevitable artifact of the experimental procedure. Data from the present study indicate, however, that the presence or absence of constricted displacement is not a function of the location of the category assigned to the remote anchor; rather, constricted displacement occurs only when previously unused categories are not available beyond that end of the judgmental scale which is opposite the value of the anchor. Further, the magnitude of displacement which occurs in contrast appears to be unrelated to the category assigned to the anchor.

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[8] Rogers, *op. cit.*, 1-43; McGarvey, *op. cit.*, 1-86.

One implication of the results of the present study is obvious. A change in judgment brought about through the introduction of a remote anchor can be maximized by making categories available beyond that end of the judgmental scale which is farthest removed from the value of the anchor.

## SUMMARY

This study explored the generality of contrast-effects under five conditions of availability of categories. The *S*s served in a control-session and an anchor-session. Treatment was the same in all five control-sessions; the *S*s were presented a series of five weights, by the method of single stimuli, which they were to categorize with Numbers 4-8. In the anchor-session, an extremely light anchor was interpolated into the original series and judgments were obtained by the method of constant stimuli.

In Condition I, all of the weights in the original series were presented along with the anchor. The *S*s were instructed to consider the anchor as Number 4 and to continue using Categories 4-8; thus, there was one less category than the number of stimuli presented—a procedure frequently used in previous studies. In Condition II, the number of categories and stimuli was equated by replacing the lightest weight in the original series with a weight equivalent to the anchor. An anchor-equivalent (A′) was also substituted for Stimulus 4 in the three remaining conditions. In Condition III, Numbers 1-8 were available in the anchor-session; in Condition IV, Numbers 4-11 were available; and, in Condition V, Numbers 1-11 were available.

There were no significant differences in the magnitude of displacement among Conditions I, II, and III or between Conditions IV and V; however, there was a greater amount of displacement in IV and V than in any of the other three conditions, because some of the 'new' categories available in IV and V were used for judgment of the heavier stimuli. The

location of the category assigned to the A′, whether proximal or distal with regard to the categories used for the other stimuli, had no effect on the amount of displacement in the judgment of any stimulus.

A significant amount of constriction in the range of judgments (*i.e.* constricted displacement) occurred in those conditions in which additional categories were not available beyond that end of the judgmental scale which was farthest removed from the anchor (*i.e.* in Conditions I, II, and III). Displacement was uniform in Conditions IV and V, where the new categories were used.

# ACQUISITION AND TRANSFER IN AUDITORY DISCRIMINATION

By HARLAN LANE, University of Michigan

The acquisition of a discrimination among verbal stimuli may have a time-course between two extremes. If *S* does not already discriminate among the stimuli, we may expect "an increase in the tendency to confuse the items, followed by the development of discrimination. Learning time should be at a maximum in this case. If such discrimination already exists, learning time should be at a minimum."[1] The former state of affairs we typically associate with the acquisition of non-verbal discriminations by human and infra-human *Ss*. Many years of training in the discrimination of verbal stimuli, which is largely the product of the human *S*'s linguistic community, may make it impossible for him ever to learn a verbal discrimination *de nouveau*. This conditioning history often leads to such rapid acquisition of discriminations as to obscure the fact that essentially the same principles are involved in verbal as in non-verbal learning.[2]

In the present study, we inquire to what extent learning to discriminate among the speech-sounds of a second language is characterized by the gradual acquisition of new discriminations (maximal learning time) and to what extent by the transfer of prior discriminations (minimal learning time). To the degree that first-language discriminations influence discriminative learning among the vowels of a second language, the influence may be described primarily as a negative transfer-effect; that is, "generalization with a previous task occurs, but the situation is such that discrimination between some aspect of the two tasks themselves is required, as well as learning of the second."[3] Transfer from first- to second-language vowel-discrimination may be expected to occur because the stimulus-dimensions are the same in both cases: primarily, the frequencies of the first and second formants. *Negative* transfer is expected to occur because the boundaries of equivalence-classes along these dimensions vary from language to language; an allophone of phoneme *a* in one language may be an allo-

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[1] E. J. Gibson, A systematic application of the concepts of generalization and differentiation to verbal learning, *Psychol. Rev.*, 47, 1940, 197.

[2] F. S. Keller and W. N. Schoenfeld, *Principles of Psychology*, 1950, 61.

[3] Gibson, *op. cit.*, 197.

phone of phoneme *b* in another. Liberman *et al.* have stated this well: "A person who is newly exposed to the sounds of a strange language finds it necessary to categorize familiar acoustic continua in unfamiliar ways."[4]

The experiments to be reported describe discriminative learning among the vowels and consonants of an Indo-European language (Spanish) and among certain vowel-like sounds. A description of the time-course of discriminative learning in these experiments permits us to infer the degree of transfer of the *S*'s prior discriminations.

## EXPERIMENT I: THE DISCRIMINATION OF SPANISH VOWELS

*Procedure.* Six Spanish vowels (ɔ, e, a, æ, i, ai), rendered by a linguist, were recorded in irregular order at 4-sec. intervals on a two-track tape recorder (Uher III A). One of the vowels, /a/, was designated as the positive discriminative stimulus ($S^D$); it appeared 30 times on the tape, while each of the other vowels ($S^\Delta$s) appeared 6 times. A 4-sec. coding tone was recorded on the second track of the magnetic tape adjacent to each presentation of the $S^D$. During playback, the vowel-signals were applied to a high-fidelity, binaural headset worn by *S*, while the coding tone operated control equipment. *S* was seated in an audiometric room in front of a microphone and a counter. Three men, undergraduate students, served individually in sessions lasting 30 min. They were instructed to respond by saying /ka/, so as to accumulate points on the counter. Each vocal response by *S* triggered a voice-operated relay (*VOR*); if the *VOR* operated while the coding tone was on, *S* received one point on his counter. The latencies of the responses were measured from the onset of the stimulus to the onset of the response with an accuracy of ± 5 m.sec. (Hewlett-Packard 522B frequency-counter). The series of 60 stimuli was presented repeatedly until *S* made less than four errors (failed to respond to $S^D$ or responded to $S^\Delta$) in a given trial; the experiment was then terminated.

*Results.* Fig. 1 shows, for a typical *S*: (a) the percentage of stimuli responded to in $S^D$ and $S^\Delta$ on successive trials; (b) the mean latency of responses over trials, and (c) the cumulative latency of consecutive responses in $S^D$ and $S^\Delta$ on the first trial. If *S* were learning to discriminate among the vowels of Spanish, we might expect the two dependent variables, conditional probability of response and latency of response, to change during the course of conditioning in the following fashion: The probability of responding to $S^D$ should be maintained or increased, while the probability of responding to $S^\Delta$ should gradually decrease. We may expect, as well, concomitant changes in response-latency; those to $S^D$ should decrease, those to $S^\Delta$ increase.

From the outset of the experiment, $S_1$ responded to $S^D$ nearly 100% of the time (Fig. 1 A); responses to $S^\Delta$ fell abruptly after Trial 1. There was no systematic change in the latency of responses to $S^D$ or $S^\Delta$ during the first trial (Fig. 1 C), although the few $S^\Delta$ responses that did occur had longer latencies than those to $S^D$. $S_2$ also showed nearly 100% responses to $S^D$ and an initial drop in responses to $S^\Delta$ after the first trial. $S^D$ and $S^\Delta$ latencies diverged slightly during the first seven responses, but subsequently there were no $S^\Delta$ responses at all. $S_3$ presented the same picture of abrupt discriminative learning.

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[4] A. M. Liberman, K. S. Harris, H. S. Hoffman, and B. C. Griffith, The discrimination of speech sounds within and across phoneme boundaries, *J. exp. Psychol.,* 54, 1957, 358-368.

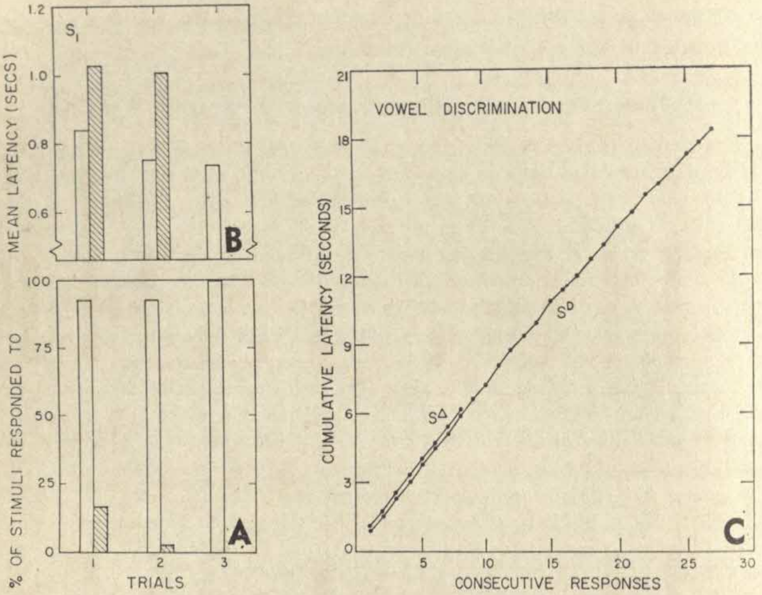

FIG. 1. VOWEL-DISCRIMINATION, $S_1$

(A) Percentage of stimuli responded to in $S^D$ (unfilled histograms) and $S^\Delta$ (filled histograms); (B) Mean latency in $S^D$ and $S^\Delta$; (C) Cumulative latency of consecutive responses in $S^D$ and $S^\Delta$ during Trial 1.

These findings show that the training procedure did effect a change in the discriminative behavior of the *S*s; however, the change took place so abruptly that it seems more appropriate to call it discriminative transfer than discriminative learning. We may infer that the experiment sampled, to a large extent, the prior discriminative repertories of the *S*s, acquired after years of first-language conditioning. It is interesting, therefore, to examine discriminative learning with an auditory continuum that does *not* play a major role in the discrimination of speech-sounds. In the hope of observing human discriminative learning in a more pristine form, the second experiment required discriminations among the rise-times of vowels.

## Experiment II: The Discrimination of Rise-Time

*Procedure.* The arrangement of the apparatus, the format of the stimulus-tape, the *S*s and the instructions were exactly the same as in Experiment I; only the stimulus-variable was changed. Six rise-times for the vowel-phoneme /a/ were obtained by gating the $S^D$ of Experiment I with an electronic switch (Grason-Stadler 829) with variable rise-decay time. All six stimuli had the same duration (150 m.sec.) and the same relative amplitude (within 2 db.). The rise-times of the gated signals were determined by processing the tape with an average speech-power circuit (integrating time 10 m.sec.), displaying the output-voltage as a function of time on a calibrated oscillograph (Minneapolis-Honeywell Visicorder), and measuring the time from onset to the steady-state of each signal. The values employed for the rise-time variable were: 80, 65, 50 ($S^D$), 40, 35, and 25 m.sec.

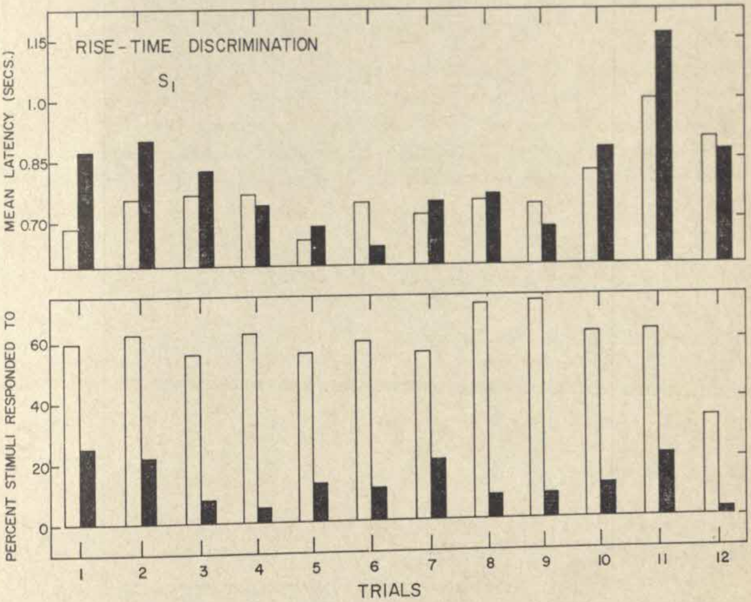

Fig. 2. Rise-Time Discrimination, $S_1$

Percentage of stimuli responded to and mean response-latency for $S^D$ and $S^\Delta$ on each of 12 trials.

*Results.* Fig. 2 shows the frequency and latency of responses to $S^D$ and $S^\Delta$ by $S_1$ during his 12 trials. The *S* responded about 65% of the time to

$S^D$, and about 20% of the time to $S^\Delta$; these percentages did not change appreciably until the last trial, when only 33% of the $S^D$s and 2% of the $S^\Delta$s evoked responses. The latencies of responses to $S^\Delta$ are, in general, longer than those to $S^D$; however, the mean latencies under these conditions do not diverge during the experiment. A breakdown of the latencies of responses to $S^D$ and $S^\Delta$ is given in Fig. 3 for the first two trials. Far from increasing, latency of responding to $S^\Delta$ decreases during Trial 1, fluctuating slightly thereafter. The latency of responding to $S^D$ shows an initial decrease but does not change appreciably over the rest of the session. If "extinction is the hallmark of discrimination learning,"[5] then $S_1$ did not learn a discrimination at all.

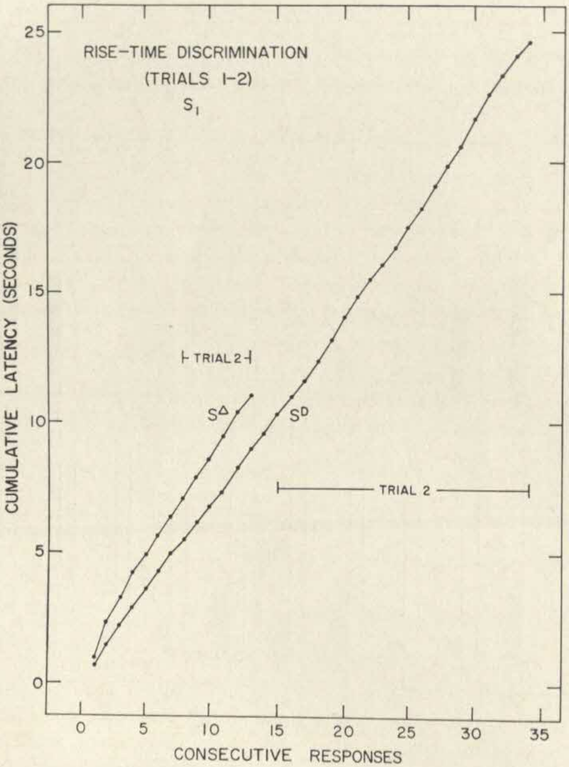

FIG. 3. RISE-TIME DISCRIMINATION, $S_1$
Cumulative latency of consecutive responses in $S^D$ and $S^\Delta$ during Trials 1 and 2.

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[5] Keller and Schoenfeld, *op. cit.*, 119.

The findings for $S_2$ and $S_3$ were similar. The properties of discriminative learning observed with rise-time as the stimulus-variable are, therefore, similar to those when vowel-quality was the stimulus-variable. What little change may take place in the frequency and latency of responding to $S^D$ and $S^\Delta$ takes place early in the session. Thereafter, differential responding does not change appreciably. The major difference between rise-time and vowel-quality as stimulus-variables seems to be only in the higher frequency of responding in $S^\Delta$ with the rise-time stimuli.

## EXPERIMENT III: DISCRIMINATIVE LEARNING WITH AN AUDIO-LINGUAL PROGRAM

*Procedure.* A self-instruction program was prepared to teach discriminations among the vowels and consonants of Spanish. The program was subdivided into 14 frames each of which comprised approximately 60 stimuli. Each frame included one $S^D$ and several English and Spanish $S^\Delta$s (generally 12 in number), which were presented several times each in irregular order. The 14 frames as well as the 60 stimuli within each frame were programmed, in the sense that items and frames were ordered according to a tentative schedule of difficulty for the English-speaking student. The stimuli were rendered by a linguist at approximately 4-sec. intervals and recorded on magnetic tape. In the manner of Experiment I, coding tones were recorded adjacent to each $S^D$ on a second tape-track; these tones served during playback to control reinforcement. The *S* listened to the stimuli with a high-fidelity binaural headset while seated in front of a lever (Lindsley manipulandum) and add-subtract counter within an audiometric room. He was instructed to respond after the first stimulus in the frame and after all subsequent presentations of that particular sound. If *S* responded to an $S^D$ or did not respond to an $S^\Delta$ (during the 4-sec. interstimulus-interval), the counter added one point; if *S* responded to an $S^\Delta$ or failed to respond to an $S^D$, the counter subtracted one point. Phase I of the experiment constituted a pre-test: each of the three *S*s was presented with the tape-recorded program, but the reinforcement-device was disconnected. In Phase II, the contingencies of reinforcement were in effect. *S* repeated each frame until he made eight errors or less; he then advanced to the next frame. Phase III constituted a post-test; the procedure was identical to that for Phase I.

TABLE I

RESPONSES ON PRE-TEST AND POST-TEST TO PHONETIC $S^D$s AND $S^\Delta$s IN AN AUDIO-LINGUAL PROGRAM

| *S* | % responses to $S^D$ | | % responses to $S^\Delta$ | | % Correct ($S^D$:R+$S^\Delta$:∼R) | |
|---|---|---|---|---|---|---|
| | pre- | post- | pre- | post- | pre- | post- |
| 1 | 55 | 76 | 19 | 21 | 67 | 79 |
| 2 | 61 | 76 | 29 | 19 | 68 | 79 |
| 3 | 53 | 72 | 34 | 20 | 65 | 82 |
| 4 | 53 | 77 | 19 | 17 | 70 | 86 |

*Results.* Table I shows the percentage of correct responses before and after the training phase. It is clear that within the 3-hr. session we effected an appreciable change in the discriminative repertories of our students. It is equally clear that our tentative program could be revised extensively. It will be noted that the increase in percentage of responses to $S^D$ was greater than twice the decrease in the percentage of responses to $S^\Delta$. This finding is evidence of the *S*s' prior discriminative training. It means that our *S*s were, initially, over-discriminating and that, during the training phase, they learned to respond correctly to more members of the population of stimuli.

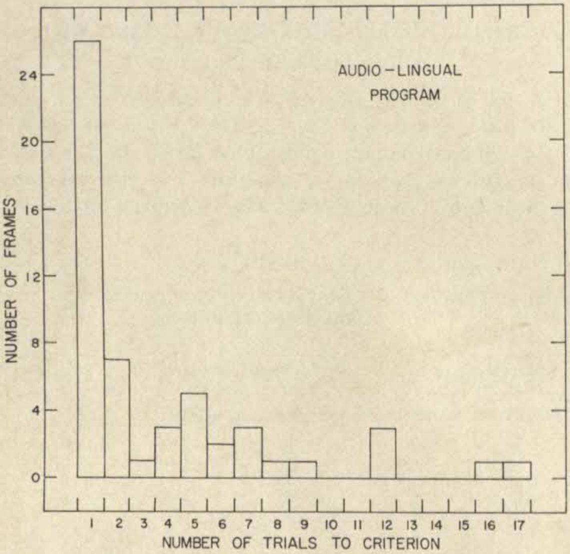

FIG. 4. DISCRIMINATIVE LEARNING WITH AN AUDIO-LINGUAL PROGRAM ($N = 3$)
The number of frames in which the *S*s reached criterion within 1 through 17 trials.

Fig. 4 presents the number of frames in which the *S*s reached criterion within Trials 1 through 17. Most of the phonetic discriminations were mastered in a single trial (during Phase II), which again shows the transfer of the *S*s' prior training. The transfer was qualitatively similar for the three Midwest American *S*s. The 14 frames were rank ordered for each *S* according to his errors during the pre-test. The correlation among the three rank orders was $W = 0.60$ ($p < 0.01$; Kendall's coefficient of concordance).

A third way of describing the relation between conditioning and the change in the discriminative behavior of our *S*s is presented in Fig. 5. For each of the 14 frames, the number of errors made during Phase II is plotted against the percentage increase in correct responses from pre- to post-test. There are some frames in which the number of errors, as well as the increase in percentage correct, is small. There are also several frames in which the number of errors is again small, but the increase in percentage correct large; this is the discontinuity in discriminative learning to which the present paper is addressed. We infer that both findings reflect the prior training of our *S*s.

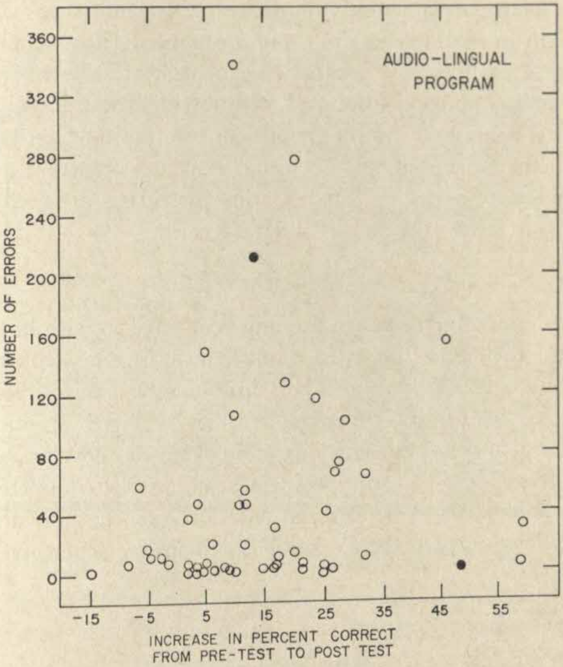

FIG. 5. DISCRIMINATIVE LEARNING WITH AN AUDIO-LINGUAL PROGRAM ($N = 3$)

For each of 14 frames, the number of errors made during discriminative learning is plotted against the percentage-increase in correct responses from pre- to post-test.

When we consider the time-course of discriminative learning with the frames of the audio-lingual program, two kinds of patterns emerge. The first, and more common, pattern is presented by the filled circle at the lower right of Fig. 5. In this frame, *S* responded to the first $S^D$ during discriminative training (as instructed) but failed to respond to the next two presentations of $S^D$. Subsequently, all responses were reinforced. This limited training, if it may be called that, effected an increase of 47% in the number of correct responses from pre- to post-test. The $S^D$ was the Spanish initial or intervocalic /g/; $S^\Delta$s were: /gw/, /k/, /kw/.

The other representative pattern of discriminative learning was far less frequent: many errors during discriminative training and a substantial improvement in performance from pre- to post-test (filled circle, Fig. 5). These data show the more gradual development of differential stimulus-control which is normally associated with discriminative learning. The $S^D$ was a trilled Spanish /r/ in intervocalic or final position; $S^\Delta$ was an English /r/ in the same positions. Erroneous responses occurred to over 60% of the stimuli presented in Trial 1; this percentage dropped somewhat with each successive trial and by Trial 12 reached 5%.

## SUMMARY

When auditory discriminative training is undertaken with human adults, the effect of their prior history of reinforcement is certain to play a major role in the course of learning. This history may manifest itself in the extensive control of discriminative behavior observed at the outset of training, or it may appear as a discontinuity in the development of differential responding—an abrupt increase to nearly complete stimulus-control. There seem to be few, if any, auditory continua that do not sample, at least in part, the *S*'s prior discriminative repertory.

# SOME SEMANTIC RELATIONS AMONG WORD-ASSOCIATES

By HOWARD R. POLLIO, University of Tennessee

Any given word may be thought to evoke at least two different classes of semantic response: word-associations ($r_a$) and conditioned meanings ($r_m$). Norms of word-association indicate that there is a relatively small number of preferential associates to any given stimulus-word.[1] One reasonable hypothesis to account for this is to assume that the meaning conditioned to a stimulus-word in part determines the nature of its associated response. This communality in meaning results from the fact that a stimulus-word and its primary word-associates have occurred, and continue to occur, in a number of similar situations and linguistic contexts. Both, therefore, have, or come to have, similar meanings conditioned to them according to the mediation-paradigm of meaning acquisition.[2] In terms of the hypothesis advanced by Osgood *et al.*, a stimulus-word and its primary associates should be expected to have proximal locations in semantic space.[3]

Staats and Staats have provided some confirmation for this hypothesis in showing that the rank-order correlation between the evaluative semantic differential rating of 10 words and the mean evaluative semantic differential rating of each of these word's first 20 associates is 0.90 ($p < 0.01$).[4] A recent compilation of semantic differential profiles for 360 words, including 52 Kent-Rosanoff word-pairs, permits a more extensive analysis of the semantic relations existing among word-associates across all three major semantic dimensions.[5]

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* Received for publication January 28, 1963. The author's thanks are due Professor J. J. Jenkins for the raw data used in the present analyses, and to Richard Cave and Edward Christy for the compilation of the data used in Analysis III.

[1] G. H. Kent and A. J. Rosanoff, The complete Minnesota norms for responses to 100 words from the Kent-Rosanoff word association test, *Tech. Report No. 11,* University of Minnesota, 1954.

[2] C. E. Osgood, *Method and Theory in Experimental Psychology,* 1953, 690-699. C. E. Osgood, G. J. Suci, and P. H. Tannenbaum, *The Measurement of Meaning,* 1957, 5-9.

[3] Osgood, Suci, and Tannenbaum, *op. cit.,* 25-30.

[4] A. W. Staats and C. K. Staats, Meaning and *m:* correlated but separate. *Psychol. Rev.,* 66, 1959, 136-144.

[5] J. J. Jenkins, W. A. Russell, and G. J. Suci, An atlas of semantic profiles for 360 words, this JOURNAL, 71, 1958, 688-699.

## ANALYSIS I

In this first analysis, we were interested in examining the degree to which word-associates tend to have congruent semantic locations. In order to determine this, we have examined the semantic differential rating of 52 Kent-Rosanoff stimulus-words and the semantic differential rating of each of these word's primary associates on the three main semantic dimensions described by Osgood *et al.* through the use of a product-moment correlation between these two ratings.[6]

*Procedure.* The semantic differential ratings used in the present analyses were taken from the list compiled by Jenkins *et al.* and are based on the mean semantic differential ratings obtained from a college population.[7]

TABLE I

CORRELATION BETWEEN THE SEMANTIC DIFFERENTIAL RATING OF A STIMULUS-WORD AND THE SEMANTIC DIFFERENTIAL RATING OF ITS FIRST ASSOCIATE FOR BOTH CHILD AND ADULT *S*s

| Subject-group | Semantic factor<br>Evaluative | <br>Potency | <br>Activity | Number of pairs |
|---|---|---|---|---|
| Adult | 0.52** | 0.255* | 0.36** | 52 |
| Child | 0.64** | 0.69** | 0.44** | 38 |
| Z: | 2.60** | 8.31** | 1.36 | |

** $P<0.01$
* $P<0.06$

The specific word-pairs used in these and subsequent correlations appear in the revised Kent-Rosanoff word-list.[8] Any pair of words in which both words produce each other as associative responses, *e.g.* TABLE-CHAIR and CHAIR-TABLE, was counted as only a single entry in computing the various correlations.

*Results.* As can be seen in Table I, correlations between the semantic differential ratings of these 52 stimulus-words and of their respective primary associates range from 0.52 for the Evaluative factor, to 0.255 for the Potency factor, and to 0.36 for the Activity factor. None of these correlations is, however, nearly as high as the value reported by Staats and Staats for the Evaluative factor alone.[9]

## ANALYSIS II

One major reason for the difference between our results and those reported by Staats and Staats concerns a special characteristic of primary associates, associates which are often contrast responses to the initial stimulus-word. Osgood, in his discussion of this problem, attributes the contrast response to a process of cultural verbal conditioning in which meaningful support is unnecessary in eliciting the opposite as a word-associate.[10] As evidence for this view he cites data showing that the latency of an opposite associate is shorter than for any other type of associate, and that a great deal of stereotypy exists within this class of associative response, relative to other associative response-classes. If cultural verbal learning is important in establishing the contrast response as a word-associate, then the number of contrast responses produced by a group of subjects should increase as a function of increasing age. In terms of the present analysis, correlations between the semantic differential rating of an initial stimulus-word and the semantic differential rating of that word's primary associate should be higher for children than for adults.

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[6] Osgood, Suci, and Tannenbaum, *op. cit.*, Chap. 2
[7] Jenkins, Russell, and Suci, *op. cit.*, 695-699.
[8] Russell and Jenkins, *op. cit.*, 3-39.
[9] Staats and Staats, *op. cit.*, 140.

*Procedure.* Word-association norms for children from 9-12 yr. of age, using largely the same stimulus-words as appear in the Kent-Rosanoff list, were collected by Woodrow and Lowell.[11] Within these norms there are 38 word-pairs for which semantic differential ratings are available for both the stimulus- and response-members.

*Results.* Correlations involving the semantic differential ratings of these 38 associative word-pairs are presented in Table I. For all three dimensions, these correlations are significantly different from zero and are all higher than the corresponding correlations involving the 52 word-pairs obtained from the adult association-norms.

All correlations presented in Table I were next transformed by the Fisher r-z method. A normal deviate (Z) test applied to these transformed scores indicated that primary associates produced by children correlate more highly with their evoking stimulus-word across all three semantic dimensions than is true of those produced by adults; although only differences between correlations for the Evaluative and Potency dimensions reached an acceptable level of statistical significance. Taken together, the results of the first two analyses indicate that preferential first associates are mediated, to some extent, by connotative meaning similarity, and that this type of mediation is more important in determining word-associates for children than for adults.

## ANALYSIS III

One salient characteristic of primary associates is that they differ in terms of their probability of evocation by a stimulus-word. Marbe long ago noted that the latency of an associative response is inversely related to the communality (or probability) of occurrence of that response as an associate in a population of subjects.[12] The correlations between communality and reaction latency reported in early studies ranged from "0.11 to 0.59, with a general average of about 0.33".[13] A more recent analysis yielded a correlation of −0.80 between log associative reaction-time and associative communality.[14] These results indicate that associative communality may be used to estimate the strength of a learned connection between two words. If contrast responses depend not upon meaningful congruence, but upon an overly strong learned paired-associate connection, then we should expect systematic differences in the relative importance of $r_m$ in mediating associative responses as a function of the communality of the primary associate. Specifically, correlations between the semantic differential rating of a stimulus-word and the semantic differential rating of its primary associate should be higher for low communality word-association pairs than for high communality pairs. In the case of high communality pairs, it is possible to assume that a learned paired-associate habit is the single most important factor determining the characteristics of the associated response.

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[10] Osgood, *op. cit.,* 709-712.

[11] Hebert Woodrow and Frances Lowell, Children's association frequency tables, *Psychol. Monogr.,* 22, 1916 (No. 97), 1-110.

*Procedure.* The distribution of associative communalities for the 52 word-pairs used in our initial analyses range between 0.09 and 0.83. In order to provide data appropriate to test the hypothesis that meaningful mediation is more important in determining the characteristics of associative responses for low than for high communality word-pairs, these 52 words were divided into 2 groups. A high communality group included all of those word-pairs in which the probability of a primary associate fell in the upper third of the distribution of associative communalities (N = 17), while a low communality group included all of those word-pairs falling in the lower two-thirds of this distribution (N = 35). The mean associative probability for pairs contained in the high group was 0.65, while the comparable figure was 0.26 for the low group.

*Results.* As can be seen in Table II, correlations for high communality word-pairs do not differ significantly from zero, while all correlations for low communality word-pairs significantly exceed this value. These correlations were next transformed by the Fisher r-z method and Z tests then applied to evaluate the significance of the difference between correlations for the high and low communality groups. Although all of the Z's obtained were in the predicted direction, only the difference between high and low communality word-pairs on the Evaluative dimension reached an acceptable level of statistical significance.

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[12] Albert Thumb and Karl Marbe, *Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung,* 1901

[13] R. S. Woodworth, *Experimental Psychology,* 1938, 361.

[14] Harold Schlosberg and Charles Heineman, as reported in R. S. Woodworth and Harold Schlosberg, *Experimental Psychology,* 1954, 61-65.

These results of Analyses I, II, and III indicate that there are at least two distinct, but interrelated, relations possible among word associates: relations based on connotative similarity and relations based on simple word-word habits. In the case in which word-word habits are strong, as is true for some of the associative responses produced by adult subjects (*i.e.* for high communality word-associates) the influence of connotative similarity is small. Where word-word habits are weak, as is true for most of the associative responses produced by children, as well as for low communality word-associates produced by adults, the influence of connotative meaning factors is greater. Undoubtedly, there is a large middle ground in which both factors correlatively determine the course of overt association.

TABLE II

CORRELATIONS BETWEEN THE SEMANTIC DIFFERENTIAL RATING OF A STIMULUS-WORD AND THE SEMANTIC DIFFERENTIAL RATING OF ITS FIRST ASSOCIATE FOR HIGH AND LOW PROBABILITY WORD-PAIRS FOR ADULT *S*s

| Nature of word-pair | Mean probability of primary associate | Semantic factor<br>Evaluative | Semantic factor<br>Potency | Semantic factor<br>Activity | Number of pairs |
|---|---|---|---|---|---|
| High | 0.65 | 0.06 | 0.00 | 0.09 | 17 |
| Low | 0.26 | 0.66† | 0.41† | 0.42† | 35 |
| Z: | | 2.30* | 1.39 | 1.14 | |

* $P<0.05$
† $P<0.01$

## ANALYSIS IV

Preceding analyses have shown that the location of a stimulus-word in semantic space is often proximal to that of its primary associate. Each stimulus-word, however, evokes more than just a single associate, and it is reasonable to expect that all of the associates evoke by the same stimulus-word will tend to have similar semantic locations. The strong correlation between the semantic differential rating of a stimulus-word, and the mean semantic differential rating of that word's first 20 associates reported by Staats and Staats, tends to support the hypothesis of semantic congruence between a stimulus-word and most of the words contained in its own associative hierarchy.[15]

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[15] Staats and Statts, *op. cit.,* 140.

The purpose of this fourth analysis is to determine if there are systematic differences in intra-hierarchy semantic distance as a function of the initial semantic location of the stimulus-word. That is, do highly polarized pleasant, active, or hard stimulus-words evoke hierarchies which are less widely scattered in the semantic space than is true for either neutral, or highly polarized unpleasant, passive, or soft stimulus-words?

*Procedure.* Jenkins, Russell and Suci have provided a table of interword semantic distances for all 360 words contained in their earlier *Semantic Atlas.*[16] Sixty-eight of these 360 words are also initial stimulus-words in the Kent-Rosanoff list. A number of the remaining 292 entries are associative hierarchy-responses evoked by many of the 68 stimulus-words. Using these data it is possible to determine the mean intra-hierarchy distance between each stimulus-word and all of its available associative responses (available in that they are contained in the *Semantic Atlas*) in the hope of determining a consistent relationship between the semantic location of an initial stimulus-word and the scatter of words contained in its associative hierarchy.

In order to provide data for this analysis, mean intra-hierarchy distances (MID) were separately computed for each of the 68 stimulus-words, such that for any one word, $MID = \Sigma d_i/n_i$, where each $d_i$ is the semantic distance between stimulus-word i and each of its available associates. After MIDs were computed for all 68 stimulus-words, these words were sorted in a number of different ways. Each word was first sorted on the basis of its evaluative semantic differential rating into one of the following six score intervals: 1.00-1.99, 2.00-2.99, 3.00-3.99, 4.00-4.99, 5.00-5.99, 6.00-6.99. According to the logic of the semantic differential rating procedure, the first three categories are connotatively positive, while the last three are connotatively negative.

Words were also similarly sorted on both the Potency and Activity dimensions. After these sortings were completed for each semantic dimension separately, a mean MID value was computed for each semantic differential interval.

*Results.* Fig. 1 presents the progression of the intra-hierarchy distance means as a function of the evaluative loading of the initial stimulus-word. As can be seen, mean intra-hierarchy distances are considerably larger for hierarchies evoked by negative words than for hierarchies evoked by positive words. A simple analysis of variance, involving six independent groups, yielded an *F* of 6.024 (5/62 df; $p < 0.001$), indicating that mean MID scores differed significantly as a function of the evaluative loading of the stimulus-word. What this means is that associative hierarchies evoked by negatively evaluated stimulus-words contain responses which are semantically more distant from their evoking stimulus-word than is true for responses contained in associative hierarchies evoked by positively evaluated words. Similar analyses, performed on these stimulus-words sorted in terms of their locations on the Potency and Activity dimensions, failed to demonstrate a systematic or significant effect of initial location on intra-hierarchy distance.

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[16] J. J. Jenkins, W. A. Russell, and G. J. Suci, *Appendix for Table of Distances for The Semantic Atlas.* University of Minnesota Library, 1959.

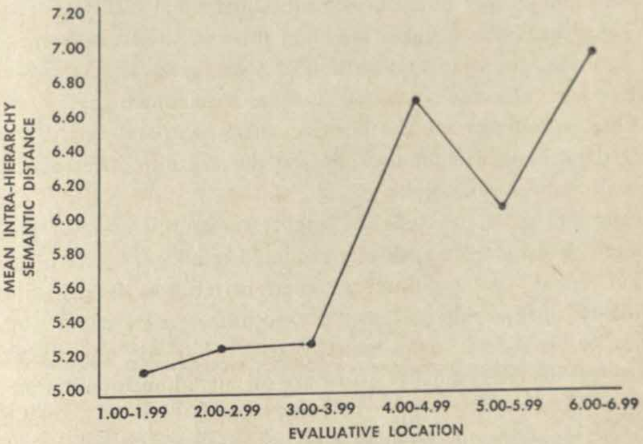

FIG. 1. MEAN INTRA-HIERARCHY SEMANTIC DISTANCE AS A FUNCTION OF THE EVALUATIVE VALUE OF THE INITIAL STIMULUS-WORD

Under the assumption that the speed with which one word produces another word as an associative response is some inverse function of the semantic distance between these words, the results of the present analyses lead us to expect systematic differences in the evocation latencies of associates to positive and to negative stimulus-words. More specifically, the evocation latency of an associate to a positive stimulus-word should be shorter than the evocation latency of an associate to a negative stimulus-word because mean intra-hierarchy distances are less for associates evoked by positively evaluated stimulus-words than for those evoked by negatively evaluated stimulus-words. In support of this hypothesis. Tolman and Johnson long ago demonstrated that the associative RT for pleasant stimulus-words is less than the associative RT for negative words.[17] Positive stimulus-words produce associative responses more quickly simply because the average distance between a positive stimulus-word and its associative responses is less than the average distance between a negative stimulus-word and its associative responses. Results which demonstrate a significant correlation between the number of associations produced in a constant time-period and the evaluative rating of the stimulus-word may also be interpreted in terms of the smaller average inter-word distance in hierarchies evoked by positive stimulus words.[18]

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[17] E. C. Tolman and Isabelle Johnson, A note on association time and feeling, this JOURNAL, 29, 1918, 187-195.

## SUMMARY

A recent compilation of mean semantic differential ratings for a number of Kent-Rosanoff stimulus-words and many of their respective associative responses permits an examination of some semantic relations among word-associates. The results of four analyses have shown that:

1) There is a significant and positive correlation between the semantic differential rating of a stimulus-word and the semantic differential rating of that word's primary associate.

2) Correlations of this type are higher for word-associates produced by children than for word-associates produced by adults.

3) For adults, the magnitude of these correlations is related to the communality of the primary associate, such that correlations for high communality word-pairs are essentially zero, while the comparable correlations for low communality pairs are all significantly different from zero.

4) The magnitude of the intra-hierarchy distance of associative hierarchies evoked by negatively evaluated stimulus-words is greater than the intra-hierarchy distance of associative hierarchies evoked by positively evaluated stimulus-words.

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[18] Frank Koen, Polarization, *m* and emotionality in words, *J. verb. Learn. and verb. Beh.*, 1, 1962, 183-187.

# TWO TYPES OF ADAPTATION TO AN OPTICALLY-ROTATED VISUAL FIELD

By HARUTUNE MIKAELIAN and RICHARD HELD, Brandeis University

Experimental findings imply that there are at least two clearly distinguishable processes that can produce after-effects of tilt in vision. On the one hand, Gibson has found these after-effects following the inspection of lines tilted from the vertical or horizontal.[1] He postulated that the critical condition for generating them was the deviation of the inspection-line from the vertical or the horizontal, which constitute norms.[2] On the other hand, a different technique of generating after-effects of tilt leads us to expect them following exposure to visual fields whose patterning neither deviates from a norm nor is atypical in any significant manner.

Held and Bossom measured compensatory shifts in visual direction-finding following the wearing of wedge-prisms that displaced the visual field.[3] They reported full and exact compensation after *S* walked about while wearing the wedge-prisms, but, when *S* was moved passively while seated in a wheelchair, no corrective shift was found. The two types of exposure of the prism-wearer (Active and Passive) provided comparable variations in visual stimulation, but these variations were distinguished by their dependence upon, or independence of, self-produced locomotion. Stimulation produced as a consequence of active motion (reafferent feedback) was, therefore, claimed to be the critical factor in producing compensation for the errors initially induced by the prism. Errors of equal magnitude but opposite direction were found upon removal of the prisms.

The changes of visual direction-finding measured by Held and Bossom represented compensation for a gross lateral displacement of the visual field. We expected that the same principle of adaptation would result in compensation for differential lateral displacements within the visual field. Right-angled prisms were used to effect such a rearrangement by rotating

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[1] J. J. Gibson and Minnie Radner, Adaptation, after-effect and contrast in the perception of tilted lines: I. Quantitative studies, *J. exp. Psychol.*, 20, 1937, 453-467; J. J. Gibson, II. Simultaneous contrast and the areal restriction of the after-effect, *ibid.*, 553-569.

[2] Gibson, Adaptation with negative after-effect, *Psychol. Rev.*, 44, 1937, 242.

[3] Richard Held and Joseph Bossom, Neonatal deprivation and adult rearrangement: Complementary techniques for analyzing plastic sensory-motor coordinations, *J. comp. physiol. Psychol.*, 54, 1961, 33-37.

the visual field. The induced lateral displacements were then proportional to height in the field of view. When a vertical line was viewed, the result was an induced tilt. Full and exact compensation for the differential displacements and the equivalent after-effects of tilt should then require self-produced movement in the visible environment, unlike Gibson's normalization which requires only prolonged inspection of a tilted pattern.

Comparison of the two procedures for producing these after-effects suggested that they could be distinguished on the basis of three criteria. The prism-generated after-effects were expected (1) to reach, after sufficient exposure to a normal environment, a magnitude equivalent to the rotation of the visual field produced by the prisms (corresponding to full and exact compensation for the errors induced by the prisms), (2) to require self-produced movement of the wearer, and (3) to be evidenced not only by an apparent tilting of a line near vertical but also by differential shifts in the egocentric localization of single points within the visual field. By contrast, available information on Gibson's after-effect suggested that it should not exceed 2-3°, that it should not require self-produced movement, and that it should occur without corresponding shifts in egocentric localization. The following experiments were designed to test for the expected differences.

## EXPERIMENT I

Subjects wearing tilting prisms were exposed in an ordinary indoor environment under both an active and a passive condition of movement approximately equated for total visual stimulation. They were measured before and after exposure on both egocentric localization and the setting of a line to verticality.

*Apparatus.* The apparatus is diagrammed in Fig. 1. It was designed to measure both changes in the setting of a line to the vertical and shifts in the egocentric localization of single points. *S* sat in the chair which he could rotate by leg movements around axis *b-b*. The *L*-shaped beam hung from the ceiling could be rotated by *E* around axis *b-b* independently of the chair. Targets were attached to the beam 72 cm. distant horizontally from the eye of *S* whose head was fixed by a biteboard, at *A*, rigidly connected with the chair. The line-target for measuring tilt was a luminous slit (12 cm. long and 0.3 cm. wide) that could be rotated by *S* in a frontal-parallel plane around axis *a-a*. In taking measurements, the slit was turned on and *S* was instructed to rotate it, by remote control, until it appeared vertical. Readings of the tilt of the line were then taken from an indicator connected to the slit by selsyn motors. For measuring egocentric localizations, two point-sources of light ($B_1$ and $B_2$) 12 cm. apart were mounted on the vertical portion of the beam facing inward in positions corresponding to the ends of the line-target when in vertical alignment. When taking measurements, *E* positioned the *L*-shaped beam in one of a set of pre-determined directions from *S*, turned on one of the point-sources, and then instructed *S* to rotate himself until the target appeared straight ahead of him.

The orientation of *S* with respect to the target was then read off as the distance in degrees between pointers *p* and *p'* attached to the chair and beam respectively. The same operations were carried out for the second point-source. *S* saw only one light-source at a time. The tilt of the imaginary line connecting the apparent locations of the two point-sources was computed from the mean orientations of *S* to the two points. All determinations were made in complete darkness except for the dimly-luminous targets. Each *S* used his right eye only (left eye occluded), with goggles removed. The targets were presented within that region of the visual field that was viewed through the aperture of the prism-goggles worn by *S* during exposure. The procedures of measuring, before and after exposure, with either naked eye or with goggles in place, yield identical changes in tilt when testing is carried out with no other change in conditions; however, measuring with naked eye makes the procedure analogous to that used in the study of after-effects. Consequently, we will refer to the effects of exposure as after-effects. Positive shifts in the measurements represent compensation for the error induced by the prisms.

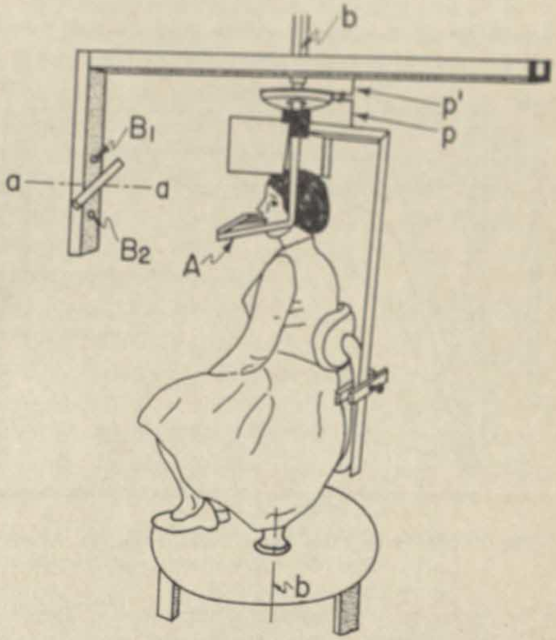

FIG. 1. APPARATUS FOR MEASURING THE SETTINGS OF A LINE TO THE APPARENT VERTICAL AND THE ORIENTATIONS OF THE BODY TO SINGLE POINTS (EGOCENTRIC LOCALIZATION)

*Short-term exposure.* During exposure, *S* viewed the environment with his right eye (left eye occluded) through a goggle-frame on which was mounted a set of two right-angle prisms in tandem. They rotated the visual field by 20° counterclockwise from *S*'s viewpoint and, in addition, displaced its center 4° left and 3° up in visual angle. The prisms limited the field of view to 15° of visual angle. The *S*s were 12 undergraduates with normal vision. Each *S* served in two sessions. In one session, under the Active Motion condition, *S* walked up and down a hallway for 1 hr., viewing his surroundings through the goggles. Under the Passive Motion condition, *S* was wheeled in a wheel-chair over the same path while viewing the same surroundings through the goggles for an equal period of time. Six *S*s had the Active session first, and six the Passive session first. Six measurements on the line and 24 on the points were taken before and after exposure. The order in which the two sets of measurements were taken was permuted among *S*s.

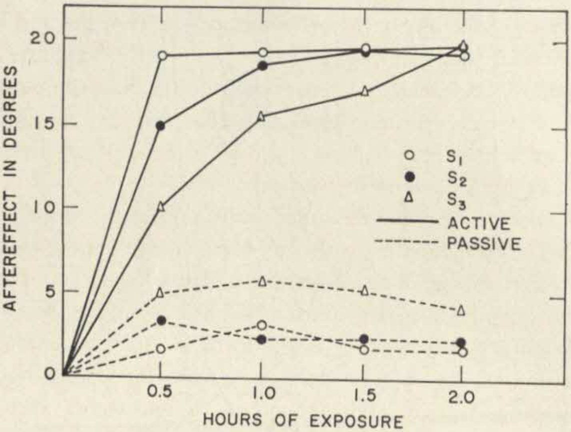

FIG. 2. RESULTS OF LONG-TERM EXPOSURE UNDER ACTIVE AND PASSIVE CONDITIONS

*Long-term exposure.* From a large group of *S*s studied in this and in similar experiments, the three who showed the most rapid rates of adaptation were selected. The conditions of exposure were exactly as described above, except that length of exposure was increased to 2 hr. and measurements were taken only on the line-target every 30 min.

*Results.* After Active exposure, the mean shift on the line for all *S*s was 6.8°. This shift was significant beyond the 1% level according to a *t*-test of the difference of this mean from zero. The corresponding shift on the points was 7.0° ($p < 0.05$). After Passive exposure, the shift on the line was 1.9° ($p < 0.01$), while that for the points was −0.8° ($p > 0.05$). The difference of 4.9° between the mean shifts measured on the line after Active and Passive exposures was significant at the 1% level.

The effects of prolonged exposure on the fast-adapting *S*s are shown in Fig. 2. All three *S*s reached full compensation, within the limits of experimental error, when allowed active movement. The curves for two of these *S*s show that compensation reached but did not exceed the exact displacement produced by the prisms. The mean shift for the active condition was 19.6°; that for the passive condition was 2.7°.

*Discussion*. Both conditions of exposure, active locomotion while wearing the tilting prisms and passive motion in a wheelchair, are effective in producing a change in settings of a line to vertical, which indicates that some adaptation occurs under both conditions. The significantly larger after-effects following active exposure clearly indicate however, the greater effectiveness of self-produced movement in bringing about adaptation to the tilted visual field under these conditions. In fact, the results of the long-term exposure indicate that about 2 hr. of active exposure are sufficient to generate, for rapidly adapting *S*s, full and exact compensation for the 20° tilt. Passive exposure yields after-effects of a magnitude roughly equal to those reported by Gibson following the mere inspection, for a few minutes, of lines tilted off the vertical.[4]

Measurements after active exposure indicate that the positions of the two points were differentially displaced by an amount approximately equal to the after-effect measured by adjustments of the line-target. The absence of a significant shift following passive exposure suggests that the after-effect shown on the line-target is a species of the pattern-generated after-effect.[5] Continuous scanning of special patterns induces changes in the perceived shape and orientation of contours. Single points, such as those that were egocentrically localized in this experiment, do not appear to be subject to the after-effects of exposure to such patterns.

These findings suggest that two types of adaptation to an optically-rotated visual field may be distinguished on the basis of the three criteria discussed in the introduction. It remains to be demonstrated that the after-effect of tilt obtained under our passive condition was of the Gibson type, an interpretation tested in the next experiment.

## EXPERIMENT II

The *S*s of Experiment I were exposed in a long hallway which, like most products of our architecture, contained many more vertical and horizontal lines than lines in any other orientation. Viewing this environment through tilting prisms was equivalent in effect, we suspected, to the inspection of a pattern of lines tilted off the vertical and horizontal—a condition which generates Gibson-type negative after-effects. We conjectured that elimination of the grid-like patterning from the visual field would eliminate normalization but would not eliminate the adaptation that follows active movement. To test this conjecture *S*s were exposed to a figured environment in which grid-like patterns were eliminated.

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[4] Gibson and Radner, *op. cit.*, 453-467; Gibson, *op. cit.*, II. 533-569.

[5] Richard Held, Adaptation to rearrangement and visual-spatial after-effects, *Psychol. Beiträge*, 6, 1962, 439-450.

*Method.* In this experiment, *S* remained stationary while controlling the motion of the point-targets. His head was fixed with a biteboard. The two point-sources of light were 120 cm. from *S*'s eye in a frontal-parallel plane. They were mounted on tracks and continually moved either to *S*'s left or right at a speed of 3 cm./sec. Only one point was visible at a time. *S* could reverse the direction of motion of the point with a switch held in his hand, but he could neither stop it nor change its speed. He was instructed to keep the point straight ahead of him by reversing its direction of motion when he judged that it had just passed the position straight ahead of him. Each target light was turned on for 20 sec., and these periods were interspersed with 20-sec. periods of rest during which *S* had his eyes closed. The movements of the target were recorded on a chart indicating the positions and timing of *S*'s switching responses. In practice, this method of egocentric localization gave measurements comparable to, but more precise than, those of Experiment I. In addition, changes of the setting of a line-target were measured by having *S* adjust a 12 cm. luminous line, 120 cm. distant from his eye, to the vertical. All testing was done in a dark room.

Each *S* was exposed in an Active and in a Passive session to a special environment—a room on whose walls 99 dimly-luminous styrofoam spheres, 15.2 cm. in diameter, were hung at random. They provided the only sources of illumination in the room, which meant that *S* could see only a set of luminous circles from any viewpoint he was allowed to take. The density of contours in the field of view was then the same for all meridional sectors in his frontal plane. Obviously, a rotation of the field of view around *S*'s line of sight effected no significant change in the patterning of his field. In the Active-motion condition, *S*, wearing the tilting prisms, walked back and forth in the room while viewing the display. In the Passive-motion condition, *S*, viewing the display through the prisms, was moved over a similar path in the same environment for an equal period of time while standing on a specially-built dolly. Eight *S*s were exposed for 90 min. Measurements were taken before and after exposure on both points and line. Two fast-adapting *S*s were exposed for 2.5 hr. and measurements were taken solely on the line.

*Results.* After Active exposure, the mean shift on the line for the 8 *S*s was 2.1° ($p < 0.01$). The mean shift on the points was 1.8° ($p < 0.01$). After Passive exposure, the mean shifts on both line and points were about zero. The shifts of the two fast-adapting *S*s were 5.4° and 4.7°.

In the absence of an exposure-field with its dominant rectangularity, no significant after-affect of tilt was found after passive motion. These results are clearly consistent with the conclusion that the adaptation found in Experiment I after passive movement was of Gibson's type. The results for the Active condition indicate that it is possible to obtain after-effects of tilt following exposure, with tilting prisms, to a visible environment whose patterning merely serves as an agent in the production of reafferent stimulation contingent upon self-produced movement. The amount of adaptation achieved under this condition was smaller than that from equivalent exposure to a more normal environment. We believe that this difference can be accounted for by the far greater density of contours in the natural environment.

These results validate the three criteria said in the introduction to distinguish between the after-effects generated as a consequence of wearing prisms in a normal environment and those generated under the conditions studied by Gibson. A number of studies have indicated, however, that Gibson's original findings result, in turn, from a variety of separable conditions.[6] Experimental work is making it increasingly clear that spatial after-effects are not the simple phenomena they have at times been thought to be. The procedures used in this experiment are an example of methods that may be used to tease apart different conditions that generate after-effects. Whether a new synthetic approach is possible remains to be seen. The scope and implications of the reafferentation model have been discussed elsewhere.[7]

## SUMMARY

The after-effects of viewing two different environments through a set of prisms that rotated the retinal image by 20° were investigated. In Experiment I, the environment was a long hallway. Large changes in the settings of a line to the vertical and equivalent shifts in the egocentric localization of two separate points were found to be contingent upon the active movement of *S* while viewing the environment through the prisms (Active exposure). Full and exact compensation for the 20°-tilt was reported for some *S*s after prolonged exposure. *S*s viewing the same environment through the prisms while being moved over the same path seated in a wheel-chair (Passive exposure) showed a small but significant shift in the settings of the line but no shift on the points. In Experiment II, the exposure-environment was designed to eliminate all grid-like patterns. Active exposure with the tilting prisms again resulted in both after-effects of the prism rearrangement. Passive exposure, however, gave no shifts whatsoever. These results clearly differentiate two processes of adaptation.

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[6] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. phil. Soc.*, 88, 1944, 269-357; R. B. Morant and H. H. Mikaelian, Inter-field tilt after-effects, *Percept. Mot. Skills*, 10, 1960, 95-98; Held, Localized normalization of tilted lines this JOURNAL, 76, 1963, 146-148; H. K. Beller, Adaptation to atypical visual environments: An inquiry, Unpublished Master's thesis, Brandeis University, 1962.

[7] Held, Exposure-history as a factor in maintaining stability of perception and coördination, *J. nerv. ment. Dis.*, 132, 1961, 26-32; A. V. Hein and Richard Held, A neural model for labile sensorimotor coordination, in E. E. Bernard and M. R. Kare (eds.), *Biological Prototypes and Synthetic Systems*, vol. 1, 1962, 71-74; Richard Held and S. J. Freedman, Plasticity in human sensorimotor control, *Science*, 142.

# SPATIO-TEMPORAL RELATIONS IN THE AFTER-EFFECTS OF AN *I*-FIGURE ON THE CRITICAL FLICKER-FREQUENCY

By IRWIN M. SPIGEL, Temple University

In a previous investigation designed to explore the effects of satiation on foveal critical flicker-frequency (*CFF*), Spigel and Hamilton found that pre-fixation on a black inspection-figure (*I*-figure) resulted in a significant increase in the threshold of fusion.[1] From the assumptions of the theory of satiation, as formulated by Köhler and Wallach,[2] it had been expected that the *CFF* would have been decreased following figural fixation. According to this position, such pre-inspection leads to increased cortical resistance to further stimulation in the involved cerebral area and a consequent reduction in the apparent size and luminosity of new targets projected to these loci. A decreased *CFF* might also be expected to occur in view of Bevan's determination that thresholds of brightness are increased within areas of previously fixated *I*-figures.[3]

It was suggested that the unexpected increment in *CFF* might be related to a negative after-image and consequent increase in the phenomenal luminance of the surround and target from the 30-sec. fixation on the *I*-figure. Two questions are raised by the findings. Although the enhanced thresholds of fusion may be associated with phenomenal increments in the brightness of either the surround or the target, it is also possible that such an effect is related to but one of these two factors. A second question arises from an interpretation of the results as indicating that the neural correlates of the negative after-image predominate over the activity in the striate cortex, whether or not this is of the kind posited by the theory of satiation. If the two influences are regarded as vectors, it is possible that a neutral point may be determined with the size of the *I*-figure on one hand and time of inspection (satiation) on the other. That is, a reciprocal relationship may exist whereby a sufficiently small *I*-figure, fixated for a sufficiently long interval, yields no change in *CFF*—with

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* Received for publication May 31, 1962.

[1] I. M. Spigel and H. C. Hamilton, A pre-exposure vector in *CFF* surround brightness, *Percept. Mot. Skills,* 15, 1962, 142.

[2] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. phil. Soc.,* 88, 1944, 269-357.

[3] William Bevan, The influence of figural after-effects on visual intensity thresholds, *J. gen. Psychol.,* 45, 1951, 189-207.

smaller *I*-figures or longer periods of fixation yielding decreased thresholds of fusion. The current investigation was designed to answer both these questions, in which the size of the *I*-figure and time of inspection were critical parameters.

### METHOD

*Apparatus.* The flickering luminance was produced by the electronic switching unit of the Lafayette Model 1202A Fusion Apparatus. The square-wave output was fed to a neon glow-lamp mounted in a specially constructed assembly. This consisted of a table-type relay-rack, the front of which was covered with a white panel with a 2⅜-in. square cut-out midway in the vertical dimension and on the right side. The white facing was set back ½ in. within the cut-out area and contained a ½-in. diameter circular aperture. A frosted NE 40 lamp was mounted flush against this aperture from the rear. Four flanged disks were so prepared that they could be rotated freely through the ½-in. space separating the front panel of the display from the set-back portion within the cut-out area which contained the flicker-aperture. The flanged portions of each of the disks were painted a flat black and only these sections were visible in the cut-out area. The flanges were cut to lengths which, when rotated by a 1 r.p.m. synchronous motor, were visible for 2, 4, 8 and 16 sec., respectively, during each revolution. The white panel was also flanged beneath the cut-out area so that a white mask could be supported. Three such masks had been prepared, containing circular apertures of 0.5 in., 1 in. and 2 in. in diameter, respectively. Thus, combinations of mask and flanged disk could provide one of 12 pairs of size of circular figure and time of exposure. The mask with the aperture ½ in. in diameter allowed only the actual target-area of the flickering light to be occluded by the flanged portion of the disk during rotation, while the other two masks permitted the flickering target and surrounds of either 1 in. or 2 in. in diameter to become occluded by the black sectors of the disks. The net result permitted exposure of *O*s to one of three circular *I*-figure sizes: 0.5 in., 1 in., or 2 in.; and one of four intervals of pre-fixation: 2, 4, 8, or 16 sec. The flicker-aperture remained constant at a diameter of 0.5 in.

*Observers.* Thirty-six women students in a general nursing program volunteered for the experiment. All were naïve as to the purpose of the investigation.

*Procedure.* The *O*s were assigned at random to one of 12 groups—each group representing a combination of size of *I*-figure and interval of pre-fixation in the 3 x 4 factorial design. There were three *O*s in each group. Two series of trials, with six presentations in each, were administered during each *O*'s single appearance at the laboratory. The first series of presentations consisted simply in the determination of the binocular foveal *CFF* without pre-exposure to an *I*-figure. Six trials, alternately ascending and descending, were presented, and the last four of these used in the computation of a mean threshold of fusion for each *O*. The *O*s were seated at a distance allowing the flicker-aperture to subtend an angle of 26′ at the eye. Constant ambient illumination of 83 mL was maintained. Following the preliminary determinations of *CFF*, the *O*s were given instructions relevant to the pre-fixation series. They were informed that each exposure to the flickering light-source would be preceded by an interval during which they were to fixate on the center of a black circle which would appear in the aperture. The mask and flanged disk which would provide the appropriate size-time combination for *O* had been pre-set. Post-fixation presentations of *CFF* occurred immediately after each complete passage of the flanged portion of the disk across the mask-aperture. There was a 60-sec. interval between trials. As before, six alternately ascending and descending post-fixation trials were run, the last four being used for the computation of a mean *CFF* for each *O*.

## RESULTS

A negatively accelerated relationship was evident when post-fixation thresholds of fusion and the differences between these and pre-inspection

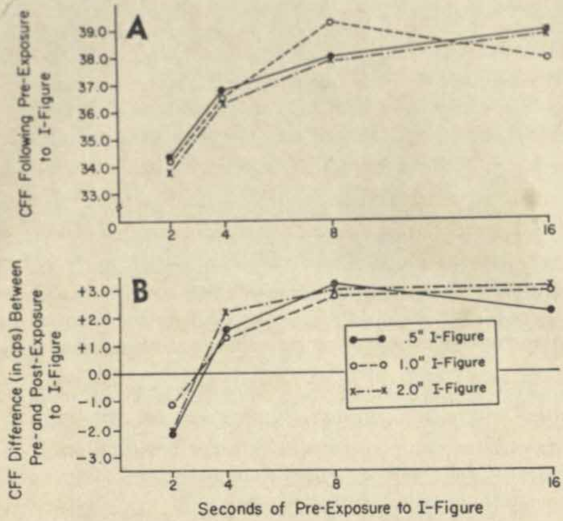

FIG. 1. CHANGE IN *CFF* FOLLOWING PRE-INSPECTION OF *I*-FIGURES

The connected points in (A) show the relationship of mean post-fixation *CFF* to duration of inspection of the *I*-figure for each of the three diameters of *I*-figure. In (B), the differences in *CFF* between preliminary and post-inspection determinations for each diameter of the *I*-figure are plotted on the same abscissa. Points represent the mean for three *O*s.

*CFF*s were plotted against duration of fixation on the *I*-figure (Fig. 1). Post-fixation thresholds of fusion for the *O*s given varying durations of pre-inspection were significantly different ($F = 10.97$, $df = 3, 24$, $p < 0.001$). Virtually identical results were obtained for differences between initial determinations of the *CFF* and the post-fixation thresholds ($F = 58.94$, $df = 3, 24$, $p < 0.001$). For the post-fixation measures, differences between thresholds of fusion for the 2- and 8-sec. conditions were all statistically significant, as were those between the 2- and 16-sec. conditions. For the difference-measures between initial and post-fixation *CFF*s, significant differences were obtained between all 2-, 4-, and 8-sec. durations, with the exception of the 2-in. *I*-figure in the 4- and 8-sec. conditions. Differences between the 2-, 4- and 16-sec. durations were also statistically significant, except for the one case noted above.

Neither the post-fixation *CFF*s, nor the differences between initial and post-inspection thresholds for size of *I*-figure and size-time interaction yielded statistically significant differences.

The equality of the groups with respect to initial *CFF* was confirmed by the non-significant *F*-ratios obtained for diameter of *I*-figure and fixation-time assignments, and for interaction.

## DISCUSSION AND CONCLUSIONS

The current investigation was designed to explore further a previously observed but unexpected increase in *CFF* following pre-fixation on a black *I*-figure. It is apparent from the data that the diameter of the *I*-figure and the subtended area of the surround are of little consequence in the direction of change in the threshold of fusion as a result of pre-fixation. The observed decrease or increase in the threshold of fusion at all of the pre-fixation times are of the same order for each of the three sizes of *I*-figure employed. Events within the area subtended by the flickering luminance thus appeared prepotent over involvement of the surround in all cases. Reciprocity of the area of *I*-figure and duration of pre-fixation failed to emerge.

The most compelling result was the critical nature of the temporal factor in fixation of *I*-figure. A reduction in *CFF* was observed for all three diameters of *I*-figure with 2 sec. of pre-inspection, while increments in the threshold of fusion were observed for all of the longer periods of pre-fixation. It would seem that the effects of cortical satiation, as theoretically implied, yield to more pronounced influences of a diametrically opposed nature at all but the shortest duration of pre-fixation. Whether the processes underlying the enhanced *CFF*s are those which subserve after-images and increments in phenomenal luminance remains to be determined.

It may be noted that the absence of a decrement in *CFF* with increased time of pre-fixation is consistent with the findings of Oyama and Ikeda and Obonai who determined that lengthening the period of *I*-figure fixation produced no greater figural after-effect if measurements were taken immediately.[4] The work of Jung and his associates with the cat provides a basis for the implication of complex cortical mechanisms in the facilitative effects of pre-inspection of an *I*-figure on *CFF*.[5] Recording from neurons in the visual cortex, fibers responding to either light-on or light-off stimulation were isolated. Reciprocal inhibition of antagonistic neurons in the same receptive field and lateral inhibition of synergistic neurons in neighboring regions were also demonstrated. Of significance for the current observations was the determination that these fibers undergo a reversal of their roles when presented with patterned stimulation. That is, the normally light-on fibers fire during stimulation by the dark portion of a dark-light pattern. If it is assumed that *CFF* is a more or less direct function of the ability of the central nervous system to transmit off-information, the effects of pre-fixation on a dark field may manifest themselves by way of such reciprocal inhibitory activity. It is possible, that is, that normally light-off fibers are inhibited during pre-fixation of the dark *I*-figure, leaving only the normally light-on fibers firing. With reëxposure to the flickering stimulus, the light-off fibers are released from inhibition and maximally sensitive while the light-on fibers, rendered refractory to varying degrees by the sustained dark fixation, require time to recover full firing potential. It is, perhaps, during this interval that the *CFF* shows the post-fixation increment.

## SUMMARY

An experiment was performed to determine whether the events underlying an enhanced *CFF* following pre-fixation of an *I*-figure were restricted to the area subtended by the flickering light, the surround, or both. A second purpose was to determine whether pre-inspection time and the area of the *I*-figure were reciprocally related in their effects on the direction of change in the post-fixation threshold of fusion. A factorial design, with three sizes of *I*-figure, 0.5 in., 1 in. and 2 in. in diameter, and four durations of pre-exposure, was employed with 36 women *O*s. It was determined that duration of pre-inspection alone was the significant factor underlying the change in *CFF,* and that a negatively accelerated increment occurred with all *I*-figures at 4-, 8- and 16-sec. pre-exposure times. At 2

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[4] Tadasu Oyama, Experimental studies of figural after-effects: (1) Temporal factors, *Jap. J. Psychol.,* 23, 1953, 239-245; H. Ikeda, and T. Obonai, The quantitative analysis of figural after-effects: II. On "self-satiation," *ibid.,* 24, 1953, 179-192.

[5] R. Jung, Neuronal integration in the visual cortex and its significance for visual information, in W. Rosenblith (ed.), *Sensory Communication,* 1961, 627-674.

sec. of pre-fixation, decreased *CFF* was obtained with the three *I*-figures. Since there was no significant difference between *CFF*s with the three *I*-figures, and the 0.5-in. figure subtended the same visual angle as the flickering luminance, it was concluded that events within the area subtended by the latter were critical in the observed *CFF* changes. Although it had been inferred from satiational theory that reduction of *CFF* would follow from pre-fixation of an *I*-figure, while events productive of a negative after-image might mediate an opposite effect, reciprocity of pre-inspection time and area of the *I*-figure in their effects on the threshold of fusion failed to emerge in the data.

# EFFECTS OF PROLONGED DELIBERATION ON PSYCHOPHYSICAL JUDGMENT

By HERBERT BARRY, III, University of Pittsburgh

In psychophysical experiments, the *O*s generally are instructed to make a single rapid judgment of each stimulus; deliberation is minimized. In the present study, the *O*s were instructed to make an immediate judgment of each stimulus and then to give careful consideration to this judgment, revising it if they found reason to do so. Prolonged deliberation therefore may be compared with brief deliberation by comparing initial and revised judgments.

Should prolonged deliberation be expected to impair or to improve accuracy? Opposite predictions might be made. The passage of time during prolonged deliberation might impair accuracy; but prolonged deliberation implies an increase in attentiveness and mental activity, which should improve accuracy.

Accuracy is not, of course, the whole story; for example, equally incorrect judgments of weight may be overestimations or underestimations. Opposite predictions might be made about the effect of prolonged deliberation on judged heaviness. The passage of time during prolonged deliberation should tend to produce underestimation (negative time-error),[1] but prolonged deliberation implies increased attentiveness to the weight, and other conditions which enhance attentiveness have been shown to increase the heaviness of weight-judgments,[2] and to increase the judged magnitude of stimuli in other modalities.[3]

Weights are judged in relation to a subjective neutral point or adaptation-level, which is close to the median of the series of weights being judged.[4] During the passage of time, the memory-trace of each weight does

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* Received for publication September 4, 1962. The experiment was done at Yale University; the author is indebted to Professor L. H. Beck for advice.

[1] C. C. Pratt, The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 292-297.

[2] Edmund Jacobson, Experiments on the inhibition of sensations, *Psychol. Rev.*, 18, 1911, 24-53; Kinya Maruyama, The effect of tone on the successive comparison of lifted weight, *Tohoku Psychol. Folia*, 15, 1956, 31-48.

[3] Jacobson, *op. cit.*, 24-53; S. M. Newhall, Effects of attention on the intensity of cutaneous pressure and on visual brightness, *Arch. Psychol.*, 9, 1923, (No. 61), 5-75.

[4] Harry Helson, Adaptation-level as frame of reference for prediction of psychophysical data, this JOURNAL, 60, 1947, 1-29.

not fade to nothingness but instead becomes assimiliated toward the *AL*. Since the *AL* generally is lighter than the weight being judged, assimilation generally signifies a change in the direction of lighter, which results in the negative time-error. When, however, the weight is lighter than the ones to which *O* has become accustomed, assimilation signifies a change in the direction of heavier; this is the circumstance under which a positive time-error has been found.[5] Contrary to the effect of a passage of time, an increase in attentiveness to a weight causes the judgment to become contrasted with the *AL*.[6] Opposite predictions might be made, therefore, about changes in judgment after prolonged deliberation, depending on whether passage of time or increase in attentiveness is the predominant effect.

*Method: (1) Observers.* There were 24 *O*s, 12 in Experiment I and 12 in Experiment II. They were young adult volunteers, with no prior knowledge of the purpose of the experiments.

*Experiment I.* The stimuli were 7 standard weights, of 300, 350, 400, 450, 500, 550, and 600 gm. *E* and *O* sat at opposite sides of a table. As soon as each weight was presented to *O* by being placed on a towel on the table in front of him, *O* lifted the weight with one hand and gave an immediate judgment of heaviness on a 1-7 scale. *O* then gave 'careful consideration' to his immediate judgment, improving it if he thought it was wrong, or confirming it by saying "no change." *E* thereupon said "correct" if the deliberated judgment was correct, or gave the correct scale-number of the weight. The purpose of the information was to enhance *O*'s motivation for prolonged deliberation after the immediate judgment. The successive weights were presented at intervals of approximately 12 sec. If *O* said 'no change' within 3 sec. after the immediate judgment, *E* delayed for 5 sec. before removing the weight to prevent rapid statements of 'no change' from speeding up the stimulus-presentations.

The effects of prolonged deliberation were measured under two conditions: *stimulus absent* and *stimulus present*. In the stimulus-absent condition, *O* was instructed to put the weight down without delay after making the immediate judgment; his decision as to whether to revise the judgment was made after the weight had been put down. In the stimulus-present condition, *O* was instructed to hold the weight steady until after revising the original judgment or confirming it.

The experiment began with a practice-sequence of 18 weight-presentations, followed by 4 test-sequences of 36 presentations; each test-sequence was preceded by a rest-period of a few minutes. The order of weights was the same in each sequence, with the median weight being presented 6 times and the others 5 times each. Half the *O*s had the stimulus-absent condition in the practice, first, and third sequences, and the stimulus-present condition in the second and fourth sequences; for the other half of the *O*s, the opposite order of conditions was used.

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[5] Herbert Woodrow, Weight-discrimination with a varying standard, this JOURNAL, 45, 1933, 391-416.

[6] P. S. Holzman, The relation of assimilation tendencies in visual, auditory, and kinesthetic time-error to cognitive attitudes of leveling and sharpening, *J. Personal.*, 22, 1954, 375-394; R. W. Gardner and L. J. Lohrenz, Attention and assimilation, this JOURNAL, 74, 1961, 607-611.

*Experiment II.* The procedures were the same as in Experiment I, unless specified as different in the following account. The stimuli were 10 standard weights, divided into a *light* and a *heavy* set, with 5 weights in each. The weights were 200, 250, 300, 350, and 400 gm. in the light set, and 500, 550, 600, 650, and 700 gm. in the heavy set. Judgments were made on a 1-5 scale. The stimulus-absent condition of Experiment I was used throughout.

The experiment began with a practice sequence of 25 presentations, followed by 3 test-sequences of 50 presentations. Each weight was presented 10 times. Half the *O*s were given the light set in the practice, first, and third sequences, and the heavy set in the second sequence; for the other half of the *O*s, the opposite order was used. The data for the last two sequences represent the judgments of each set of weights preceded by judgments of the other set.

## RESULTS

The effect of prolonged deliberation on accuracy of judgments may be indexed by the number of judgments changed in the direction of greater accuracy as compared with the number changed in the direction of lesser accuracy. The relevant data are given in the last column of Table I, separately for the two experiments and for the two conditions of each experiment. In this and the following tables, the numbers shown are means, and statistical significance was calculated by *t*-tests (11 df.). Under all of the experimental conditions, the majority of changes were in the direction of greater accuracy.

TABLE I

THE NUMBER OF IMMEDIATE JUDGMENTS INCORRECT AND CORRECT, THE NUMBER OF THESE CHANGED AFTER PROLONGED DELIBERATION AND THE NUMBER OF CHANGED JUDGMENTS WHICH WERE MORE ACCURATE (MA) AND LESS ACCURATE (LA)

| | Incorrect | | | Correct | | |
|---|---|---|---|---|---|---|
| | Total | Changed | | Total | Changed | $(MA-LA)$ |
| | | $(MA)$ | $(LA)$ | | $(LA)$ | |
| Experiment I (over-all) | 65.7 | 12.8 | 0.7 | 74.3 | 7.7 | 4.4 |
| Stimulus absent | 32.0 | 6.6 | 0.2 | 38.0 | 3.2 | 3.2* |
| Stimulus present | 33.7 | 6.2 | 0.4 | 36.3 | 4.5 | 1.3 |
| Experiment II (over-all) | 40.7 | 10.8 | 1.1 | 57.3 | 6.2 | 3.5* |
| Light set | 16.0 | 4.9 | 0.2 | 33.0 | 3.8 | 0.9 |
| Heavy set | 24.7 | 5.8 | 0.8 | 24.3 | 2.3 | 2.7* |

* Significant at the 5% level.

In Table II, incorrect judgments are distinguished as heavier or lighter than the correct weight. The incorrect immediate judgments tended to be heavier than the correct weight, the only exception being a non-significant difference for the light set of Experiment II. These results are consistent with previous findings that weights tend to be overestimated unless *O* has been accustomed to a series of heavier weights.[7] The Table shows also that the improvements in accuracy after prolonged deliberation can be traced predominantly to judgmental shifts in the direction of heavier. The same is true of changes which resulted in lesser accuracy; in all the experimental conditions, more of the changes were in the direction of heavier than lighter.

TABLE II

The Judgments Heavier than the Correct Weight (*H*) Compared with the Judgments Lighter than the Correct Weight (*L*) in Terms of (1) Number of Immediate Judgments, (2) Percentage of these Judgments in Each Category (*H* or *L*) which were Changed to Become More Accurate, (3) Number of Judgments which were Changed in the Indicated Directions (*H* or *L*) to Become Less Accurate

| | (a) | | (b) | | (c) | |
|---|---|---|---|---|---|---|
| | *H* | *L* | *H* | *L* | *H* | *L* |
| Exp. I (over all) | 49.2 | 16.4* | 16 | 26 | 6.8 | 1.5* |
| Stimulus absent | 24.7 | 7.3* | 18 | 20 | 2.6 | 0.8† |
| Stimulus present | 24.6 | 9.1* | 14 | 29* | 4.2 | 0.7* |
| Exp. II (over all) | 26.6 | 14.1* | 22 | 34† | 4.6 | 2.7 |
| Light set | 6.9 | 9.1 | 25 | 35 | 2.7 | 1.4 |
| Heavy set | 19.7 | 5.0* | 21 | 30 | 1.9 | 1.2 |

* Significant at the 1% level.
† Significant at the 5% level.

In Table III, incorrect judgments are distinguished as farther than the correct weight from the previous weight, or closer to it. The previous weight rather than the median weight was taken as a measure of *AL*, because the procedure of informing *O* of the correct weight, after each judgment, emphasized the previous weight as a comparison-point. The incorrect immediate judgments tended to be closer to the previous weight. The percentage of incorrect immediate judgments too close to the previous weight which were changed in the direction farther-from-the-previous-weight was larger than the percentage of judgments too far from the previous weight which were changed in the direction closer-to-the-previous-weight. That is, improvements in judgment tended to reflect a movement away from the previous weight. The same was true of changes in the direction of lesser accuracy under all conditions except the heavy set of weights in Experiment II.

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[7] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493; William Bevan and C. L. Darby, Patterns of experience and the constancy of an indifference point for perceived weight, this Journal, 68, 1955, 575-584.

## DISCUSSION

In the present experiments, changed judgments tended to be more accurate, heavier, and farther from the previous weight. The results suggest that the favorable effects of increased attentiveness outweighed the deleterious effects of time-delay. An earlier study of individual differences in psychophysical judgment showed that *O*s whose judgments tended to be contrasted from the *AL* ('sharpeners') were more accurate than the *O*s whose judgments tended to be assimilated toward the *AL* ('levelers').[8] In the present study, immediate judgments were more characteristic of the leveler and the changes in judgment after prolonged deliberation more characteristic of the sharpener.

TABLE III

JUDGMENTS DIFFERING FROM THE CORRECT WEIGHT BY BEING FARTHER FROM THE PREVIOUS WEIGHT (*F*) COMPARED WITH THE JUDGMENTS DIFFERING FROM THE CORRECT WEIGHT BY BEING CLOSER TO THE PREVIOUS WEIGHT (*C*) IN TERMS OF (1) NUMBER OF IMMEDIATE JUDGMENTS, (2) PERCENTAGE OF THESE JUDGMENTS IN EACH CATEGORY (*F* OR *C*) CHANGED TO BECOME MORE ACCURATE AND (3) NUMBER OF JUDGMENTS CHANGED IN THE INDICATED DIRECTIONS (*F* OR *C*) TO BECOME LESS ACCURATE

| | (a) | | (b) | | (c) | |
|---|---|---|---|---|---|---|
| | *F* | *C* | *F* | *C* | *F* | *C* |
| Exp. I (over-all) | 28.7 | 37.0† | 15 | 23 | 5.7 | 2.7* |
| Stimulus absent | 13.8 | 18.2 | 17 | 23 | 2.2 | 1.2 |
| Stimulus present | 14.8 | 18.8 | 15 | 21 | 3.4 | 1.5* |
| Exp. II (over-all) | 17.1 | 23.6† | 21 | 30 | 3.6 | 3.7 |
| Light set | 7.2 | 8.8 | 26 | 35 | 2.6 | 1.5 |
| Heavy set | 9.9 | 14.8* | 18 | 27 | 1.0 | 2.2† |

* Significant at the 1% level.
† Significant at the 5% level.

The procedure of informing the *O* of the correct weight after each judgment gave *O* the opportunity to compensate for constant errors of immediate judgment, during the process of prolonged deliberation. In Experiment I and with the heavy set of weights in Experiment II, most of the incorrect immediate judgments were, however, too heavy, and compensatory changes should have been in the direction of lighter. Any compensatory trend seems then to be outweighed by a stronger tendency for change in the direction of heavier.

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[8] Holzman, *op. cit.*, 375-394.

In the stimulus-absent condition of Experiment I, and under both conditions of Experiment II, prolonged deliberation occurred immediately after the weight had been put down. It has been suggested that this procedure should tend to cause judgments to be changed in the direction of lighter on the ground that the gradual release of the weight gives a diminishing sensory impression.[9] In the present study, the tendency for prolonged deliberation to increase the heaviness of judgments apparently outweighed this effect. In the 'stimulus-present' condition of Experiment I, the prolonged deliberation was not influenced by this diminishing sensory impression, because the weight was held steady until after the revised judgment; the heaviness of judgments was more strongly increased by prolonged deliberation in this condition than in the 'stimulus-absent' condition.

## SUMMARY

Weights were presented by the method of single stimuli to 24 *O*s in two experiments. The *O*s were instructed to make an immediate judgment of each weight, and then, upon careful consideration, to change the immediate judgment if it seemed incorrect. In the majority of cases, changed judgments were more accurate than immediate judgments. Changes tended also to be in the direction of heavier (rather than lighter) and farther away from the preceding weight (rather than closer to it). These effects, which are explainable in terms of increased attentiveness, outweigh the deleterious effects to be expected from the passage of time during deliberation.

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[9] D. C. McClelland, Factors influencing the time error in judgments of visual extent, *J. exp. Psychol.*, 33, 1943, 81-95.

# SIZE-DISTANCE INVARIANCE AND PERCEPTUAL CONSTANCY

By RODERICK P. McDONALD and PATRICIA T. O'HARA,
University of New England, N.S.W., Australia

Koffka's concept of perceptual invariants appeared for a time to offer at least a useful simplification for the description of a number of perceptual phenomena.[1] His hypothesis of invariants in the relationships of perceived size and distance, shape and slant and the like, would still seem to be implicit in a number of discussions of the perceptual constancies.[2]

For the case of size and distance, the hypothesis commonly takes the form: "A retinal projection or visual angle of given size determines a unique ratio of apparent size to apparent distance."[3] As soon, however, as we attempt to express this proposition in terms of laboratory-operations, difficulties arise that are due to the sheer variety of psychophysical procedures and instructional sets that may be used in obtaining explicit judgments of size and distance. It cannot be expected that different psychophysical methods for obtaining explicit judgments, under different instructional sets, will be in one-to-one correspondence with each other. Hence, it can hardly be assumed that any one of these procedures will be in perfect quantitative correspondence with 'apparent' size or distance. It is really not surprising, then, that a good deal of evidence has been accumulated by now which shows that the invariance-hypothesis is by no means universally valid. The field has been well reviewed recently by Epstein, Park, and Casey.[4]

In many ways, the most convincing type of evidence bearing on the invariance-hypothesis comes from experimental situations in which stimulus-correlates of one perceived attribute are manipulated experimentally, and explicit judgments of the other attribute are studied as a result of these variations. This type of procedure is described by Epstein *et al.* as an attempt to show "that a variable having a consistent influence on size-judgments has no consistent influence on distance-judgments" or vice versa.[5] Their review cites four such studies yielding results contrary to the

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* Received for publication February 2, 1962.

[1] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 223-230.

[2] F. P. Kilpatrick and W. H. Ittelson, The size-distance invariance hypothesis, *Psychol. Rev.*, 60, 1953, 223-231.

[3] Kilpatrick and Ittelson, *op. cit.*, 224.

[4] William Epstein, John Park, and Albert Casey, The current status of the size-distance hypotheses, *Psychol. Bull.*, 58, 1961, 491-514.

[5] Epstein, Park, and Casey, *op. cit.*, 498.

invariance-hypothesis, and two that are in its favor. The investigation reported below is of this general type, and in many ways it parallels Kilpatrick and Ittelson's experiment, one of the four negative instances just mentioned.[6] In their investigation, a stimulus-correlate of perceived distance, linear perspective, was subject to experimental manipulation by means of Ames's window. Due to the reversal of perspective induced by the window, objects passed through it in a direction normal to *O*'s line of regard were perceived by most *O*s to move away from them. In complete disagreement with the invariance-hypothesis, a considerable number of *O*s reported apparent decreases in the size of the objects.

From all the evidence reviewed by Epstein *et al.*, it is clear that the general validity of the invariance-hypothesis is no longer an issue. As Kilpatrick and Ittelson have pointed out, invariance-behavior would seem to occur only in some conditions but not in others, and the conditions of invariance-behavior remain to be discovered.[7]

As a first guess, invariance-behavior might be expected under conditions favoring perfect size-constancy. Consider a situation such that the judged size of an object does not diminish when its *physical* distance is increased (and hence its retinal angle decreased). In such a situation, it seems reasonable to suppose that the judged size of the object should increase if its *apparent* distance is increased while the retinal angle remains constant. Thus, it still seems reasonable to expect a close relation between the amount of size-constancy exhibited in a given experimental situation, and the extent to which the invariance-hypothesis holds or fails to hold in that situation. This, after all, is an integral part of Koffka's original notion, though it should be noted that this does not justify using invariance in an explanation of constancy, as Koffka did, or vice versa.[8] With the recognition that the degree of perceptual constancy and the extent of invariance are both merely summary descriptions of behavior in particular situations, the experiments described below are intended to test the hypothesis that conditions favoring size-constancy also favor invariance-behavior, as tested by the manipulation of stimulus-correlates.

For this purpose, the experimental situation should allow of a high degree of size-constancy, and a method of measuring it, together with control over stimulus-variables which effectively manipulate perceived distance. Also, it should preferably provide quantitative operations for the measurement of *O*'s judgments of size and distance. Gibson's well-known demonstration, in which an object is suspended above a textured ground, seemed more likely to meet these requirements than the situation based on Ames's window as used by Kilpatrick and Ittelson.[9]

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[6] Kilpatrick and Ittelson, *op. cit.*, 227-230.

[7] Kilpatrick and Ittelson, *op. cit.*, 230.

[8] Koffka, *op. cit.*, 224-230.

## EXPERIMENT I

*Observers.* The *O*s were 34 undergraduate students of psychology. None had previous experience or knowledge of the phenomenon.

*Apparatus.* The stimulus-objects were two black triangles, right-angled and isosceles, with the equal sides 12 in. in length. A table, 15 ft. long by 4 ft. wide, was covered with a gray and white woven checked cloth, the checks being about 1 in. square. A screen at one end of the table contained a small viewing hole 15 in. above the level of the table-top at its midline. The two triangles were placed similarly with their hypotenuses to the left as seen from *O*'s viewpoint, one on each side of the midline of the table with a minimal separation of 2 in., at a distance of 10 ft. from the viewing hole. The left-hand triangle was elevated about 4 in. above the surface of the table by a concealed rod which could be clamped to the far end of the table, while the right-hand triangle rested on the textured surface. Illumination was so arranged that the supporting rod for the elevated triangle did not cast any detectable shadows as seen by *O*.

*Procedure.* Each *O* viewed the triangles with his preferred eye and was asked to say whether the left-hand triangle was nearer or farther than the right-hand one, and whether it was larger or smaller.

*Results.* All 34 *O*s reported that the left-hand (elevated) triangle was further away than the right-hand triangle, and 33 out of the 34 reported that the former was larger, while one was uncertain.

## EXPERIMENT II

*Observers.* Ten of the *O*s employed in the first experiment went on immediately to make the observations required by the second.

*Apparatus.* In the situation of the first experiment, the right-hand triangle was replaced by an adjustable comparator consisting of a black triangle with a maximal size of 18 in. for the two equal sides, emerging from the vertical edge of a white screen. The size of the triangle could be adjusted and read from a scale by *E* from a position behind its masking screen. In this experiment, four non-critical targets, seated on the textured ground, were employed, as well as the elevated triangle (to be identified as 12 E) of the first experiment. These were:—(9) a 9-in. triangle at 13 ft. from *O*; (12 D) a 12-in. triangle at 10 ft. from *O*, *i.e.* the same distance as the elevated target and the comparator; (12 B) a 12 in. triangle at 13 ft., the distance at which the elevated triangle was expected to be perceived; (15) a 15-in. triangle at 11 ft. from *O*. Each of the five targets in turn was so arranged that its nearest edge to the midline of the table was about 1 in. to the left of it as seen by *O*.

*Procedure.* Judgments of size and distance were obtained for the four non-critical triangles and the elevated triangle. Each *O* made his observations in a prearranged random sequence. To obtain judgments of distance, the triangle was fully retracted while *E* moved a pointer on the surface of the table towards or away from

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[9] J. J. Gibson, *The Perception of the Visual World*, 1950, 178-180.

*O* in a line about 6 in. to the right of the midline. The essential part of the instructions was "tell me when you think it is opposite the triangle." For judgments of size, *E* slowly increased or decreased the size of the variable triangle, with the instruction "tell me when you think it is the same size as the left-hand triangle." Ascending and descending observations were averaged for each *O* to give *PSE*s for size and distance for each of the five triangles.

*Results.* Observations for the 9-in. and 15-in. triangles need not be considered, as they were included purely to prevent *O* from guessing that the same triangle was being used throughout.

The mean *PSE*s for distance were: 12 E, 13 ft. 6.2 in.; 12 D, 10 ft. 0.5 in.; 12 B, 12 ft. 6.1 in. An analysis of variance (which was hardly necessary) for the three matched groups of measures yielded an $F$ of 128.414 on 2 and 18 *df* ($p < 0.001$). By using the error-term from the analysis of variance, the difference between the mean distance-settings for 12 E and 12 D gave a $t$ of 15.58 on 18 *df* ($p < 0.001$).

The mean *PSE*s for size were: 12 E, 14.69 in.; 12 D, 12.05 in.; and 12 B, 11.81 in. An analysis of variance parallel to the previous one yielded an $F$ of 32.542 on 2 and 18 *df* ($p < 0.001$). Use of the error term provided showed that the difference between the mean size-settings for 12 E and 12 D gave a $t$ of 6.67 on 18 *df* ($p < 0.001$), while the difference of the means of 12 D and 12 B gave a $t$ of 0.67, which is not significant. It should be noted, however, that the mean *PSE* for the size of 12 E, 14.69 in., falls short of the size that would be predicted by the physical geometry of the situation, in terms of a strict interpretation of the invariance-hypothesis, the latter value being 15.9 in. ($p < 0.05$).

## DISCUSSION

The important feature of the experiment reported above is the provision for measuring size-constancy. As predicted, results in the direction implied by the invariance-hypothesis were obtained under conditions which do in fact favor size-constancy, as shown by the small and non-significant difference in size-judgments for 12 D and 12 B.

As pointed out earlier, one cannot assume a perfect quantitative correspondence between given psychophysical measures and an alleged phenomenal attribute like 'apparent' size or distance; hence, it seems to us that the invariance-hypothesis as commonly stated can only be tested in terms of such comparatively weak implications as the direction and significance of differences. The hypothesis *could* be taken to imply that, in the situation described above, the mean size-judgment for the elevated triangle should converge stochastically on 15.9 in. as the sample size becomes indefinitely large, irrespective of the psychophysical method and instructions employed. Such an interpretation seems uniquely over-strict.

The results of both experiments are in marked contrast to those obtained by Kilpatrick and Ittelson. In the present investigation, all individuals gave judgments in accord with the invariance-hypothesis, with the exception of one *O* in Experiment I, who was "uncertain." The distances of the stimulus-objects and the conditions of observation employed in this study are closely comparable to those used by Kilpatrick and Ittelson. Even so, it cannot be said that the results of this investigation cast doubt on their qualitative findings. A major difference lies in the method used for manipulating the stimulus-correlates of perceived distance. There may also have been an important difference in the instructions given to the *O*s. Unfortunately, Kilpatrick and Ittelson do not report their instructions.

The stimulus-conditions under which specific degrees of perceptual constancy can be expected have not yet been fully explored, on a systematic basis.[10] Even less is known as to what conditions favor invariance-behavior. It is this fact which lends heuristic value to the hypothesis that invariance-behavior and 'good' perceptual constancy go together. Clearly, in any relevant investigation, there is a good case for measuring both aspects of behavior together wherever this is possible.

Finally, two points deserve further emphasis. First, there is no reason to use perceptual invariants, where they can be found, in the explanation of the constancies, or vice versa. On this point, we agree with Gruber in rejecting Koffka's original notion.[11] Secondly, the outcome of the present investigation has no necessary bearing on the likely results for other types of tests of other alleged implications of the size-distance invariance-hypothesis. There would appear to be a number of subspecies of the hypothesis which need to be considered on their individual merits.

## SUMMARY

To test the hypothesis that apparent size and apparent distance would be related under conditions favoring size-constancy, an experimental situation was developed on the basis of Gibson's demonstration involving an object suspended above a textured ground. Results obtained were in accord with a weakened interpretation of the size-distance invariance-hypothesis, and were in marked contrast to the results obtained in a similar experiment by Kilpatrick and Ittelson.

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[10] Apart from a broad generalization in terms of the number of available cues, from the work of A. H. Holway and E. G. Boring, this JOURNAL, 54, 1941, 21-37, there are many isolated, nonsystematic studies, far too numerous to be worth listing.

[11] H. E. Gruber, The relation of perceived size to perceived distance, this JOURNAL, 67, 1954, 422.

# INFERRING THE LOCUS AND ORIENTATION OF THE PERCEIVER FROM RESPONSES TO STIMULATION OF THE SKIN

By **THOMAS NATSOULAS** and **RICHARD A. DUBANOSKI**,
Wesleyan University

A way of presenting stimuli for response which permits interpretation in terms of the locus of the perceiver is described by Krech and Crutchfield.[1] A figure, say *b*, is drawn by *E* on *S*'s forehead. If it is experienced as *b*, then the locus of perception may be said to be external; if it is experienced as *d*, the mirror-image, then the locus is internal. The special interest of the procedure lies in the fact that it yields a locus for the perceiver which is not prejudiced entirely by the physical locus of *S*'s perceptual system. Hebb has stressed the separability in experience of perceiver and body and has urged, for practical and theoretical reasons, the experimental study of the perceiver as a mediating process.[2] Piaget and Inhelder provide evidence, using other techniques, which suggests that the likelihood of a shift in the locus of perception from an internal to various external positions increases with the age of the child.[3]

The present experiment broadens the base of observations concerned with the determinants of the locus and orientation of the perceiver. The independent variables have been dictated in large part by the following conceptual scheme.

*(1) Head forward.* Considered first is *S* facing forward with eyes closed. Without special instructions or manipulations, the locus of the perceiver will be internal and the orientation straight ahead and will correspond, therefore, to the locus and orientation of the perceptual system. (a) When *b* is traced on *S*'s forehead, it will be experienced most frequently as *d*. To experience the figure as *b* would require shifting the locus of perception, would require greater 'perceptual effort' than maintaining the current locus. (b) When *b* is traced on the right or left side of *S*'s head, it will be experienced as *d, i.e.* from an internal locus, less frequently than when the letter is traced on the forehead. To experience the figure as *b* would require once more a shift in the perceiver's locus. Now, however, this change is pitted

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* Received for publication March 5, 1962.

[1] David Krech and R. S. Crutchfield, *Elements of Psychology*, 1958, 205.

[2] D. O. Hebb, The American revolution, *American Psychologist*, 15, 1960, 735-745.

[3] Jean Piaget and Barbel Inhelder, *The Child's Conception of Space*, 1953, 209-246.

against a change in the perceiver's orientation. To experience the figure as *d* requires a shift in orientation of approximately 90°. Thus it is assumed that *the greater the shift of orientation necessary for perception, the greater the probability of the alternative, a shift in location.* Consequently, drawing a figure on the back of the head will result in even fewer perceptions from an internal locus.

*(2) Head turned.* Considered next is *S* with head turned, right or left, and eyes closed. Whereas under conditions of facing forward, it is stated that the orientation of the perceiver corresponds to that of the head, this assumption cannot be extended confidently to the present conditions. Two other likely alternatives are, first, that the perceiver's direction of orientation will be frontal, will correspond to the direction of orientation of the remainder of the body, and secondly, that the direction of orientation of the perceiver will be a compromise between the frontal direction and the direction in which the head is turned. Taken together, however, with the assumption that the frequency of perceptions from an internal locus decreases with the degree of change in orientation necessary, each of the three alternatives result in hypotheses clearly discriminable from the other two, in terms of empirical expectations.

*(a)* If the orientation of the perceiver corresponds to the orientation of the head, *i.e.* is shifted by approximately 90° when the head is turned that much, then it can be shown that the results obtained from drawing figures at various places on the head will not differ from those obtained under conditions of facing forward.

*(b)* If the orientation of the perceiver is unchanged by the orientation of the head, *i.e.* always forward relative to *S*'s torso, then the results obtained by means of the letter-tracing procedure will depend on the head's direction. By use of the assumption of more changes in locus when greater change in orientation is necessary, the following predictions result. When the head is turned right [left], the greatest and smallest numbers of perceptions from an internal locus will be associated respectively with the left [right] and right [left] sides of the head, the other two places being intermediate and equal.

*(c)* If the orientation of the perceiver is at an angle which falls between the frontal direction and the direction of the head, then a more complicated set of results, depending on the angle of orientation, may be expected. When the head is turned right [left], the greater number of perceptions from an internal locus will be associated with the forehead and left [right] side of the head, while the smaller number of such perceptions will take place when the back and right [left] side of the head are stimulated. The relative contributions to the perceiver's orientation of the direction in which the head is turned and the orientation of the rest of the body will determine the precise angle of orientation of the perceiver and consequently the inequalities within the pairs referred to in the immediately previous statement, forehead and left [right], and back and right [left].

In addition to the two independent variables already discussed, handedness is studied on the assumption that the various changes in orientation of the perceiver may depend in ease of occurrence on this factor.

## METHOD

*Subjects.* Ninety-six men, undergraduates at Wesleyan University, volunteered and served individually as *S*s. Every *S* was tested by the same *E*. The place of testing varied, from an experimental cubicle to *S*'s room, but in every case, *S* and *E* were alone. Handedness was determined by asking *S*. If he showed some doubt, he was not used. Half of the *Ss* were left-handed and half right-handed.

*Procedure*. As soon as *S* agreed to participate, the following instructions were read to him while a 3 x 5-in. index card was held before him. "Here are four letters, *d*, *p*, *b*, and *q*. In the following experiment, you are to tell me which one of these letters you experience. There will be no repetition of any letter and you should respond as fast as possible. Therefore, your entire concentration is necessary. Are there any questions?" Whenever possible a part of the instructions was repeated to answer a question. Most frequently, the *S* expressed doubt as to the mode of and stimulus for response. The answer given was "This will be evident in the experiment." The instructions then continued, "Will you please close your eyes and keep them closed throughout the remainder of the experiment." *E* then traced the first symbol on the *S*'s head saying, "Now you just tell me what letter you experience." The letters were traced with the forefinger in a single motion beginning with the end of the stem of each letter. Four areas on the head were used as places to trace, the forehead, the two sides in front of the ears, and the back of the head.

TABLE I

PROPORTIONS OF *Ss* SHOWING INTERNAL PERCEPTUAL LOCUS

| Orientation of head | Place where letter is drawn on head | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | forehead | | | right side | | | left side | | | back | | |
| | $R_1$* | $R_2$† | T‡ | $R_1$ | $R_2$ | T | $R_1$ | $R_2$ | T | $R_1$ | $R_2$ | T |
| Facing forward | .687 | .708 | .698 | .437 | .521 | .469 | .479 | .500 | .490 | .167 | .083 | .125 |
| Turned right | .667 | .708 | .687 | .271 | .313 | .292 | .563 | .563 | .563 | .125 | .229 | .177 |
| Turned left | .583 | .646 | .615 | .542 | .604 | .573 | .375 | .354 | .365 | .083 | .187 | .135 |
| All positions | .646 | .687 | .667 | .417 | .479 | .444 | .472 | .472 | .472 | .125 | .167 | .146 |

* $R_1$ = Replication 1; † $R_2$ = Replication 2; ‡ T = Total over both replications.

The second independent variable was the orientation of the head. *S* was instructed to face forward, or to turn his head right, or to turn it left. Every *S* responded a total of 12 times to every combination of head-direction and place on head where the figure was drawn. The *Ss* differed as to the order in which they were tested with each combination. Two 12 x 12 Latin squares were used to counterbalance the order of presentation of the 12 stimulus-conditions for the 24 *Ss* in each experimental group. The four experimental groups were a left-handed and a right-handed group in each of two replications. For all the *Ss*, the order of presentation of the 12 stimuli used was *d*, *p*, *b*, *q*, *b*, *p*, *d*, *b*, *q*, *d*, *p*, *q*.

*Design and analysis*. The proportion of the *Ss* displaying an internal locus of perception under each condition was calculated. This provided a total of 24 proportions, one for each combination of replication, handedness, orientation of the head, and place on the head where letter was drawn. These proportions were subjected to an arcsine transformation to angles. An analysis of variance was performed on the transformed scores. All effects were tested by means of the corresponding interaction with replications. All analyses beyond the analysis of total variance were done with the transformed proportions.

## RESULTS AND DISCUSSION

Table I shows the proportions of *Ss*, under the various experimental conditions, who responded in such a way as to indicate an internal locus for perception. Because no statistically reliable differences are associated with the factor of handedness, right- and left-handed groups are combined.

The effect due to place on head where letter is drawn, the only significant main effect, has a probability of occurrence by chance of less than 0.001 ($F = 268.74$, $df = 3, 3$). There is also one significant interaction, that between the place on the head where the letter was drawn and the orientation of the head ($F = 6.18$, $df = 6, 6$, $p < 0.025$).

Since the predictions concern differences between pairs of values within the body of Table I, Duncan's new multiple-range test was used to determine the statistical significance of such differences.[4] The first prediction concerned *S* facing forward and involved differences, with respect to the relative frequency of perceptions from an internal locus, between places on the head where a letter is drawn. It was stated that the greatest and smallest numbers of such perceptions would occur respectively for the forehead and back of head, with the right and left sides of the head yielding intermediate values. These predictions are supported by the data. A greater proportion of the *S*s show an internal locus when stimulated on the forehead than on the left ($p < 0.05$), right ($p < 0.05$), or back ($p < 0.001$) of the head. Drawing the letter on the back of the head yielded a smaller proportion of perceptions from an internal locus than doing so on the left or right sides ($p < 0.01$ in both cases).

These results support the assumptions (a) that the orientation of the perceiver corresponds with that of the head when *S* faces forward and (b) that the greater the change of orientation necessary for perception from an internal locus, the greater the probability of a change in perceptual locus from internal to external.

When the head is turned right, the expectation was, under the third alternative [see (*c*) above], that the greater number of perceptions from an internal locus would occur when tracings are made on the forehead and left side of the head. The statistical analysis indicates that indeed forehead exceeded right ($p < 0.001$) and back ($p < 0.001$) as did left (versus right, $p < 0.005$; versus back, $p < 0.001$).

When the head is turned left, the parallel expectation under the same hypothesis was that the greater number of perceptions from an internal locus would occur when letters are traced on the forehead and right side of the head. Again this prediction is supported by statistical test. Forehead did exceed left ($p < 0.01$) and back ($p < 0.001$); similarly, the right exceeded both left ($p < 0.05$) and back ($p < 0.001$).

Consistent with the third alternative, these results place the direction or orientation of the perceiver at an angle between the frontal direction (relative to *S*'s torso) and the direction in which the head is turned. A somewhat more precise specification of the angle, *i.e.* whether the direction or orientation is closer to the frontal direction or to the direction in which the head is turned, can be determined by examining the proportions in Table I. It can be seen that under all conditions of head-position the proportion of the *Ss* who showed an internal locus is greatest when the letters are traced on the forehead, and least when they are traced on the back of the head. Taken together with the results previously described, this means that the angle of orientation of the perceiver is not only between the frontal direction and the direction in which the head is turned, but is closer to the direction in which the head is turned.

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[4] A. L. Edwards, *Experimental Design in Psychological Research,* 1960, 136-140.

## SUMMARY

A letter is drawn on the *S*'s head: on the forehead, the right or left side, or the back of his head. This letter may be perceived from an external locus, *i.e.* from *E*'s vantage point, or as its mirror-image, from an internal locus. The conceptual scheme presented offers two intermediary causes of how the letter will be perceived: (a) the orientation of the perceiver; and (b) the magnitude of change in orientation necessary for perceiving the letter from an internal locus. The greater the shift necessary, the greater the likelihood of perception from an external locus.

The orientation of the perceiver is assumed to be forward when *S* is facing forward. When *S*'s head is turned right or left, the perceptual orientation may correspond to the head-direction, to the frontal direction relative to the body, or to a direction between the latter two. The data of an experiment are used to infer the orientation of the perceiver.

Undergraduate men responded to letters drawn on the head in an experiment which involved three experimental variables: place on head where letter is drawn, the orientation of the head, and handedness. Every *S* underwent every treatment.

The results support the conceptual scheme. They also suggest that the orientation of the perceiver is at an angle between head-orientation and the frontal direction. Handedness had no effect.

# THE EFFECT OF INSTRUCTION ON POSITIVE TIME-ERRORS IN SUCCESSIVE COMPARISON

By HENRY GLEITMAN, Swarthmore College, and
JOHN CAMERON HAY, Smith College

Positive time-errors usually are found when a more intense stimulus is interpolated between the standard and the comparison-stimulus.[1] Positive time-errors have also been found when the more intense stimulus precedes, rather than follows, the standard.[2] Such results have been attributed to some kind of averaging process involving the standard and the extraneous stimulus. In Lauenstein's theory, the traces of the standard and of the extraneous stimulus are assimilated.[3] In Helson's theory, the two stimuli contribute to an *AL* with respect to which the comparison-stimulus is judged.[4] Both theories suggest, then, that the standard is underestimated because it is not compared with the standard as such, but with the standard as modified by the extraneous stimulus.

Whatever the nature of the averaging process, the standard stimulus seems to be weighted much more heavily than the extraneous one. Guilford and Park used a 200-gm. weight as the standard and a 400-gm. weight as the extraneous stimulus; as compared with a control condition in which no interpolation was provided, th eaverage upward shift was only 7 gm.[5] In another study, with a standard of 60 db. and an extraneous stimulus of 80 db. the effect of interpolation was no more than 2 db.[6] The fact of unequal weighting does not contradict the views of either

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[1] J. P. Guilford and D. G. Clark, The effect of interpolated weights upon comparative judgments, this JOURNAL, 43, 1931, 589-599; Otto Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 130-177; J. G. Needham, Interpolation effects with different time-intervals, *J. exp. Psychol.*, 18, 1935, 767-773.

[2] Henry Gleitman, Proactive and retroactive assimilation in the successive comparison of loudness, this JOURNAL, 70, 1957, 117-119.

[3] Lauenstein, *op. cit.*, 150-152.

[4] Harry Helson, Adaptation-level as frame of reference for prediction of psychophysical data, this JOURNAL, 60, 1947, 1-29; Adaptation-level as a basis for a quantitative theory of frames of reference, *Psychol. Rev.*, 55, 1948, 297-313.

[5] Guilford and Park, *op. cit.*, 592.

[6] Gleitman, *op. cit.*, 119.

Helson or Lauenstein; differential weighting is, in fact, demanded by these theories.

Why should the standard be weighted more heavily than the extraneous stimulus? The order of presentation cannot be the major cause, for the *PSE* still is far from the mean (arithmetic or geometric) of the two stimuli when the extraneous stimulus precedes the standard.[7] The cause of differential weighting seems rather to lie in the differential function of the two stimuli, only one of which is relevant to the judgment. Perhaps stimuli are weighted more heavily if they are more closely attended to; almost surely less attention is paid to the extraneous stimulus than to the standard. This suggestion has been made in somewhat similar form by Brown, who has shown that stimuli which must be judged by *S* contribute more to the *AL* than do stimuli which are presented but not judged.[8]

If the two stimuli are indeed weighted differentially because only one of them is relevant to the comparison, equal weightings should be achieved by equating their relevance at the time they are perceived. Usually there is unequal relevance, because *S* is informed in advance which of the two stimuli will serve as standard and which as the extraneous stimulus. If, however, *S* is not told which of them he is to use as standard until the third stimulus has actually been presented, he cannot know which of the two is more relevant. Furthermore, if the first and second stimuli serve equally often as standard, *S* has no basis for predicting which of the two will be more important on any given trial. Under such conditions, presumably, he must give equal attention to both stimuli. An averaging theory therefore should predict larger positive time-errors when *S* is instructed *after* rather than—as is usually the case—before the first two stimuli are presented. The purpose of the present study is to test this prediction.

*Method: (1) Design.* In essence, the experiment involved the successive presentation of three weights, *A*, *B*, and *C*. *A* and *B* were always the same for a given *S*; *C* was drawn from either of two series of comparison-stimuli ranging symmetrically around either *A* or *B*. On half the trials, the standard was *A*, on the other half, it was *B*. The choice of the series from which the comparison-stimuli were drawn was determined by which stimulus was the standard. On half the trials, furthermore, *S* was informed which stimulus was to be compared with *C* before he was exposed to any (*pre-instructed*). On the other half of the trials, this instruction was given only after *A* and *B* had been presented (*post-instructed*). When post-instructed in this manner, *S* had no way of predicting whether his comparison would concern *A* or *B*, since each was equally likely to serve as standard.

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[7] Gleitman, *op. cit.*, 119.

[8] D. R. Brown, Stimulus-similarity and the anchoring of subjective scales, this JOURNAL, 66, 1953, 199-214.

For 12 *Ss*, the stimuli were 100 gm. and 200 gm. with two comparison-series consisting of five weights, ranging from 85-115 gm., in one case, and from 185-215 gm., in the other. For another 12 *Ss*, the stimuli were 150 gm. and 300 gm., with comparison-series ranging from 130-170 gm., and from 240-360 gm., respectively. Each group was further subdivided by the order in which the first two stimuli were presented; for half the *Ss*, the first weight in the triad was always the larger, while for the other half, the reverse was true.

The conditions of the experiment that are of primary interest are those in which the standard is the *smaller* of the two weights. These are the conditions that have typically produced positive time-errors and to which averaging explanations apply most readily. The reverse conditions, in which the standard is the *larger* and the extraneous stimulus the smaller of the two weights were provided mainly to prevent *S*'s guessing which stimulus would serve as standard on post-instructed trials. These conditions are of lesser theoretical interest, since the averaging effect works in the same direction as the constant error to be expected even if no extraneous stimulus is provided. A negative time-error is expected in either case, although it is not clear under which of the two conditions it should be greater.[9]

*(2) Procedure*. The *Ss* were 24 undergraduates of the University of California at Berkeley. None had previously served in a psychophysical experiment. The weights were made of druggists' salve-boxes, cylindrical in form, with a diameter of 1.7 cm. and a height of 4.8 cm. These boxes were weighted with lead to a maximal error of ½ gm. and were painted flat black. *S* was instructed to lift all of the weights with the same hand and in the same manner. The timing of the stimulus-presentation was controlled by a clicker which sounded every 4 sec.

The general procedure followed the method of constant stimuli with randomized presentations of comparison-stimuli and of within-subjects conditions. *Equal*-judgments, were not permitted. Each comparison-stimulus was presented six times in each separate condition. Since there were five comparison-stimuli, there were 30 judgments per condition and 120 judgments per *S*.

The time-interval between *A* and *B* was always 4 sec. The interval between the 100-gm. weight and the comparison-weight (or the 150-gm. and the comparison-weight if the second stimulus-series was used) was always 8 sec. The heavier weight (200 gm. or 300 gm., repectively, for the first or second series) either preceded or followed the lighter one by 4 sec. In this manner, the time-interval between standard and comparison-stimulus was always the same under the crucial conditions (when the standard was lighter than the extraneous stimulus).

Instructions were given by colored patches on the weights. For some *Ss*, the first weight was always marked white and the second black (with reverse conditions for the other half of the *Ss*). When *S* was post-instructed, he had to wait until he saw the third weight before he could tell with which of the first two it had to be compared—if white, with the first; if black, with the second. When *S* was pre-instructed, the situation was exactly the same, except that *E* told him the color of the third weight before he was presented with the first.

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[9] The situation in which a standard is followed by a weaker extraneous stimulus raises difficult questions of interpretation. C. C. Pratt reports that this condition leads to a greater negative time-error than one in which no extraneous stimulus is presented, and he uses this finding as an argument against the exclusive determination of time-errors by averaging processes; silence presumably should have a greater effect on the trace-average than a soft sound, but the opposite was the case (The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 292-297). Kurt Koffka argues that Pratt's result is not conclusive, since his study involved the interpolation of both silence and a soft sound (*Principles of Gestalt Psychology*, 1932, 472). We are evidently sure neither of what actually happens nor of what should happen on theoretical grounds.

*Results.* For the conditions in which the extraneous stimulus was the heavier of the first two weights, *PSE*s were computed by the Müller-Urban method. The results are presented in Table I. As the table indicates, pre-instruction yields the positive time-error usually found with this method. The effect of post-instruction is to reduce, or even to reverse this error. Furthermore, the standard is estimated as heavier when the extraneous weight precedes rather than follows it. These observations are borne out by an analysis of variance of the constant errors produced under these conditions. To equate for the different standards, the constant errors produced with the larger stimulus-series (standard = 150 gm.) were multiplied by ⅔. The results of the analysis show a significant effect of pre- vs. post-instruction ($F = 28.33$, $df = 1$ and 20, $p < 0.01$) and of the order of standard and extraneous stimulus ($F = 4.84$, $df = 1$ and 20, $p < 0.05$). No other effects were significant.[10]

TABLE I

*PSE* AS A FUNCTION OF INSTRUCTION

| Condition (stimuli in gm.)* | *N* | *PSE* (gm.) | |
|---|---|---|---|
| | | pre-instructed | post-instructed |
| *100*–200 | 6 | 103.1 | 97.4 |
| *150*–300 | 6 | 156.1 | 147.6 |
| 200–*100* | 6 | 106.7 | 104.2 |
| 300–*150* | 6 | 158.2 | 152.8 |

* Italics are used to indicate the stimulus which served as the standard.

At least on first glance, these results throw some doubt upon an averaging theory of the positive time-error. Contrary to predictions made by such a theory, post-instruction led to a decrease rather than an increase in the postive time-error. A possible interpretation is suggested by the comments of the *S*s after the experiment. They reported that they "paid no attention" and in some cases "tried to ignore" the extraneous weight when pre-instructed. When post-instructed, however, they "tried to remember both weights" and sometimes "compared the two of them." Post-instruction evidently did heighten attention to the extraneous stimulus, but this stimulus was attended as a member—in fact, as the standard—of *another* stimulus-series.

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[10] For the conditions in which the extraneous stimulus was smaller than the standard, it was evident from mere inspection that no difference was produced by any of the experimental variations.

This line of reasoning makes it possible to reconcile the present findings with Brown's results.[11] In the post-instruction method, the extraneous stimulus was indeed judged (as it was not in the pre-instruction method), but it was judged as a potential member of another series entirely. Brown's categories of "judged" and "not judged" stimuli correspond to the difference between positive relevance and neutral status. In the present study, "not judged" (the effect of pre-instruction) and "judged separately" (the effect of post-instruction) may correspond instead to the difference between neutral status and explicit *non*-relevance.

Rather different issues are raised by the effect of temporal position of the extraneous stimulus. An earlier study showed no effect of position, which was taken as an argument against the belief that the processes responsible for the positive time-error are basically akin to those involved in retroactive inhibition: Positive time-errors should be larger when the extraneous stimulus follows rather than precedes the standard, for by the same analogy the latter is the condittion that corresponds to proactive inhibition, and *PI* is generally found to be less than *RI*.[12] The results of the present experiment make the same point even more strongly, since the observed effect was actually in the opposite direction. In addition, this result argues against any interpretation of the positive time-error based upon a simple contrast between the extraneous stimulus and the comparison-stimuli. A contrast-effect would be expected to increase as the extraneous stimulus becomes more contiguous with the comparison-stimulus. Since the positive time-error was greater when the extraneous stimulus preceded the standard, this view must be in error.

## SUMMARY

The primary conditions of the experiment concerned the successive comparison of a standard and of a comparison-weight as effected by a heavier, extraneous stimulus-weight which either preceded or followed the standard. On half the trials, the *S*s were told in advance which of the first two stimuli would serve as standard; on the other half, they were not informed until the comparison-stimulus was presented. Positive time-errors were obtained which were larger when *S*s were pre-instructed than when they were post-instructed. Furthermore, the positive time-errors were larger when the extraneous weight preceded rather than followed the standard. These results are not easily interpreted in the light of an averaging theory of the positive time-error.

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[11] Brown, *op. cit.*, 205.

[12] Gleitman, *op. cit.*, 119.

# PREFERENCES FOR BODILY PARTS AS A FUNCTION OF SEX, AGE, AND SOCIO-ECONOMIC STATUS

By SIDNEY WEINSTEIN, EUGENE A. SERSEN, LARRY FISHER, and ROBERT J. VETTER, Albert Einstein College of Medicine

In the course of research on changes in the perception of bodily parts deprived of sensory imput and on the effects of amputation, the question arose as to the relative value of the various bodily parts for normal individuals. In a study by Thorndike,[1] *S*s placed monetary value on a variety of bodily parts and functions. Thorndike dealt with only a few parts, however, and did not present values for each part separately. The present study was intended to provide a comprehensive analysis of the roles of sex, age, and socio-economic level in relation to part-preference.

TABLE I

DISTRIBUTION OF *S*s ACCORDING TO AGE, SEX, AND SOCIO-ECONOMIC RANK

| Socio-economic rank | Sex | Age | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | <20 | 20–29 | 30–39 | 40–49 | 50–59 | 60–69 | 70–79 | >79 | Total |
| 1 | M | 0 | 68 | 74 | 51 | 41 | 25 | 7 | 0 | 266 |
| | F | 0 | 56 | 46 | 37 | 29 | 5 | 2 | 2 | 177 |
| 2 | M | 0 | 206 | 24 | 15 | 9 | 1 | 0 | 0 | 255 |
| | F | 0 | 200 | 41 | 18 | 14 | 5 | 1 | 0 | 279 |
| 3 | M | 331 | 17 | 18 | 18 | 31 | 12 | 4 | 3 | 434 |
| | F | 283 | 36 | 21 | 46 | 29 | 19 | 5 | 0 | 439 |
| 4 | M | 0 | 7 | 4 | 11 | 12 | 15 | 32 | 9 | 90 |
| | F | 0 | 3 | 4 | 15 | 14 | 28 | 23 | 4 | 91 |
| Total M | | 331 | 298 | 120 | 95 | 93 | 53 | 43 | 12 | 1045 |
| Total F | | 283 | 295 | 112 | 116 | 86 | 57 | 31 | 6 | 986 |
| Total | | 614 | 593 | 232 | 211 | 179 | 110 | 74 | 18 | 2031 |

*Population.* A total of 2031 *S*s without amputations participated in the study, 1045 men and 986 women. Table I presents the number of *S*s in each age-group

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[1] E. L. Thorndike. Valuations of certain pains, deprivations and frustrations. *J. genet. Psychol.*, 51, 1937, 227-239.

as a function of sex and socio-economic rank. All *S*s resided in the New York metropolitan area, except for 86 men and 79 women who lived in Georgia. This group did not differ noticeably from the larger sample in terms of age or occupation. Most of the questionaries from the older *S*s were obtained through the coöperation of various old-age organizations.

*Questionary.* The questionary consisted of a single sheet with space for recording *S*'s age, occupation, and highest education completed. Separate forms were administered to men and women. The instructions printed on each sheet were as follows:

> This study is part of a large program under the auspices of the U. S. Department of Health, Education, and Welfare. We are studying the effect of patient's attitudes toward body parts that have been removed. We would like to evaluate the attitudes of normal individuals toward these parts.
>
> Rank all the following parts, starting with the one you would *least miss* if removed, ending with the one most missed. For example, if a fingernail were included you would probably put a '1' next to it since it would be least missed. The part most missed should be given the rank of '12' [13 for the men]. Each intermediate part must get a different number from 2 to 11 [12 for the men] with no ties or omissions.

The remainder of the questionary consisted of a list of bodily parts with a space next to each for the rank. Those parts in common for both sexes were: *arm, big toe, ear, eye, foot, hand, leg, nose, thumb, tongue,* and *tooth. Breast* was added to the female list, *penis* and *testicles* to the male list. To control for the effects of order of listing, the parts were listed in alphabetical order for half the forms and in reverse order for the other half.

*Socio-economic rank.* A socio-economic rank was devised for each *S* on the basis of the means of 5-point occupational and educational scales. Examples of the occupational ratings are: (1) lawyers and physicians; (2) store and factory managers; (3) salesmen; (4) clerks; and (5) laborers. The educational rating was based on the following categories: (1) completed 4 years of college; (2) attending college (or completed less than 4 years); (3) completed high school only; (4) attending high school (or completed less than 4 years); (5) completed grade school or less. The mean of the educational and occupational ratings yielded a socio-economic rank determined as follows: mean ratings of 1 and 1.5 were given a rank of 1; 2 a rank of 2; 2.5 and 3 a rank of 3; and 3.5-5 a rank of 4. In the case of students and a few others for whom information concerning only education (or occupation) was available, the rank was based solely on the data available.

*Administration.* The questionary was, in most cases, completed by *S*. For elderly *S*s who had visual or language-difficulties, it was necessary for an interviewer to assist in completing the form. The questionaries were administered and returned immediately upon completion, except for 100 which were sent by mail.

*Results.* Since the two orders of listing parts on the questionary yielded no apparent differences, the data from both forms were combined. Further, since the ranks of the Georgia group were quite similar to those of the New York group, the two sets of data were combined. We were surprised to find that the responses of an essentially rural population (Statesboro, Georgia) did not differ from those of *S*s in the New York metropolitan area. An even more surprising result was that differences in socio-economic level did not affect preferences.

Indicative of the unimportance of these variables is the finding that the greatest range of mean ranks across the four socio-economic levels and all ages for any bodily part was only 1.0 for men and 1.5 for women.

Since the differences among socio-economic levels were slight, these data were also combined.

Fig. 1 presents the mean ranking of the men and women according to age. It can be seen that the ranks of relatively unimportant parts, *i.e.* tooth, hallux, and thumb, remain much the same over the years, while for the other parts more variability is apparent. The changes with age which are most apparent are the devaluations of the sex-specific parts. The other changes and fluctuations with age for parts such as ear and leg may depend upon changes in cosmetic or functional values. Despite some variability within certain groups of rankings, such as the cluster comprising nose, leg, eye, and ear in the females, these groups tend to remain distinct from one another. Thus, if one ignores adjacent ranks, the curves for the majority of parts rarely intersect. Further, except for the sex-specific parts, few parts increased or decreased more than one or two ranks. If we consider the woman's hand, for example, we find that in eight decades it exchanges

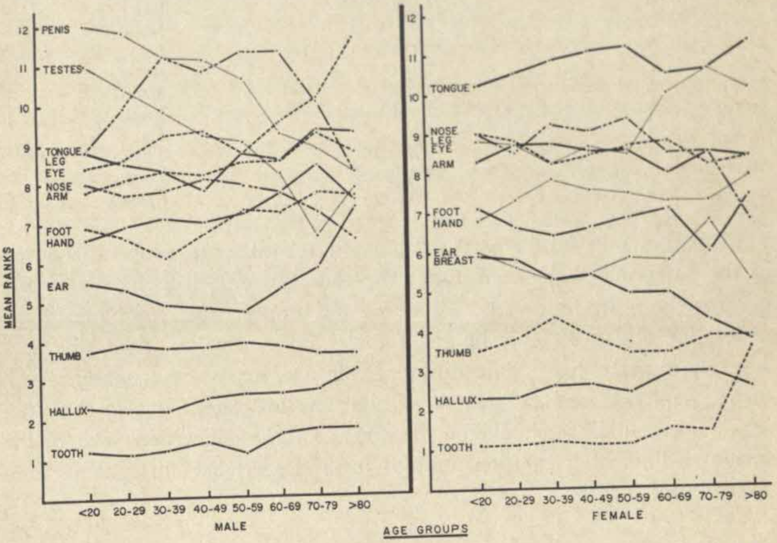

FIG. 1. BODILY-PART PREFERENCES OF MEN AND WOMEN AS A FUNCTION OF AGE

ranks with only foot, ear, and leg, and for each part at only a single decade. It remains distinct from all other parts; it is always superior to breast, thumb, hallux, and tooth, and always inferior to arm, eyes, nose, and tongue.

If we compare the sexes, regardless of age or socio-economic status, we find once more a relatively high level of consistency. Table II, which presents the ranks for the parts in common to both sexes, shows that seven of the parts shared the same rank; the maximal rank-difference was only two for the other four parts. Thus, eye and arm were valued more by the males, leg and nose more by the females. In conclusion, then, the data strongly indicate that despite differences in age, sex, socio-economic level, or geography, there is, in general, a high level of consistency in the rankings. These findings should be of value to those concerned with bodily image and somatic sensation, and to medical specialists in rehabilitation and general surgery.

TABLE II

RANKS OF BODILY PARTS COMMON TO BOTH SEXES

| Rank | Male | Female |
|---|---|---|
| 11 | Tongue | Tongue |
| 10 | Eye | Leg |
| 9 | Leg | Nose |
| 8 | Arm | Eye |
| 7 | Nose | Arm |
| 6 | Hand | Hand |
| 5 | Foot | Foot |
| 4 | Ear | Ear |
| 3 | Thumb | Thumb |
| 2 | Hallux | Hallux |
| 1 | Tooth | Tooth |

*Summary.* A questionary on preferences for bodily parts was administered to 1045 men and 986 women. Mean ranks of 12 parts in the female and 13 parts in the male were analyzed as a function of sex, socio-economic status, and age. Socio-economic or sex-differences had little effect. The sex-specific parts tended to decline in relative preference with age. The rankings of the majority of the other bodily parts fell into clusters whose rankings remained quite consistent.

# CROSS-CULTURAL CONSISTENCY OF CHILDREN'S PREFERENCES FOR THE ORIENTATION OF FIGURES

By HELEN FAIGIN ANTONOVSKY, The Hebrew University, Jerusalem, and LILA GHENT, George Washington University

It has been reported recently that American children of preschool age show remarkably consistent preferences for the orientation of geometric figures, considering them to be right-side-up in one orientation and upside-down in another.[1] Analysis of the children's responses suggested that a figure was considered right-side-up when its focal portion was in the upper half. This hypothesis was supported in an experiment with cards in which the focal portion of each card was defined as the one point of differentiation in the card. The present study is concerned with the responses of children in a non-western culture to the same series of cards.

*Subjects.* The *S*s were 20 boys and 20 girls attending a nursery school in Teheran, Iran.[2] All the children came from upper-class homes. Since their ages were known only approximately, the *S*s were drawn from the groups of children classified as 4-5 yr. of age in the school. For 30 of the 40 *S*s, the teacher was able to specify whether the child was four or five years old. The previous study had been done with 40 children, 4-5 yr. olds in a nursery school for children of working mothers in New York City.

*Method.* The procedure was essentially the same as that used previously. Pairs of cards, identical in form, but varying 180° in orientation, were held before the *S*s. Half the *S*s were asked to point to the picture that was upside-down, and the other half were asked to point to the picture that was right-side-up. Since there are no Persian equivalents of 'right-side-up' and 'upside-down,' the children were asked to point to the picture in each pair that was not the way it should be or turned around, or they were asked to point to the picture that was correct. In general, the children were coöperative and interested, and the only *S*s excluded were three children who showed strong position-habits.

The series of 33 cards presented to the *S*s can be divided into three groups. One group comprised 10 pairs of realistic forms (man's face, horse, tree, flowers, house, full-length man, table and chair, cow, cat, car), which were included to provide a check on the child's understanding of the instructions. Another group comprised the 13 pairs of very simple geometric cards used in the earlier study and on which

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[1] Lila Ghent, Form and its orientation: a child's-eye view, this JOURNAL, 74, 1961, 177-190.

[2] The authors are indebted to Dr. P. Birjandi, Director, and to Miss A. Naficy, Head Teacher, of the nursery school, without whose interest and coöperation this work could not have been done.

interest was centered in this study. The additional 10 geometric cards consisted of more complex cards which had been designed originally to explore the effects of size, brightness, and color in evoking preferences for orientation. Since these complex cards had been presented to the American children at the same time as the 13 simple cards, the group of 10 complex cards was also presented to the Iranian *S*s to keep the conditions of testing the same for the Iranian and American *S*s. The two groups of geometric cards, and the cards with the realistic figures, were arranged in a fixed random order; they were presented to half the *S*s in one sequence, and in the reverse sequence to the other half of the *S*s.

*Results.* The number of American and Iranian *S*s making the predicted responses to the set of 13 simple cards is shown in Fig. 1.

The responses of the Iranian children were in the same direction as those of the American children for every one of the cards, and the choices of the Iranian *S*s were significantly different from chance (at the 5% level or beyond) for 11 cards; differences for the remaining two (the fourth and the eighth items) were at the 6% level. In general, it appeared that the relative effectiveness of the various figures in evoking preferences was approximatly the same in the two groups. For example, the two items that just missed significance in the Iranian group were those figures that evoked the lowest consistency of response in the American *S*s.

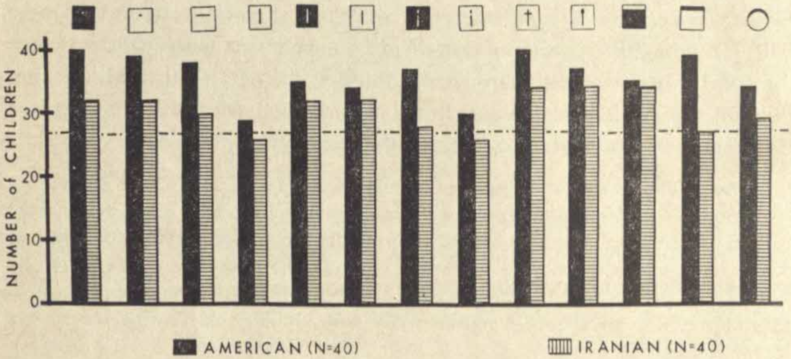

FIG. 1. THE NUMBER OF CHILDREN CONSIDERING THE CARD RIGHT-SIDE-UP IN THE ORIENTATION SHOWN HERE.

The broken line extending across the graph indicates the number of choices required for significance at the 5% level.

It should be noted that two of the forms used were not placed asymmetrically on the cards, nor did the forms contain focal points (Fig. 1, the fifth and sixth items); the pairs of cards presented for choice consisted of a centered line oriented vertically and a line oriented horizontally. These pairs of cards were part of the series used originally with the

American children to test the hypothesis (suggested by other data) that children not only prefer the focal part at the top, but that they prefer forms to be oriented vertically rather than horizontally. Both the American and the Iranian *S*s showed a strong preference for the vertical orientation.

The one conspicuous difference between the American and Iranian *S*s was in the lower consistency of response to all the cards shown by the Iranian *S*s. To evaluate statistically the apparently lowered consistency of preference, we gave each child a score representing the total number of responses that were made in the predicted direction. (Since the majority of the children showed a high number of predicted responses, the distributions of scores for the American and Iranian *S*s both were very skewed.) It seemed that the low scores in the Iranian group were contributed primarily by the 4-yr.-old children, but, since the ages of the Iranian *S*s were only approximate and 10 children were omitted from this analysis by age because of lack of information concerning age, the reliability of the age-difference is, perhaps, questionable. It would be reasonable to expect a lesser consistency in the Iranian group, and perhaps in the younger children particularly, due to the distracting influence of a foreign examiner and the frequent presence of the nursery-staff members in the testing room.

The responses to the simple cards of Fig. 1 constitute the cross-cultural comparison of main interest, since it was to these cards that the American children showed such consistent responses, but responses to the group of more complex cards also were analyzed. The American *S*s had showed significant preferences for some, but not all, of the cards, and the Iranian *S*s presented, in general, a similar pattern. The one exception was the response to the one card in the series consisting simply of two colors, a rectangle of royal blue adjacent to a rectangle of bright orange. The American *S*s showed a significant preference for the blue on top and the Iranian *S*s showed a significant preference for the opposite orientation. This curious difference between the groups has defied interpretation.

*Conclusion.* The preferences for orientation of figures shown by American children are not specific to this culture. This fact is consistent with the interpretation that the preferences relate to basic processes of form-perception.

# THE RELATION BETWEEN INTROVERSION AND SALIVATION

By D. W. J. CORCORAN, Applied Psychology Research Unit, Cambridge, England

The neurophysiological correlate of high levels of activation has been considered to be a state of high cortical facilitation.[1] On the basis of this hypothesis, it is reasonable to expect that the output from an effector of a highly aroused organism would be greater than that of a lesser aroused organism, when both are equally stimulated. It has been suggested by the writer[2] that the personality dimension introversion-extraversion, as measured by Heron's scale,[3] is related to degree of arousal.

If introverts are in general more highly aroused than extraverts, then, assuming that arousal is synonymous with a state of high cortical facilitation, it follows that the output of an effector of an introvert should be greater than that if an extravert when both are equally stimulated. Thus, a few drops of lemon juice applied to the tongue of an introvert should, in a given interval of time, produce a greater output of saliva than from an extravert. Introverts should also be more susceptible to painful stimuli and less susceptible to sensory deprivation—both of which have recently been demonstrated.[4]

*Subjects and procedure.* A pad of cotton wool was weighed, placed on S's sublingual salivary gland, and left there for a prescribed time. It was then reweighed to determine how much saliva had been absorbed by it. Another pad was then positioned, and a given quantity of lemon juice dropped on the S's tongue. The pad was removed after the same interval of time, weighed, and from this weight was substracted the weight of saliva absorbed by the first pad.

This procedure was carried out on three groups of Ss. Group 'Lemon 1' consisted of 11 Ss. The pad of cotton wool was left in place with this group for 20

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* Received for publication Feb. 21, 1963.

[1] Frederick Bremer, The neurophysiological nature of sleep, in J. F. Delafresnayé (ed.), *Brain Mechanisms and Consciousness,* 1954, 137-158: Neurophysiological mechanisms in cerebral arousal, in G. Wolstenholme and M. O'Connor (eds.), *Nature of Sleep,* 1961, 30-49.

[2] D. W. J. Corcoran, Individual differences in performance after loss of sleep, 1961. Unpublished Doctoral dissertation. University of Cambridge.

[3] Alastair Heron, A two-part personality measure for use as a research criterion, *Brit. J. Psychol.,* 47, 1956, 243-251.

[4] Asenath Petrie, Walter Collins, and Philip Solomon, The tolerance for pain and for sensory deprivation, this JOURNAL, 73, 1960, 80-91.

sec., and four drops of lemon juice were used. The procedure was repeated four times, twice in the forenoon and twice in the afternoon of successive days. In the second group, the 'Citric Acid' group, four drops of citric acid were used, with the same time-interval, *i.e.* 20 sec. The citric acid solution was subjectively assessed to be of approximately equal concentration to that of lemon juice. This group of 12 *Ss* was tested twice under normal conditions on two successive afternoons, once after having been awake for about 19 hrs., that is from 7 A.M. of one day to 2 A.M. of the next day, and once after 43 hr. of wakefulness. The *Ss* in a third group, the 'Lemon 2' group, were tested with two drops of lemon juice, and the pads of cotton wool were left in place for 10 instead of 20 sec. This group consisted of 12 *Ss* and was tested once only.

The Heron scale was administered by laboratory staff, unconnected with the present work, according to normal laboratory procedure.

*Results.* No significant differences in introversion-scores were observed among the three groups.

*(1) Group Lemon 1.* Since the *Ss* in Group Lemon 1 were tested on four occasions, it was possible to calculate retest coefficients. These correlations were very high with *Rhos* higher than 0.9 and $p$-values less than 0.01. The amount of saliva secreted in the forenoon was no different from that secreted in the afternoon. Pooled over the four trials, the amount of saliva secreted was positively related to introversion; Kendall's *tau* coefficient gave a correlation of 0.62, $p = 0.014$.[5] Extraverts secreted a mean of 0.526 gm. ($SD = 0.4438$), and introverts a mean of 0.949 gm. ($SD = 0.4176$).

*(2) Citric Acid Group.* The Citric Acid Group showed no significant relationship with introversion (tau = 0.06) and no differences in the quantity of saliva secreted after loss of sleep. The quantities of saliva secreted were very slightly higher than those of Group Lemon 1.

*(3) Group Lemon 2.* In view of the negative result in the Citric Acid Group, it was decided to test a further group, using lemon juice. A relation between the quantity of saliva secreted and introversion was found in this group (Spearman's $Rho = 0.70$, $p < 0.02$). Over the 10-sec. period of the stimulation, extraverts secreted on average 0.248 gm. ($SD = 0.2121$) and introverts 0.538 gm. ($SD = 0.1000$).

The results appear on the whole to indicate that differences exist between introverts and extraverts in terms of the quality of saliva secreted in response to lemon juice. There are, however, some curious features about the results. It is peculiar (1) that the differences in the citric acid group were not reliable. The amount of saliva secreted by this group was only slightly higher than that of lemon 1, which suggests that the acidity of the citric acid was not greatly different from that of the lemon juice; and (2) that, if the salivation measure were detecting differences in level of arousal between introverts and extraverts, it did not detect differences in arousal between the experiments in the morning and the afternoon. This expected difference is deduced: (a) from the well-known, diurnal rhythm shown by such indices of arousal as body temperature and heart rate;[6] and (b) from the fact that differences in performance exist between introverts and extraverts when the time of testing is changed from morning to afternoon.[7]

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[5] Sidney Siegel, *Nonparametric Statistics*, 1956, 213-223.

The salivation-measure should also have been lower after loss of sleep than under control conditions, since there is evidence relating lowered arousal to loss of sleep.[8]

Although the results on the whole confirm the prediction, they may of course be due to some difference between introverts and extraverts other than that of degree of arousal.[9]

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[6] Nathaniel Kleitman, *Sleep and Wakefulness,* 1939, 191-282.

[7] W. P. Colquhoun, Temperament, inspection efficiency, and time of day, *Ergonomics,* 3, 1960, 377-378.

[8] Corcoran, Noise and loss of sleep, *Quart. J. exp. Psychol.,* 14, 1962, 178-182.

[9] The author's thanks are due Mr. D. E. Broadbent for his assistance and advice, and to the British Royal Navy for supplying *Ss*, equipment, and assistants.

# APPARATUS

## AN APPARATUS FOR THE VISUAL DISPLAY OF SPEECH-SOUNDS

By Cecil C. Bridges, Texas Christian University

The device described here is an apparatus for the visual display of the patterns of speech, *i.e.* variations in frequency-time relationships. For deaf or partially deaf persons, a number of phonemes are not associated with visible movements of the speaker's tongue or lips, hence simple imitative learning is not possible. Some phonemes in foreign languages also resist learning by imitative methods, and additional visual cues might be useful in shaping correct pronunciation. This device provides a simple visual feedback as a substitute for missing or ignored auditory cues. In addition to applications in speech-training or teaching in the pronunciation of foreign languages, other aspects of speech may be investigated using this visual feedback.

In the present device, the height of an oscilloscopic trace (assuming a steady tone) or its rate of approach to some asymptotic height (assuming a transient tone) is a function of the frequency of the peak-clipped speech-signal. The device functions in a manner similar to the frequency-discriminator in an FM-radio; it produces a voltage which varies directly with frequency of the signal and this voltage is translated by the oscilloscope into height of a visible trace. The apparatus incorporates a microphone, amplifier, peak-clipper, differentiating and integrating circuits, and an oscilloscope (Fig. 1).

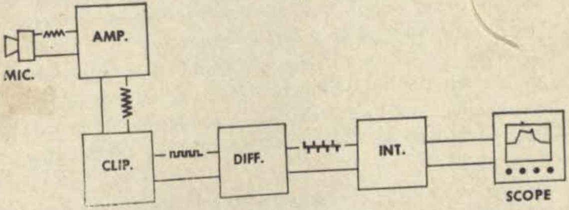

Fig. 1. Block Diagrams of the Device

It is believed that the circuitry is of ultimate simplicity. An auxiliary unit incorporating the peak-clipping, differentiating, and integrating circuits was designed to be used with available amplifiers and oscilloscopes without an additional power supply. A transformer provides an impedance match between a high fidelity high-gain amplifier and the device. Two silicon diodes serve as a peak-clipper. A resistor and a capacitor serve as a differentiator, and a germanium diode and capacitor (in conjunction with the high internal resistance of the oscilloscope) serve as the integrator (Fig. 2). More concretely, each of the silicon diodes acts as a low resistance 'short' whenever the voltage in either direction exceeds a certain value, thus truncating sine (or other wave-forms) to an approximate square wave. The first capacitor is charged almost immediately after the square wave crosses the time-axis and is discharged rapidly by its associated resistor, producing a sharply peaked 'pip.' The germanium diode acts as a rectifier, allowing these sharp pulses to flow into the last condenser in only one direction to charge the condenser. The input resistance of the oscilloscope dissipates this charge at a rate proportional to its amount. Thus, given a steady tone, the greater the number of pulses flowing into the condenser per unit of time, the higher its charge and the greater the voltage difference across its terminals and the higher the oscilloscope trace.

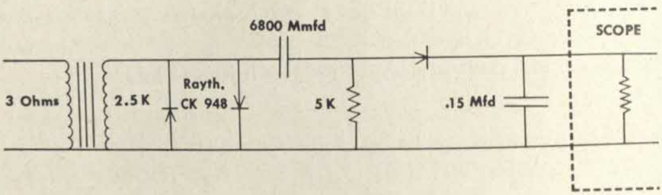

FIG. 2. CIRCUITRY OF THE DEVICE.

An analogy for this circuit would be an air-tank with a leak. The faster one pumps, the higher the pressure, but when one stops pumping, the pressure leaks off. Because of the peak-clipper and differentiation, all pulses entering this final capacitor are equal, so all "strokes of the pump" are equal, and output is not dependent on the intensity of the initial signal, but only on its frequency.

In using the display as a training device, one could use a grease pencil to draw a 'model' trace on the face of the oscilloscope for *S* to imitate, or one could use a tape-recorded speech-sound and run it through a similar circuit to provide a model. One could then use one trace of a dual trace oscilloscope for *S*'s response and the other trace for the model.

By measuring the momentary voltage-difference between two such circuits, one fed by *S*'s speech-signal and one fed by a tape-recorded model, one could measure the degree to which *S*'s response approximates the model using the momentary voltage difference between *S* and the outputs of the model. One could integrate this difference signal over the duration of the speech-sound to provide a measure of the over-all degree of correspondence between response and model, or one could simply use maximal voltage difference as a measure of momentary maximal discrepancy. In either case, a relay with adjustable sensitivity could be added, along with an electrically activated reinforcement-device, so that *S* was automatically reinforced on each trial in which the discrepancy of the response-model was below, some pre-set level.

One might make the acceptable level of discrepancy dependent upon *S*'s performance, *e.g.* if *S*'s last response were within the limits, the limits might be tightened by automatically biasing the relay to open with a lower voltage difference, or if the last response were unsuccessful, the limits might be 'loosened' by changing the bias on the relay so it would open only with a large voltage difference.

Using this apparatus, we are shaping the clipped speech-pattern of *S*'s vocalizations. To the extent that clipped speech contains the important aspects of unclipped speech, we would expect *S*'s unclipped speech to become more intelligible with training. Because of the high intelligibility of clipped speech, it apparently does contain most of the information-conveying aspects of unclipped speech.

One of these devices is presently being used in our speech clinic for clinical evaluation of its usefulness in speech-therapy.

# A CHAIR-STABILOMETER

By BEN FUJITA, University of Portland, and
STEPHAN P. SPITZER, State University of Iowa

In the history of psychology, many instruments have been used for measuring physiological reactions to stimuli. Among these have been the galvanic skin-response, electrocardiograph, electroencephalograph, electropneumograph, plethysmograph, and eye-blink recorder, etc. A disadvantage common to these instruments is that they require an overt attachment of some apparatus to the *S*, who can hardly fail to be aware of it. Any subsequent response may be in part, at least, a consequence of this awareness.

The instrument described here gives a single numerical score of total activity over any given period of time plus a single graphic recording of activity.[1] Our primary purpose was to construct an apparatus with which we could detect differences in gross bodily movement.[2]

The stabilometer unit consists of four components:

*(a) The chair.* The seat was cut out of an ordinary wooden classroom chair. A new bottom was fitted 2 in. below the level of the first seat. The space was filled with two air cushions that were sewn together (one on each side). A new seat was fitted loosely on top of the cushions. On top of this was placed a standard chair cushion of canvas material. The appearance and 'feel' of the chair were so natural that the chair did not arouse the *S*'s suspicions that it was not an ordinary chair.

*(b) The transducer.* The transducer consisted of a crystal phonograph cartridge that was activated by an opposing pair of metal bellows which were connected by tubing to each cushion. The transducer gave no reading as long as the weight on each cushion was unchanging. When, however, a shift of bodily posture or position occurred, the pressure in the cushions and bellows changed and activated the phonographic cartridge. Whether or not the weight of the body is evenly divided between the cushions is not crucial because the amplification of the signal is a function of rate of change and not upon pressure exerted upon the cushions.

*(c) The amplifier.* The resulting signal of the transducer was amplified, fed into a phase-inverter and a push-pull cathode-follower which was connected to the recorder. The amplifier was conventional except that care was taken to extend the

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[1] Parts of this paper are based on a dissertation submitted to the faculty of the Graduate School of the University of Washington. The authors wish to express their appreciation to Allen L. Edwards, Roger Brown Loucks, and Ezra Stotland, without whose aid the study could not have been made.

[2] The apparatus was built by Henry E. Guttman, who was then Chief Electronics Technician for the Department of Psychology, University of Washington.

low frequency response. The input impedance was adjusted to match a five milliampere Esterline Angus recorder.

*(d) Recording unit.* Responses were recorded on an Esterline Angus Pen recorder and a digital score or tally was kept on a Hunter Electronic Klockcounter. The counter was activated by a sensitive relay. The relay could be adjusted to trip whenever the signal exceeded a given minimum. It could be set, therefore, to count only the largest movements or the smallest movements (such as pulse and respiration). For the present study the Hunter Klockcounter was set to register one count on each closure of the relay. It was not set to give an indication of the amount of time each *S* spent in movement.

A pilot study using approximately 110 *S*s was conducted to identify types of body movement tending to activiate the recording units. Talking aloud, writing, or engaging in motor tasks activated the counter and graphic recorder. Limb movements, such as tapping of the foot or scratching, movements of the head and trunk, sway from side to side or backward or forward activated both recording units.

Two experiments were conducted with the stabilometer.

*Experiment I.* The difficulty-level and type of instructions for a series of verbal analogies were varied for 73 *S*s. Thirty-seven *S*s were subjected to a stress-inducing condition (intelligence-test instructions with a difficult set of analogies) which was shortly thereafter followed by a non-stress inducing condition (neutral preliminary instructions paired with a simple set of analogies). Thirty-six *S*s received the reverse procedure, non-stress condition followed by stress-condition. Body-movement was recorded on the counter for a total of 3 min. under each experimental condition, while the *S*s worked on the task. The *S*s showed significantly fewer movements under the stress-condition than under the non-stress condition ($t = 2.40$; $p < 0.02$).

*Experiment II.* Twenty *S*s were told that good performance on a puzzle task would allow them to win a monetary reward. After being given a practice trial on the puzzle, 10 *S*s were informed that they were highly successful and had an excellent chance of receiving the reward. The remaining 10 *S*s were informed the opposite, that they were very unsuccessful on the practice trial and therefore had a poor chance of winning the reward. Measurement of body movement on the graphic recorder for a 1-min. period following evaluation of the *S*s performances showed significantly less movement under the failure condition than under the success condition ($F = 8.48$; $p < 0.02$).

## SUMMARY

The purpose of this study was to determine whether a newly-developed instrument could detect changes in gross bodily activity. The instrument, known as the chair-stabilometer, consisted of four components: (1) a

sensing unit; (2) a transducer element; (3) an amplifier for amplifying voltage changes; and (4) two standard recording devices. The *S*s, seated on the stabilometer, were subjected to two kinds of conditions: (a) conditions designed to produce a state of stress or anxiety; and (b) conditions designed to produce no undue stress or anxiety. Two experiments using the instrument indicate that during stress conditions *S*s show significantly less bodily movement than under non-stress conditions. The chair-stabilometer is ideal for investigations in which a physical measure of bodily activity is required without the disadvantages of direct attachment of apparatus to *S*. The design is such that measurements can be made without *S*'s awareness. The instrument is also valuable for studies in which physical measurements of induced psychological tension are desired. The principle of this apparatus may be adapted for use to other types of stabilometers, such as infant crib-recorders, sleep-activity recorders, and animal-activity recorders.

# AN APPARATUS FOR THE MEASUREMENT OF THE STARTLE-RESPONSE IN THE RAT

By HOWARD S. HOFFMAN and MORTON FLESHLER,
Pennsylvania State University

The device presented here was designed to measure the intensity of the startle-response in an unrestrained rat.[1] Previous instruments for this purpose have employed a postage-scale as the sensing unit and have required mechanical linkages to the recording-pen.[2] In general, such systems are difficult to damp, and, because of their delicate structure, they tend to get out of adjustment quite easily. The present device differs from earlier instruments in that its sensing unit and recording linkages are electrical rather than mechanical.

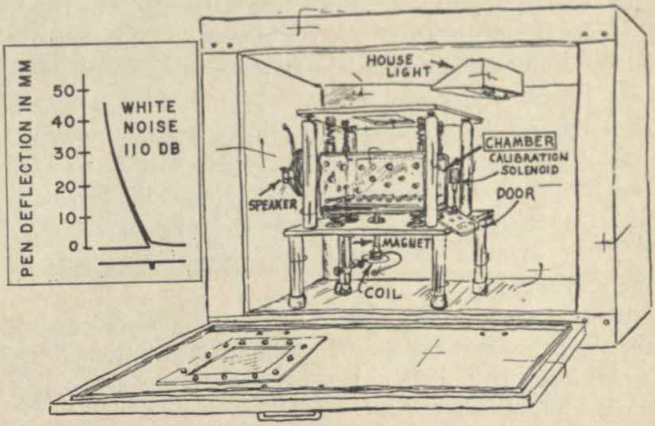

FIG. 1. APPARATUS FOR THE MEASUREMENT OF STARTLE
The inset shows the type of record produced when a rat is exposed to a sudden, loud acoustic stimulus.

The device is shown in Fig. 1. A small, rigid plexiglas chamber is suspended and supported by coil springs connected to the upper and lower plates of a superstructure constructed of heavy-gauge aluminum (¼-in. flat stock for the plates and 1-in. round stock for the supporting rods). For purposes of sound-isolation, the super-structure is placed inside a large chest, the door to which is fitted with a one-way-observational port. The four corner-screws at the top of the superstructure, which permit adjustment of the spring-tension, are tightened to produce substantial compression of the springs. As a result, even vigorous movements of *S* produce displacements of the chamber so small that they cannot be seen, and oscillatory movements are heavily damped.

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[1] This device was developed and tested as part of a program of research supported by NIMH grant M2433 (C3).

[2] J. S. Brown, H. I. Kalish, and I. E. Farber, Conditioned fear as revealed by magnitude of startle response to an auditory stimulus, *J. exp. Psychol.*, 41, 1951, 317-328.

The response-detection system involves the transduction of chamber-movements into electrical current and the measurement of the current by an Esterline Angus recording milliammeter. Firmly fixed to the bottom of the chamber is an aluminum rod which has a magnet in its bottom end. The magnet rides in a coil mounted directly to the superstructure in a manner permitting adjustment. Movements of the magnet in the coil produce a current proportional to the *rate* at which magnetic lines of force cross the coil. For this reason, the device is highly sensitive to the sudden sharp movements involved in startle, but is relatively insensitive to the slow, though perhaps gross, movements involved in general activity. The current generated in the coil goes to the input of a 20-w. audio-amplifier, the output of which is rectified before being passed to the recording milliammeter.

The inset in Fig. 1 illustrates the type of record produced; the stimulus was a 0.4-sec. pulse of white noise at 110 db. re 0.0002 dynes per $cm^2$. The record reads from left to right and the deflection of the lower pen signals the occurrence of the noise.

To assess the system's reliability, a solenoid was fastened to one of the supporting poles of the superstructure (see Fig. 1), and a brass extension-rod was fixed to its iron slug. When a fixed voltage is applied to the coil, the slug is drawn in and the result-impact yields a response on the recorder. Since the amplitude of the impact-record has remained constant (18 ± 2 mm.) for thousands of trials involving several months of work, it is concluded that the system stays in adjustment and is highly reliable.

# A VERSATILE DEVICE FOR THE MEASUREMENT OF SKIN-RESISTANCE IN RATS AND HUMANS

By Stephen Kaplan, University of Michigan, and
James L. Hobart, Laser Systems Center

Basal resistance-level (*BRL*) has recently been gaining support as a motivational measure fully as promising as the more traditional galvanic skin-response (*GSR*).[1] A device capable of the simultaneous recording of both these variables is thus of some interest. A further recent development is a technique for recording *BRL* in unrestrained rats.[2] These varied possibilities have led to the development of a versatile device capable of recording the resistance-level of humans as well as that of rats at a wide variety of different sensitivities.

*Circuit.* The main considerations in the design and construction of the circuit of the control-unit (Fig. 1) were reliability, ease of operation, and low cost. To this end, the design has been kept as simple and straightforward as possible.

The current for the *S* is provided by a 200-v. half-wave power supply which is filtered and regulated by three NE-2 neon bulbs. A 20 MΩ resistor in series with the supply gives a current of 10 μA. For *Ss* whose resistance does not exceed 1M Ω, the current is constant to within 5%.

The voltage across the *S*, developed by the 10-μA current, is attenuated by a high impedance voltage-divider, the output of which goes directly to a 10-mv. servo-recorder. The servo-recorder must have a large input impedance to avoid loading the divider.

In many situations, it is desirable to limit the voltage which may be developed across the *S* and as a consequence limit the upward deflection of the servo-recorder. This is accomplished by a type 1N482 silicon diode, chosen for its very low reverse current. The anode of the diode is connected to the *S* and the cathode to a variable divider across the power supply.

Scale-calibration is accomplished by switching-in, in place of the *S*, various precision-resistors. All the resistors and the *S* are so connected in series with a shorting switch across each that, by opening the appropriate switch, one may select any desired combination.

*Characteristics.* The control unit[3] connected to a suitable servo-recorder[4] forms a system with the following properties: (1) It has a rectilinear write-out, permitting use and easy reading of the scale at both extremes. (2) It has a wide-band write-out, permitting the simultaneous recording (without the necessity of shifting sensitivities) of small, transient variations (*GSR*) as well as large, slower shifts (*BRL*). (3) It is portable. (4) It provides a wide variety of ranges suitable for human- and rat-research with a high degree of linearity. (5) It has a built-in calibration system covering its wide range. (6) It has a current-limiter which permits the selection of scales linear over the lower three-quarters of the band, and nonlinear thereafter.

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[1] E. Z. Levy, V. H. Thaler, and G. E. Ruff. New techniques for recording skin resistance changes, *Science*, 128, 1958, 33-34.

[2] Stephen Kaplan, and Rachel Kaplan. Skin resistance recording in the unrestrained rat, *Science*, 138, 1962, 1403-1404; Rachel Kaplan, Rat basal resistance level under stress and non-stress conditions. *J. comp. physiol. Psychol.*, 1963, 56, 775-777.

[3] Commercially available from D-J Labs, 626 Duane Court, Ann Arbor, Michigan.

[4] Any servo-recorder with a 0–10-mv. range and high input impedance will suffice.

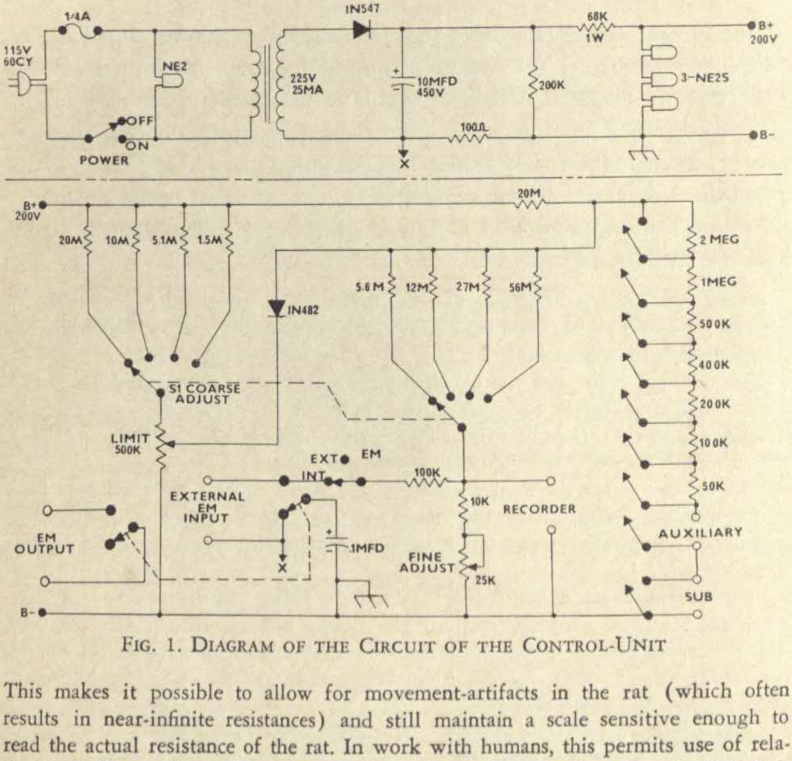

FIG. 1. DIAGRAM OF THE CIRCUIT OF THE CONTROL-UNIT

This makes it possible to allow for movement-artifacts in the rat (which often results in near-infinite resistances) and still maintain a scale sensitive enough to read the actual resistance of the rat. In work with humans, this permits use of relatively sensitive scales while preventing an extreme *S* from going off scale. (7) It is compatible with the simple, highly stable electrodes for human research described by Lykken.[5] (8) It has an unique event-marking system utilizing the recorder-pen. This can be used in addition to the regular event-marker, and is particularly valuable for use with recorders lacking a separate event-marker. (9) *GSR* devices currently available commercially possess few of the above advantages and are in general considerably more expensive.

The low cost of this control unit makes it a suitable device for instructional laboratory use and for multiple installations, while its high accuracy and flexibility make it appropriate to the most exacting research.

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[5] D. T. Lykken. Properties of electrodes used in electrodermal measurements, *J. comp. physiol. Psychol.*, 52, 1959, 629-634.

# APPARATUS NOTES

## A NEW SPLIT-BEAM TACHISTOSCOPE GIVING AN EXACT COLOR-MATCH

Dodge tachistoscopes embody a system of semi-silvered mirrors for combining or alternating the stimulus-fields. It is possible to match the intensities of the reflected and transmitted fields by careful control of the silvering. It is not, however, possible to match the colors of the two fields, as the metal-film which forms the semi-silvered surface absorbs light at the blue end of the spectrum from the transmitted beam while the reflected beam is reflected more or less non-selectively. This disadvantage of semi-silvered mirror systems is overcome in the present design, which also provides other advantages.

Fig. 1 illustrates the basic arrangement of the system for a three-channel tachistoscope. $M_1$, $M_2$ and $M_3$ are semi-silvered mirrors. Light in Channel 1 reflects off Mirror $M_2$ and is transmitted through Mirror $M_1$. Light in Channel 2 is transmitted through Mirror $M_3$ and is reflected off Mirror $M_1$. The beams in Channels 1 and 2, therefore, arrive at the eyes having been treated identically, assuming that the three mirrors are optically equal. If the mirrors are cut from one large mirror, then they should be similar. The defective color-match inherent in traditional semi-silvered

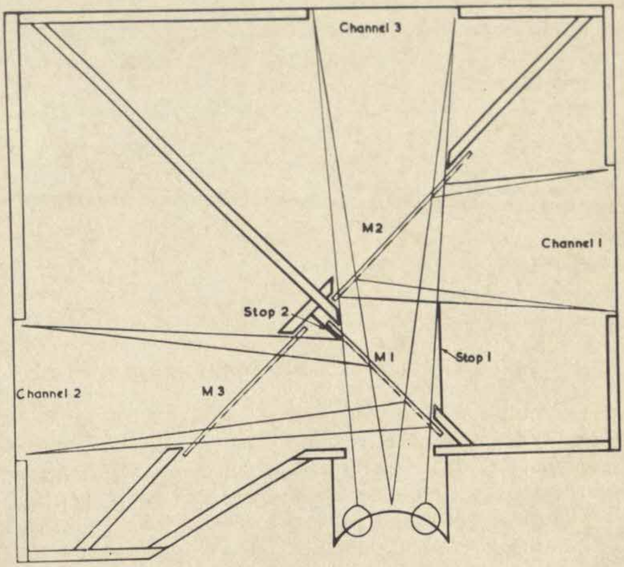

FIG. 1. THE DESIGN OF THE NEW TACHISTOSCOPE

mirror tachistoscopes has therefore been overcome. Light in Channel 3 is transmitted through two mirrors and is, therefore, not color-matched with the other channels. For most purposes, however, two matched fields are sufficient. A three-channel matched instrument could be made by adding a reflecting mirror in Channel 3 and extra transmitting mirrors in the other two channels.

Our design has several advantages over that of the conventional three-channel tachistoscope. The semi-silvered mirrors are nearer to the eyes, so that even though the field is just as large, the amount of mirror used is less. The shape of our instrument is much more compact and less awkward. Light can easily leak between channels in the old design but no direct path exists between the separate channels in the new design. The Stops 1 and 2 are provided to close the direct path between Channels 1 and 2. Finally, in the new design the semi-silvered mirror does not need to have any specified ratio of transmission to reflection. Plain glass will give a pair of channels perfectly matched for color and intensity, and if the glass is very thin the doubling of the reflected image is not obvious.

University of Durham,
England

IAN P. HOWARD
JOSEPH A. EVANS

# NOTES AND DISCUSSIONS

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## HYPNOTICALLY HALLUCINATED COLORS AND THEIR NEGATIVE AFTER-IMAGES

In recent reviews, Weitzenhoffer and Kirkner concluded that hypnotically hallucinated colors resemble actual colors in that they give rise to negative (complementary colored) after-images.[1] Since this conclusion has major implications for research in the areas of hypnosis, hallucinations, and visual processes, the data from which it derives require careful scrutiny. The present paper attempts to meet the need for a critical reëxamination of this issue.[2]

As far back as 1888, Binet and Féré reported that hypnotic color hallucinations are followed by negative after-images.[3] Their subjects consisted of an unspecified number of hysteric patients. When the patients were in 'a somnambulistic hypnosis,' they were shown a sheet of white paper and given the suggestion that the paper was red. After fixating on the suggested red, they were presented with a second sheet of white paper and asked to report what they saw there. All their *Ss* stated that they saw the color green on the second white sheet. Binet and Féré did not give further details of their experiments stating only that all hypnotized *Ss* always reported that hallucinated colors gave rise to complementary colored after-images; and, since the *Ss* were naïve hysterics, they could not possibly have had prior knowledge of the quality of the negative after-images.

Of nine subsequent investigations which attempted to confirm the findings of Binet and Féré, six obtained negative or equivocal results and three positive.

*(1) Negative and equivocal.* Sidis obtained equivocal findings in an early study.[4]

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* The writing of this paper was made possible by a research grant (MY4825) from the National Institute of Mental Health, United States Public Health Service.

[1] A. M. Weitzenhoffer, *Hypnotism: An Objective Study in Suggestibility,* 1953, 156-157; F. J. Kirkner, Control of sensory and perceptive functions by hypnosis, in R. M. Dorcus, *Hypnosis and Its Therapeutic Applications,* 1956, Chap. 3.

[2] This paper is part of a series which critically reëxamines the effects traditionally attributed to 'hypnosis.' Other papers in this series include: T. X. Barber, Antisocial and criminal acts induced by 'hypnosis': A review of experimental and clinical findings, *Arch. gen. Psychiat.,* 5, 1961, 301-312; Physiological effects of "hypnosis," *Psychol. Bull.,* 58, 1961, 390-419; Experimental controls and the phenomena of "hypnosis": A critique of hypnotic research methodology, *J. nerv. ment. Dis.* 134, 1962, 493-505; Hypnotic age regression: A critical review, *Psychosom.* Med., 24, 1962, 286-299; Toward a theory of hypnosis: Post-hypnotic behavior, *Arch. gen. Psychiat.,* 7, 1962, 321-342; Toward a theory of "hypnotic" behavior: The "hypnotically induced dream," *J. nerv ment, Dis.,* 135, 1962, 206-221; The effects of "hypnosis" on pain; A critical review of experimental and clinical findings, *Psychosom: Med.,* 25, 1963, 303-333.

[3] Alfred Binet and Charles Féré, *Animal Magnetism,* 1888, 250-255.

[4] Boris Sidis, Are there hypnotic hallucinations?, *Psychol. Rev.,* 13, 1906, 239-257.

Although he did not present details of his experiments, he reported that his hypnotized *Ss* (a) never claimed that hallucinated colors gave rise to negative after-images when they were unacquainted with the negative after-image phenomenon, and (b) always reported such after-images after this phenomenon had been explained to them.

Bernheim, Dorcus, and Naruse reported negative findings.[5] Bernheim placed two selected *Ss* in 'deep trance' and suggested that part of a white disk was red. The *Ss* claimed that they saw the "red" very distinctly. They were told to fixate on the "red" portion of the disk and then told to name the color perceived on the other half of the disk. They did not report the complementary hue, stating that the other part was white (the actual color). Dorcus and Naruse instructed hypnotized *Ss* to project an hallucinated color on a different color and to describe the resultant after-image. In no case did the 5 *Ss* in the Dorcus experiment or the 10 *Ss* in the Naruse experiment report 'correct' after-images for the suggested colors.

In a more complex experiment, Hibler obtained data indicating that the after-images of hallucinated colors reported by hypnotized *Ss* are a function of whatever preconceptions the *Ss* have concerning the after-image phenomenon.[6] Four *Ss* were used, each of whom possessed normal color vision and met criteria for 'deep trance.' The experiment included three major aspects: (1) The *Ss* were asked what they expected to see if they stared at a color (blue, red, yellow, and green) and then turned their gaze to a neutral surface. (2) They were then placed in 'deep trance,' presented with a gray card for 45 sec., and given the suggestion that the gray card was alternately blue, red, yellow, and green. After they testified that they 'perceived' the suggested color, they were shown a second gray card and asked to report what they saw there. (3) Finally, actual colors were presented and the hypnotized *Ss* were instructed to hallucinate each color as some other color. After 'perceiving' the suggested color, they were asked to look at a gray surface and to describe the after-image.

The first part of the experiment showed that all the *Ss* had preconceptions as to what colors they would see after fixating on blue, red, yellow, and green. (After fixating on blue, for instance, one *S* expected to see an orange after-image, a second expected to see a blue after-image, and the remaining two expected to see a yellow after-image.) The second part of the experiment demonstrated that the after-images of the hallucinated colors reported under 'deep trance' were in every case consistent with the *Ss*' preconceptions. (The *S* who had expected to see an orange after-image after fixating on blue, consistently reported that hallucinated blue was followed by an orange after-image; the *Ss* who had expected to see blue or yellow, consistently 'saw' blue or yellow after-images.) In the third part of the experiment, two *Ss* again reported after-images from hallucinated colors that were in accord with their preconceptions and the remaining two reported the after-images of the real rather than the suggested colors. Hibler concluded that "within the limits of the experiment, there is no evidence that [hypnotic] hallucinations produce any central or sensory change in the organism that cannot be explained in terms of mere verbal agreement and coöperation with the experimenter."

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[5] H. Bernheim, *Suggestive Therapeutics*, 1888, 97-98; R. M. Dorcus, Modification by suggestion of some vestibular and visual responses, this JOURNAL, 49, 1937, 82-87; Gosaku Naruse, Hypnosis as a state of meditative concentration and its relationship to the perceptual process, in M. V. Kline, *The Nature of Hypnosis*, 1962, 37-55.

[6] F. W. Hibler, An experimental study of positive visual hallucinations in hypnosis, Doctoral dissertation, Ohio State University, 1935; F. W. Hibler, An experimental investigation of negative after-images of hallucinated colors in hypnosis, *J. exp. Psychol.*, 27, 1940, 45-57.

Hibler's experiment has been criticized by Elizabeth Erickson in that (a) the suggestions employed were too ambiguous and too tentative to elicit visual hallucinations, and (b) the hypnotic inductive procedure, which subsumed a period of less than 5 min., was too brief to induce "sufficiently profound dissociation to permit such difficult phenomena as valid visual hallucinations."[7] Although the first criticism may be justified, an experiment presented by Elsea, described below, suggests that the second criticism is not of major importance.

Elsea obtained negative findings in a rigorous study.[8] To insure naïvete with respect to the negative after-image phenomenon, this investigator selected his *Ss* from 14- to 17-yr. old girls at the Oklahoma State Training School. A preliminary study with 40 *Ss* from the school demonstrated that girls at this institution did not have knowledge of complementary color phenomena. Thirty-two volunteers, who met criteria for 'deep somnambulistic trance,' were assigned to the experimental group; and 32 volunteers, who were not seen for hypnotic testing, were assigned to the control group. The effects of three variables were investigated: (1) The first was concerned with the duration of the trance-induction procedure. The control group was not exposed to the induction of trance. Half of the experimental group were exposed to a 5-min. trance-induction procedure and the other half to a 30-min. procedure. (2) The second was concerned with the *Ss'* knowledge of negative after-images. Half of the *Ss* in each group were given information concerning complementary colored after-images and the others were not given such information. (3) The third variable was concerned with the chromaticity of the pseudo-stimulus cards. Half of the *Ss* in the hypnotic group were shown gray cards in conjunction with the color-suggestions and half were shown colored cards and given the suggestion that the cards were another color. Half of the controls were instructed to "look at" gray cards and the others to "look at" chromatic cards.

The results were as follows: (a) the *Ss* given the extended trance-induction reported no more 'correct' after-images to the hallucinated colors than the *Ss* receiving the brief trance-induction; (b) the hypnotized *Ss* did not report a greater number of 'correct' after-images than the controls; (c) the *Ss* given information concerning negative after-images did not report more 'correct' after-images than those not given such information; (d) the *Ss* shown the gray cards reported no more 'correct' after-images than those shown the chromatic cards. In brief, none of the experimental variables had a noticeable effect; the number of 'correct' after-images reported did not exceed chance expectations under any of the experimental conditions.

*(2) Positive findings.* The negative findings obtained in the above experiments are not conclusive; three recent experiments, summarized below, obtained positive results.

Milton and Elizabeth Erickson worked with five college students who were highly

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[7] E. M. Erickson, Critical comments on Hibler's presentation of his work on negative after-images of hypnotically induced hallucinated colors, *J. exp. Psychol.*, 29, 1941, 164-170.

[8] O. C. Elsea, Jr., A study of the effect of hypnotic suggestion on color perception, Doctoral dissertation, University of Oklahoma, 1961.

'trained' hypnotic *Ss*.[9] After a "profound hypnotic trance" was induced, the *Ss* were shown white sheets of paper and given suggestions that the sheets were alternately red, yellow, green, and blue. They were asked to describe the after-images of the hallucinated colors as perceived on a second white sheet. One *S* did not hallucinate the suggested colors; the other four apparently hallucinated each suggested color and consistently reported negative after-images. A word-association test and post-experimental interviews seemed to indicate that three of the four *Ss* who reported complementary-colored after-images under hypnotic trance were unable to verbalize complementary color relationships in the waking state.

Rosenthal and Mele replicated the essential features of the Ericksons' procedure with four selected *Ss* who were, presumably, students at the University of Chicago.[10] Three of the hypnotized *Ss* reported negative after-images for each of the suggested colors and the fourth reported negative after-images for all but one of the suggested colors. Post-experimental interviews appeared to indicate that under normal circumstances three of the four *Ss* did not have "conscious knowledge" of complementary colors.

Barber obtained positive findings with six "good" hypnotic *Ss* selected from Harvard undergraduates.[11] After the administration of a brief (less than 5 min.) trance-induction procedure, two of the six *Ss* claimed that suggested hues (blue, red, yellow, green, and orange) gave rise to complementary-colored after-images. The *Ss* were then asked to imagine the colors under 'waking' conditions and to report the after-images. Two of those who had not reported negative after-images under the 'trance'-condition reported complementary colored after-images under the 'waking' condition. Both of these *Ss*, however, later stated that they felt as if they had "gone into trance" when imagining the colors under the supposedly 'waking' condition. As a control, Barber instructed 11 *Ss*, selected from the same college population as the hypnotic *Ss* but not screened for "hypnotizability," to imagine the colors under "normal waking conditions." Two of the 11 controls claimed that each imagined color gave rise to a negative after-image. In post-experimental interviews, three of the four hypnotic *Ss* and both controls, who had reported 'correct' after-images, denied that they had prior knowledge of complementary colors.

Additional research is needed to reconcile the contradictions in the above investigations. It is imperative, however, that further experiments in this area institute very stringent selection procedures to exclude the possibility that *Ss* may have knowledge of negative after-images. That this possibility may not have been excluded in the investigations presenting positive findings is indicated by the following.

The 'correct' after-images reported by the *Ss* participating in the Binet and Féré, Erickson and Erickson, Rosenthal and Mele, and Barber experiments were consistent with the negative after-image phenomenon as it is described in elementary college textbooks, but they were not consistent with the negative after-image phenomenon as it is actually observed in real life. These *S*s almost always reported green as the after-image of hallucinated red, blue as the after-image of hallucinated yellow, and vice versa. Although elementary texts give the impression that the negative after-image of red is green, that the after-image of yellow is blue, and vice versa, two considerations suggest that these are over-simplified statements.

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[9] M. H. Erickson and E. M. Erickson, The hypnotic induction of hallucinatory color vision followed by pseudo negative after-images, *J. exp. Psychol.*, 22, 1938, 581-588.

[10] B. G. Rosenthal and Howard Mele, The validity of hypnotically induced color hallucinations, *J. abnorm. soc. Psychol.*, 47, 1952, 700-704.

[11] Barber, The afterimages of "hallucinated" and "imagined" colors, *J. abnorm. soc. Psychol.*, 59, 1959, 136-139.

(1) Elsea asked 25 *S*s, who possessed normal color vision, to describe the after-images perceived after gazing at actual colors.[12] The majority did *not* describe the after-images in the manner given in elementary texts or in the manner of the *S*s giving a 'correct' performance in the above experiments. On the contrary, 88% described the after-image of red, not as green, but as various shades of blue; 88% described the after-image of green, not as red, but as violet, lavender, purple, and so on; 60% described the after-image of yellow, not as blue, but as violet or purple. Blue was the only color whose after-image (yellow) was described in the textbook fashion by the majority.

(2) Not only are there wide individual differences in descriptions of after-images, but also (a) *S*s unacquainted with the negative after-image phenomenon often fail to perceive the after-images produced by actual colors until they have had a number of practice trials, and (b), as Downey has observed, "even under the most unvarying conditions variations in the results obtained from any one individual will occur."[13]

Since the *S*s participating in the experiments of Binet and Féré, Erickson, Rosenthal and Mele, and Barber tended to describe the after-images in a textbook fashion and showed very little intra- and inter-individual variations in their descriptions, they may have previously read elementary textbook accounts of the negative after-image and were able, during the experiment, to surmise what responses were expected from them. It should be noted that Ericksons and Barber worked with college students and that Rosenthal and Mele imply that they also used college students. It should also be noted that Bernheim implicitly criticized the Binet and Féré experiment for failing to take the precaution of not "saying a

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[12] Elsea, *op. cit.*, 69-71.

[13] J. E. Downey, An experiment on getting an after-image from a mental image, *Psychol. Rev.*, 8, 1901, 42-55; J. P. Sutcliffe, "Credulous" and "sceptical" views of hypnotic phenomena: A review of certain evidence and methodology, *Int. J. clin. exp. Hypn.*, 8, 1960, 73-101. Experiments on esthesia, hallucination, and delusion, *J. abnorm. soc. Psychol.*, 62, 1961, 189-200.

'trained' hypnotic *Ss*.[9] After a "profound hypnotic trance" was induced, the *Ss* were shown white sheets of paper and given suggestions that the sheets were alternately red, yellow, green, and blue. They were asked to describe the after-images of the hallucinated colors as perceived on a second white sheet. One *S* did not hallucinate the suggested colors; the other four apparently hallucinated each suggested color and consistently reported negative after-images. A word-association test and post-experimental interviews seemed to indicate that three of the four *Ss* who reported complementary-colored after-images under hypnotic trance were unable to verbalize complementary color relationships in the waking state.

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[10] B. G. Rosenthal and Howard Mele, The validity of hypnotically induced color hallucinations, *J. abnorm. soc. Psychol.*, 47, 1952, 700-704.

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[12] Elsea, *op. cit.*, 69-71.

[13] J. E. Downey, An experiment on getting an after-image from a mental image, *Psychol. Rev.*, 8, 1901, 42-55; J. P. Sutcliffe, "Credulous" and "sceptical" views of hypnotic phenomena: A review of certain evidence and methodology, *Int. J. clin. exp. Hypn.*, 8, 1960, 73-101. Experiments on esthesia, hallucination, and delusion, *J. abnorm. soc. Psychol.*, 62, 1961, 189-200.

Medfield Foundation THEODORE XENOPHON BARBER
Harding, Massachusetts

## IRREGULAR ELECTROSTATIC SURFACE-TEXTURES FOR USE IN THE POINT-SOURCE SHADOW-PROJECTOR*

To make randomly irregular surface-textures for use in the point-source shadow-projector, Gibson devised a method of scattering talcum or a similar powder over a transparent sheet of plexiglas.[1] (See Fig. 1.)

FIG. 1. POWDER-SPRINKLING TEXTURE

FIG. 2. ELECTROSTATIC TEXTURE

This method of preparing randomly textured surfaces has several limitations: (1) The irregularities (in shape, size, distribution, opacity) of the textural elements are not maximized. The granular particles of which the powder is composed tend to be round globules relatively regular in size and shape and of approximately the same opacity. (2) During the course of a study with the texture mounted vertically, as powder shakes down the surface, not only do rivulets occur in the texture which are roughly linear in shape, but the total amount of powder adhering to the surface continuously decreases. (3) Since the powder is sprinkled directly onto the surface by hand, the distribution and amount of powder on the surface depend upon *E*'s arm- and hand-movements, which are not entirely random.

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[14] Bernheim, *op. cit.*, 96.

[15] Sutcliffe, *op. cit.*, 1960, 84.

* This work was supported in part by a contract between Cornell University and the Office of Naval Research, Contract NONR 401 (14). The author wishes to acknowledge the collaboration of Professor Allen Atwell of the Departments of Architecture and Fine Arts at Cornell University in developing the electrostatic method of making the surface-textures described below.

[1] J. J. Gibson, Optical motions and transformations as stimuli for visual perception, *Psychol. Rev.*, 64, 1957, 290 f.; J. J. Gibson and E. J. Gibson, Continuous perspective transformations and the perception of rigid motion, *J. exp. Psychol.*, 54, 1957, 131 f.

To eliminate some of these objections and also to procure a greater degree of irregularity than is possible by the 'powder-sprinkling' method, the following procedure is proposed.

Place an electrostatic charge on a transparent plexiglas surface. If a Flo-master pen with a felt tip is held 1—2 in. above the charged surface, its ink will flow toward the charges on the surface in a pattern determined by the charges.[2] With *E* moving the pen above the surface, the ink will be attracted to loci on the surface that are sometimes 12—15 in. away from the location of the pen. Once the ink strikes the surface, it will be further distributed on the surface in accordance with the nature and strength of the charge prevailing at that time and place and in accordance with the field of charges across the surface. At no time in the process is the pen allowed to touch the surface.

With this technique the electrostatic surface-texture that emerges will have a randomly irregular set of shapes, irregular in size, irregular in distribution, and randomly opaque. (See Fig. 2.) The kind of texture occurring can be varied considerably, depending, apparently, on the relative humidity prevailing when the texture is produced. (The inks seem to flow more freely on relatively dry days.) By using several pens linked together, each with a different colored ink, displays of randomly varying colors can also be produced.

The inks are quick-drying and seem to adhere to the surfaces for indefinite periods of time. Though their rate of fading is unknown, the black inks have not faded perceptibly during a period of four years over which half a dozen surfaces have been inspected. The inks have no adverse effects on the surface of the plastic and are easily removed with the cleaner.

To place a static charge on the plexiglas, the following method is proposed: (1) Place the plexiglas between two supports, its edges resting on the supporting surfaces. (2) Attach a soft woolen buffer to a portable electric drill. (3) Have an assistant lie on his back beneath the plastic surface, continuously operating the buffer in random movements over the bottom face of the surface. At the same time that the charge is being placed on the surface, move a Flo-master pen randomly about above the surface.

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[2] Cushman and Denison Co. manufactures a variety of inks for use in Flo-master pens. Their inks are of varying colors and opacities. A cleaner is also available for thinning the inks or for removing them from the surface.

With respect to the total amount of ink that is distributed on a surface, it should be noted that the flow is not entirely determined by the electrostatic charges. It is true that *E* has little control of how the ink will distribute itself over small regions of the surface. By restricting his arm movements, however, to a particular region of the surface, he can increase the flow of ink to that region at the expense of other regions. Moreover, by controlling the length of time that he will allow the ink to flow, *E* also controls the total amount of ink that will be placed on the surface.

Hunter College of the City of New York HOWARD R. FLOCK

## A COMMENT ON "THE ROLE OF GRAPHEME-PHONEME CORRESPONDENCE IN THE PERCEPTION OF WORDS"

Gibson, Pick, Osser, and Hammond proposed that the consistency of grapheme-phoneme correspondence be considered as a basic variable in perceptual recognition.[1] Specifically, they predicted that a skilled reader would recognize letter-groups (pseudo-words) having a highly predictable English pronunciation faster than letter-groups lacking an obvious English pronunciation. They constructed 25 pairs of pseudo-words. One member of each pair had a predictable letter-to-sound correspondence (pronounceable or *P*-word) and the other member had no such correspondence (unpronounceable or *U*-word). The *P*-word and the *U*-word of any particular pair were composed of the same letters, but in different order. The words were exposed at five durations ranging from 30 to 250 m.sec. Counting the number of correct recognitions for each word indicates that in 21 out of the 25 pairs, the *P*-words were recognized more often than the *U*-words. A Wilcoxon matched-pairs signed-ranks test showed that this difference was significant at better than the 1% level. The authors argued that this result could not be caused by a difference in the familiarity

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[*] The helpful suggestions of J. B. Carroll are gratefully acknowledged. This paper was written while the author was a Research Associate in the Harvard Graduate School of Education.

[1] E. J. Gibson, Anne Pick, Harry Osser, and Marcia Hammond, The role of grapheme-phoneme correspondence in the perception of words, this JOURNAL, 75, 1962, 554-570.

of the units because the same units were used in the two lists, and concluded that it was due to the differences in pronunciability.

The purpose of the present note is to raise the possibility of an alternative interpretation. Although letter-frequency was controlled, the frequencies of bigrams and trigrams were not. Using bigram-frequency tables,[2] we summed the frequencies of the successive bigrams of each of the 50 words. (For a word composed of *n* letters, *n*-1 successive bigrams can be formed.) This analysis reveals that, with three exceptions, in any given pair, the word with a higher bigram-frequency has a higher recognition-score than the word with a lower bigram-frequency. A Wilcoxon test indicated that this difference is significant beyond the 1% level. Thus, bigram-frequency can predict the differences in the recognition-scores with somewhat greater accuracy than pronunciability. It is also clear from the tabulation of the bigram-frequencies that pronunciability and bigram-frequency covary: in 22 out of the 25 pairs, the *P*-words have higher bigram-frequencies than the *U*-words. Bigram-frequency will therefore have to be controlled before pronunciability can be established as an independent variable in recognitive thresholds.

In view of the results of Postman and Conger, who found no relation between trigram-frequency and speed of recognition,[3] it was interesting to check whether trigram-frequency was related to recognition in the experiment of Gibson *et al.* Computing, from published tables,[4] the summed successive trigram-frequencies, one can, indeed, predict the direction of better recognition for 19 out of the 25 pairs. The difference between the units of high trigram-frequency and those of low trigram-frequency is significant beyond the 1% level. The discrepancy between the study of Postman and Conger and that of Gibson *et al.* suggests that for certain classes of verbal units trigram-frequency is a variable in perceptual recognition and for other classes it is not. One difference between the two studies is that the former used three-letter items whereas the latter used longer items. Further research will have to determine whether this is or is not a relevant consideration. In the meantime, here is an additional puzzle created by trigram-frequency.[5]

Harvard University MOSHE ANISFELD

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[2] B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning,* 1960, 333-335, "Total" column.

[3] Leo Postman and Beverly Conger, Verbal habits and the visual recognition of words, *Science,* 119, 1954, 671-673.

[4] Underwood and Schulz, *op. cit.,* 336-369, "Total" column.

[5] Underwood and Schulz, *op. cit.,* see especially 280-286. Contrary to predictions, trigram-frequency did not correlate with rate of learning.

## THE REDUNDANCY OF LETTER-SEQUENCES AND SPACE-INFORMATION

In a recent note, Tulving has commented on the estimate made by Miller, Bruner, and Postman of the redundancy of the letter-sequences they used.[1] This estimate was based on that of Shannon of the redundancy of printed English, which assumes an infinitely long sequence of letters. As Tulving points out, this leads to an over-estimation of the redundancy of higher approximations. Thus, a fourth-order approximation assumes that each letter is influenced by the previous three letters; this obviously cannot be true of the first two letters of any sequence, and these letters must therefore be less redundant than the rest. In recalculating the appropriate redundancy-values, however, Tulving neglects the fact that Shannon's estimates are based on a 27-letter alphabet (26 letters and a space).[2] Consequently, he assumes that the redundancy of the first letter of a sequence is constrained only by its individual frequency of occurrence in English. This is clearly not the case, as is illustrated by the letter E, which has the greatest *over-all* frequency of occurrence in printed English, but which is only the 16th most frequent letter following a space.[3] If account is taken of the space-information, it follows that the estimate of redundancy made by Miller *et al.* is correct for the zero-, first-, and second-order approximations, and is inaccurate only for the fourth-order approximation, where their value should be modified from 43% to 40.5%, rather than 37%, as Tulving suggests.

Medical Research Council, Applied Psychology Unit,
Cambridge, England A. D. BADDELEY

## SIXTIETH ANNUAL MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The sixtieth annual meeting of the Society of Experimental Psychologists was held at the University of California at Berkeley on April 3 and 4, 1964, with Stanford University and the University of California as co-hosts. Frank A. Beach, chairman of the Society for 1963-64, served as Chairman of the meeting.

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[1] Endel Tulving, Familiarity of letter-sequences and tachistoscopic identification, this JOURNAL, 76, 1963, 143-146; G. A. Miller, J. S. Bruner, and Leo Postman, Familiarity of letter sequences and tachistoscopic identification, *J. gen. Psychol.*, 50, 1954, 129-139.

[2] G. E. Shannon, Prediction and entropy of printed English, *Bell Syst. Tech. J.*, 30, 1951, 50-64.

[3] A. D. Baddeley, R. Conrad, and W. E. Thomson, Letter structure of the English language, *Nature*, 186, 1960, 414-416.

Members present were: Beach, Brogden, Brown, J. S., Duncan, Estes, Gagne, Gibson, E. J., Gibson, J. J., Grant, Hilgard, Hirsh, Irwin, Guilford, Kendler, Kennedy, Kimble, Lawrence, Lindsley, Luce, McGill, Marquis, Meyer, Morgan, Mueller, Newman, Riesen, Rosenblith, Sears, Smith, K. U., Solomon, Teuber, Underwood, and Wickens. Georg von Békésy was an invited guest.

G. Robert Grice, Daniel S. Lehrman, Robert A. McCleary, and James Olds were elected to membership in the Society, bringing the total to 64 members and 27 fellows.

Members presented reports of research in progress at scientific sessions held the afternoon of April 3 and on April 4.

The Warren Medal for 1964 was awarded to Benton J. Underwood "for his fundamental contributions to the experimental analysis of verbal learning. His work has combined the painstaking exploration of empirical problems with a deep concern for a rigorous scientific methodology. In a monumental series of experimental investigations he has made major advances in our understanding of the basic processes of acquisition and retention and of the relationship between language habits and learning."

The Society accepted the invitation of Ohio State University to meet in Columbus, Ohio in 1965. Delos D. Wickens was elected Chairman for 1964-65.

Princeton University JOHN L. KENNEDY

# BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

*Experiments in Personality.* Edited by H. J. EYSENCK. New York, The Humanities Press, 1961. Vol. 1, pp. xi, 262; Vol. 2, pp. viii, 333. $16.00.

Much of the value and also the problem of reviewing adequately this two-volume work resides in the breadth of its coverage. Dr. Eysenck and his colleagues chose this form of publication of what might well have been a series of journal articles for several reasons: (1) to help to preserve and to reveal the nature of the program of research underway in psychology at Maudsley Hospital; (2) to allow space for theory and discussion too seldom allowed by current journal policies; and (3) to commemorate the tenth anniversary of the founding of the Institute of Psychiatry and the Department at Maudsley Hospital as part of the University of London. Dr. Eysenck notes also in his introduction that this work is a sequel to his *Dynamics of Anxiety and Hysteria,* "just as *The Scientific Study of Personality* was the sequel to *Dimensions of Personality.*"

The breadth of coverage shows in the contents. In Volume 1, Part I, "Experiments in Psychogenetics" takes 100 pages. Here, P. L. Broadhurst gives a review of previous work on the genetics of behavior in the rat and describes his own experiments on the inheritance of "emotionality," as assessed by a version of Hall's open field. Part II, "Experiments in Pharmacology" takes six chapters totaling 128 pages. The first five chapters report the effects in 24 adult subjects (16 common to all studies) of the depressant drugs (Doriden and meprobamate), commonly used in clinics to reduce tension and anxiety; on serial learning and eyeblink conditioning (R. A. Willett); on such perceptual processes as threshold perimetry and peripheral critical flicker frequence, and body sway (H. C. Holland); on auditory vigilance and pursuit-rotor performance (Emer Treadwell); on the galvanic skin-response to stimulation and on the rate of adaptation to stimulation (Irene Morten); and on simple auditory reaction times and "set" (Irene Morten). A weak systematic framework for these various studies derives from the use of Eysenck's notion that introversion-extraversion (I-E) is based upon the inhibition-excitation balance of the central nervous system and his postulate that depressant drugs increase cortical inhibition, decrease cortical excitation, and thereby produce extraverted behavior; while stimulant drugs decrease cortical inhibition, increase cortical excitation, and thereby yield introverted behavior. In the sixth chapter, the Eysencks (H. J. and S. B. G.) make an inteersting application of that generalized form of Fisher's "discriminant function analysis" (called by Slater, "canonical analysis of discriminance") to the task of classifying drugs in terms of their behavioral effects *per se.*

In Volume 2, Part I is a single long chapter (107 pages) concerning thought disorder in psychotic patients. Part II (140 pages) concerns psychodynamics. There Gordon Claridge describes an attempt to test Eysenck's hypothesis that groups of "dysthymic," hysteric, and normal persons would behave predictably on a variety of tests cutting across perception, attention, and learning, according to their respective positions on the I-E dimension of personality. R. A. Willett reports four investigations of relationships of measures on salivary, spatial, verbal, and eyeblink conditioning to measures of the I-E dimension. H. C. Holland describes the relationships found in a study of perceptual measures of dark adaptation, auditory flutter fusion, spiral after-effect, and two measures of figural after-effect, and of measures of introversion-extraversion. Finally, Eysenck describes a factor analysis of these measures to show that some of them are related to I-E dimension and to neuroticism. Part III (68 pages) concerns some psychiatric methods. Slater relates the geometric model of principal-component factor-analysis to the weights employed in partial regression equations and in discriminant functions, describes what he calls "canonical analysis of discriminance," the variation of Fisher's discriminantive function used by Eysenck to classify drug effects, and reëxamines with this statistical method the Hildebrand data, which have been cited by Eysenck and reanalyzed by Storms, to show the basis for a synthesis among the interpretations which have been given. Volume 2 ends with an epilogue in which Eysenck defends the role of weak theories in psychology.

A critical discussion of a presentation of such scope experiment-by-experiment is out of the question. Moreover, any pointing at various studies undoubtedly reflects more the interests of the pointer-reviewer than any relative merits of the studies themselves, but perhaps such subjectivity can be forgiven.

The fact that the editor has made Broadhurst's work on psychogenetics Part I derives, one gleans, from his conviction that such personality variables as the I-E dimension are basic and genotypically determined. Broadhurst's review is valuable not only for its coverage of the literature but for its critical effort in showing the genetic models utilized in previous studies to be inappropriate. Moreover, his work on "emotionality" in rats with biometric analyses of both selective breeding experiments and a diallel cross with five pure strains is highly worthy of note. Perhaps no more sophisticated discussion of "emotionality" as a construct exists in the literature. It is interesting to note, however, that in attempting to establish the validity of the defecation and activity measures of this construct in a Hall-type open field, Broadhurst is one of a small minority of investigators to find these measures unaffected by such infantile experiences as gentling, handling, and shocking.

The studies of the effects of depressant drugs deserve the attention of anyone interested in the topic. They have the merit of observing the proper controls including that of the double blind, *i.e.* the experimenter who measured performances did not know whether *S* got Doriden, meprobamate, or no drug, and *S* knew only whether he got a drug or not. Eysenck bases his use of "canonical analysis of discriminance" to classify drug effects on the parallels between this problem and that of classifying normals, neurotics, and psychotics. The fact that only one latent root is significant indicates that the behavioral effects of Doriden and meprobamate are indeed of the same order, *i.e.* depressant. Inasmuch as the resulting classification is 70% accurate, the method looks promising.

In the study of thought disorders, Payne and Hewlett report a factor analysis of tests of intellectual and motor speed, tests of concreteness, and tests of "overinclusion" given to a sample of persons including normal people, neurotics of two types, and both depressive and schizophrenic psychotics. It is interesting that a factor of "overinclusion" was found which served to maximize differentiation between depressive and schizophrenic patients.

Anyone concerned with individual differences in eyeblink, salivary, spatial, and verbal conditioning in normal subjects will want to examine Willett's paper in Volume 2. Anyone concerned with normal individual differences in such perceptual functions as dark adaptation, auditory flutter fusion, and in the height, slope, and distance of spiral after-effect will want to see Holland's paper, also in Volume 2.

Eysenck's factor analysis of 34 of the 59 scores resulting from these measures of learning and perceptual functions, and from the personality tests given the same subjects, shows nearly all of these measures to be within loadings of +.25 or —.25 on the first (I-E) and second (neuroticism) factors. The fact that the extraversion questionary loads only 0.55 on the I-E factor means, as Eysenck admits, that it measures only something like 30% of the total I-E variance and puts very severe limits upon its use as a criterion. In consequence, the hypotheses about the relation of the measures of conditioning and perception to introversion-extraversion and neuroticism must almost inevitably be of a low order of correlation. It is therefore surprising to find two of the direct measures of spiral after-effect loading as high as 0.726 and 0.685 in the expected positive direction on the I-E factor. Two measures of spatial conditioning also show the expected positive loadings on this factor but of the low order (0.25 and 0.466). On the other hand, the predictions linking the length of figural after-effect with introversion, linking failure to maintain fixation with the I-E factor rather than the neuroticism factor, linking both verbal and eyeblink conditioning positively with introversion either fail, by being in the wrong direction, or are of a clearly insignificant low order of loading. It is interesting that the measure of salivary conditioning fails to correlate with those of eyeblink and verbal conditioning, and that it loads on neuroticism rather than on the I-E factor because salivation would appear to belong to a different motivational system from that to which the former belong. Salivation belongs to the homeostatic need of hunger, while the eyeblink is defensive, as perhaps verbal learning may also be, but such considerations fall outside the scope of Eysenck's purpose and interests. It is also interesting that Irene Morten found Doriden and meprobamate increasing the level of palmar skin resistance and reducing the number of *GSR*s given to a series of stimuli in one experiment. Such results might be expected both from the mobilization theory of Duffy and Eysenck's notion that depressant drugs potentiate inhibitory processes which also occur during adaptation. Although Morten noted great drops in the level of skin resistance with both the first tone and the first question in a series and the decreasing galvanic reactions with successive tones and questions, and considers these findings comparable with those of other investigators and sees them to have important theoretical implications, she ventures none of these implications but rather echoes Eysenck's complaint that theory in the neuropsychological area is too underdeveloped to suggest broad implications. One wonders.

One must admire the ingenuity and scope of the relationships Eysenck has hypothesized between personality variables and the experimental variables in learning and perceptual functions. Unless one has been brain-washed by the recent vogue for empirical schedules of reinforcement or miniature theories, creativity of such scope brings a thrill. But, as Zysenck is the first to admit in his espousal of the hypothetico-deductive method, hypotheses of broad scope must match an observable—

and hopefully a measureable—reality. Thus, in the light of the current graduate-school folkway of avoiding studies that might merely fail to confirm a weak deduction of some kind, Eysenck's defense of the utility of weak theories in psychology in his epilogue for this work is very well taken. But there is also another side to this question. Eysenck admits that, among the "large number of individual predictions; some of these were verified, some failed to be verified; others had to be modified, and in others certain parameters had to be added to the statement of the hypothesis to incorporate the results." One also tends to glean from the writers of the various contributions to this work that in spite of this evidence of humility in the editor-leader of the group, those led may possibly be less impressed with how much is verified than with how much fails of verification. The low order of most of the factor loadings accepted as evidence, even though they may be expected, leaves one wondering if supposedly basic genotypic differences among persons contribute enough of the variance in behavior to deserve to be the main focus of psychological investigation. One also finds oneself looking for modifications in the theory more basic than "certain parameters had to be added to the statement of the hypothesis to incorporate the results." Occasionally, moreover, even though it is admittedly weak, the theory appears perhaps to be blinding the investigators to the possibility of new syntheses of behavioral variables with social and physiological variables, *vis.* the salivation-study and the study of the effects of depressant drugs on palmar sweating mentioned above. In the light of such considerations, some element of defensiveness may be discerned in Eysenck's epilogue.

But does it matter? Eysenck quotes with approval J. J. Thomson's aphorism that "a theory in science is a policy rather than a creed" (Vol. 2, p. 304). He also quotes and espouses Roby's statement that, "suggestive hypotheses should not be put directly to drudgery but should be entertained for a while as rare and welcome guests" (Vol. 2, p. 309). If these methodological precepts be accepted, and this reviewer does accept them, who can say when Dr. Eysenck should turn the "cold shoulder" to his guest.

University of Illinois — J. McV. HUNT

*Systems and Theories in Psychology.* By MELVIN H. MARX and W. A. HILLIX. New York, McGraw-Hill, 1963. Pp. xvii, 489. $8.95.

*Systems and Theories in Psychology.* By J. P. CHAPLIN and T. S. KRAWIEC. New York, Holt, Rinehart and Winston, 1960. Pp. xv, 473.

Since the Chaplin and Krawiec book appeared nearly four years ago, and has been reviewed elsewhere (*Contemporary Psychology,* 1961, 6, 417-419), emphasis here will be upon the newer Marx and Hillix. It will be worthwhile, however, to bring out certain features which make the two books, despite the nearly identical titles, quite different. They represent divergent developments of the same basic purpose: to describe the historically important system of psychology and to show how they have influenced contemporary thinking. Both sets of authors are convinced that the all-inclusive system is *passé,* and that for a long time to come theories will be much narrower and more specific.

The Chaplin and Krawiec book is intended for the undergraduate, Marx and Hillix for the advanced undergraduate or beginning graduate student. Chaplin and

Krawiec give a good deal of factual material, while Marx and Hillix discuss content strictly within the framework of systems and theories. This difference makes Marx and Hillix seem much more sophisticated; the Chaplin and Krawiec book strikes one as a kind of advanced general psychology, valuable for a student reviewing for a comprehensive examination. But if one wants to know where and how current psychological theorizing is done, the Marx and Hillix will be the better guide.

Both volumes are good jobs of book-making. The type is clear, practically no proofreader's errors were noted, the headings and subheadings are meaningful and attention-demanding. (Two minor errors were noted in Marx and Hillix: Thorndike's *Animal Intelligence* is dated 1882 (instead of 1911), both on p. 117 and in the bibliography, and Underwood (instead of Osgood) is credited with the theory of reciprocal inhibition on p. 468, and, in the bibliography, is made the author of *Method and Theory in Experimental Psychology.*) There is a considerable number of figures, both pictures and line diagrams, in Chaplin and Krawiec, only 12, all graphs, in Marx and Hillix. The latter give a 500-item bibliography at the end of the book, but cite a few references after each chapter, to guide the student who wishes to go further in the particular area; these references are annotated, and extremely well-chosen. Chaplin and Krawiec give references at the ends of chapters; they are quite repetitive, since in most chapters the points of view of Watson, Titchener, *et al.* are summarized. A check on all names beginning with four different letters of the alphabet, given in the indexes, showed that Marx and Hillix mention 102 not mentioned by Chaplin and Krawiec, the latter mention 36 not in Marx and Hillix, and 31 are mentioned by both.

One valuable and unusual feature of Marx and Hillix is the addition of three appendixes which summarize briefly major trends in psychology in three non-American regions: Drever describes European psychology, Brōzek summarizes work in the Soviet Union, and Iwahara describes Oriental psychology (India, China, Japan).

The organizations of the books differ fundamentally. After two introductory chapters dealing with the historical development of psychology as a science, including a very brief synopsis of five systems, Chaplin and Krawiec make conventional psychological processes (sensation, perception, learning, etc.), the core of their discussion. They first show how the problems involved were handled historically, then they describe the position taken by the most outstanding proponent of each of the five systems, and, finally, they discuss the contemporary trends, citing a good deal of the empirical evidence on which the modern positions are based.

Marx and Hillix divide their book into three parts: psychology as science, systems of psychology, and contemporary theories. Part I discusses the nature of science, psychology's place in science, and systems and theories. These chapters are mildly historical in orientation, but the emphasis is on scientific attitude and methodology, operationism, scientific control, etc. The third chapter discusses the significance of a number of important concepts (*e.g.* construct, hypothesis, law), and then gives the criteria for systems, particularly as developed by McGeoch, and for theories, as formulated by Estes. The summary of the changes that have taken place in theorizing in psychology in recent years is logical and especially well-conceived.

The six chapters of Part II describe structuralism, functionalism, associationism, behaviorism, Gestalt psychology, and psychoanalysis. The major characteristics of each system are given, and then there is a section which considers the position the main proponent of the system took with respect to each of McGeoch's six criteria for a system in psychology: definition of psychology, the explicit postulates, the nature of the data used, the position with respect to the mind-body problem, the organization of its data, and its principles of selection. It is admitted that these criteria are rather old-fashioned, but so are the systems. Not all of the systems fit equally well into this framework, of course, but it lends a kind of unity to the discussion and makes comparisons between the systems easier. Dynamic psychology is not discussed, nor is James; the contribution of Woodworth and James are considered briefly in the chapter on functionalism. McDougall appears only antiphonally in response to Watson.

Marx and Hillix are frank to admit a behavioristic bias. Of the 177 pages in Part II, 24% are devoted to behaviorism (chiefly Watsonianism), as compared to 20% for psychoanalysis (including Adler, Jung, etc.), 17% for Gestalt, and 12% or 13% for each of the others. The most unexpected chapter in this section is the one on associationism, admittedly more a principle than a school. Sandwiched between functionalism and behaviorism, it traces the development of associationistic thinking from Aristotle, via the British empiricists to Ebbinghaus, Pavlov, and Thorndike. It is a bit disappointing to find nothing on Herbart, but it is useful to have much of the material out of which *S-R* thinking grew, so well presented.

Part III describes some contemporary theorizing in four areas: (1) varieties of *S-R* theory, concerned exclusively with various kinds of learning theory; (2) varieties of field theory, also with emphasis on learning; (3) varieties of personality theory, covering *S-R*, trait, organismic, neoanalytic, and self theories.

The fourth area is the most original; it deals with engineering and mathematical influences on psychology: the role of computers, data processing, information theory, detection theory, and the relation of mathematics to model building. It is likely that this chapter will be one of the most valuable for students, since it covers material that is not available in other secondary sources, and gives an excellent guide to the original material. Parenthetically it may be remarked that this chapter is difficult reading; a little more redundancy seems called for, since new and difficult concepts are introduced.

One may quibble over some details; for example, it is implied that not until behaviorism hit the boards was psychology objective. Granted that introspection as a method can have a demerit or two counted against it, psychophysical methodology had long since risen to a high level of sophistication, and its objectivity cannot be impugned. In addition, even that most introspective of introspectionists, E. B. Titchener, made a great point of the need for describing the stimulus-situation, for defining terms, etc., which represent a certain kind of objectivity.

It is, of course, unfair to criticize authors for not doing what the reviewer would like, instead of what they set out to do, but there are gaps in Marx and Hillix that are disappointing. One looks in vain for any mention of cognitive dissonance, Helson's adaptation level, the sensory-tonic theory of perception, activation theory, Osgood's mediation theory of language, etc. The title *Systems and Theories* suggests more than learning and personality. It is obviously unfair to Marx and Hillix to suggest that this is all they cover; they discuss some interesting and up-to-date problems, especially in their last chapter, but for the reader who is not much excited by learning or personality, the emphasis is disappointing. True, some of the most effective theorizing has been done in learning, but it is also probably the field that has been most completely reported in secondary sources.

Is another summary of the historical systems needed? Although the framework out of which modern theorizing developed is obviously something to which every budding psychologist should be exposed, it strikes the reviewer that Heidbreder did it all long ago, and did it very well. The chapter on associationism is valuable, since Warren's and Robinson's books have long been out of print, and the material is nowhere summarized briefly and as a unit. It is also true that the particular points emphasized in each chapter are selected so as to lead into the contemporary developments discussed in Part III, and it is certainly convenient to have the background material in the same volume. The question is therefore answered, "Yes." Graduate students are going to be grateful for this presentation.

Tufts University DOROTHEA E. JOHANNSEN

*Concept Learning: An Information Processing Problem*. By EARL B. HUNT. New York, Wiley, 1962. Pp. viii, 286. $7.50.

According to the author, this modest-sized but complex monograph has the primary goal of integrating some knowledge of logic, psychology, and computer technology toward a better understanding of the behavior of human beings faced with conceptual problems. Because of its breadth of purpose, and in part because it is often what it pretends not to be, the volume is difficult to analyze and to review. While Hunt's book is, by admission, not a complete resumé of the psychological literature on concept learning, it does provide fairly exhaustive accounts of certain facets, notably recent experimentation and theorizing growing out of the more or less traditional learning theory. In fact, the book as a whole is as much a survey as it is an integration of information on concept learning. Furthermore, although the author disclaims any attempt to produce an original theoretical orientation toward concept learning, perhaps the most important and original contribution he makes arises out of a systematic reinterpretation of available data and ideas.

The book may not attract a wide audience of psychologists and may be found unappealing even by some with vested interests in the psychological aspects of thinking and concept formation. Certain readers may object that concept learning, as an area of psychological inquiry, is far broader and even somewhat different from the way it is delimited by Hunt and may further question his failure to utilize more fully the voluminous non-experimental literature. There is good reason to suspect, for example, the wisdom of Hunt's restrictions on concepts and conceptual behavior, *i.e.* concepts are (1) general and statable, (2) deterministic (nonprobabalistic) rules which, the learner (3) must be able to transmit to a human being through instruction and without the benefit of examples. These limitations, however, lead to no obvious misinterpretations of available evidence and have the positive feature of reducing the domain of discourse to evidence which is direct and pertinent to later arguments and interpretations of the internal logic or processes of the concept learner. Furthermore, Hunt makes not only his goals but his biases quite explicit even in the Preface, hence a disinterested reader will not spend time needlessly.

Hunt presents an information-processing model which is a direct outgrowth of a considerable research program initiated by Hovland at Yale University over a decade ago and carried on after his death by his students and associates. While answers to many questions about an adequate simulation of human performance in conceptual problems came out of this research, the model as presented is by no means complete. It does not adequately reproduce all characteristics of performance in these tasks, fails to generate results similar to those obtained in many studies of concept learning, and is vague to the point of permitting several alternative ways of performing essentially the same operations. Many of its predictions remain to be checked by experiment and it is unquestionably open to the common objection of skeptics that such models have yet to produce many testable theorems. For these reasons, some readers may argue that the book is premature. One cannot escape the feeling, in reading Hunt's discussion, that it might have been better to wait two or three years to publish this monograph, on the assumption that more definitive data are forthcoming. Postponements, however, have a way of perpetuating themselves, and since there is more to this book than a tentative model, there seems to this reviewer to be sufficient reason for its publication at this time. Over-all, the author has undertaken a prodigious task, and the success he has, although not complete, is truly remarkable.

The book may be divided into three parts, corresponding roughly to the areas from which the author draws knowledge. Part I (Chapters 1 and 2) begins with some general statements about the nature of concepts and then elaborates a notational system, more rigorous, complete and unambiguous than any other known to this reviewer, for describing the elements of a conceptual problem. The system is neither a theory nor an orientation for research, although its lucidity often makes formerly vague (empirical) problems, *e.g.* the differences among types of concepts—conjunctive, disjunctive, relational—remarkably clear to a careful reader. The system is purely descriptive, consisting in the last analysis of a language for discussing the overt structure, on both the stimulus- and response-ends, of conceptual tasks. The system borrows moderately heavily from symbolic logic and set-theory. Its presentation may impress some readers as being unnecessarily detailed, especially in view of the fact that some of it finds no use in subsequent discussions. The value of the system, however, would have been disproportionately reduced if it was not presented in full.

Part II (chapters 3, 4, 5, and 6) is mainly a review of some relevant psychological theory and research. This is as thorough a resumé of the recent experimental evidence as exists and demonstrates the author's intimate contact with the efforts of his contemporaries. It begins in Chapter 3 with data growing out of S-R analyses of conceptual behavior and progresses systematically toward process-oriented investigations of Bruner and others, which are closer to and more directly implicated in computer models of behavior to be discusssed in Part III. Interpretations of these data by the author make quite clear his belief that a scientific description of concept learning will require a different set of laws, possibly underivable from an understanding of simpler processes no matter how closely related in appearance. Whereas the legitimate inquiry of conventional learning theory, according to the author, concerns the principles governing the growth of response strength, complex processes such as concept learning are problems in the utilization of response systems, called information processing units (or strategies), which are largely already formed. He cites well-documented learning to learn phenomena as possibly sufficient reason for abandoning traditional learning theories in complex situations. To this reviewer the arguments are not compelling. Although the mechanisms of learning sets are far from fully understood in general, there are several interpretations which do not violate fundamental assumptions of S-R theory and which do provide relatively accurate descriptions of learning set data. The proposed orientation of the author, moreover, does not at this time produce any better understanding of these data.

Chapter 4 is unhappily vague, dealing with a ticklish distinction between the conceptual and perceptual aspects of learning tasks and coming to no firm conclusion regarding the relevance of cue learning experiments, usually lumped under the general heading of concept formation, to the present inquiry. There appears to be a misunderstanding of the procedures of these studies, which makes Hunt's analysis unconvincing. He attributes the apparent difficulty of cue learning problems to a rapid rate of stimulus-presentation (p. 117, and elsewhere) and the resulting inability of subjects to make accurate and complete identifications of stimulus-patterns. But, in fact, the subject may, in any of the cited experiments, inspect a stimulus-object for as long as he wishes before making a response. It is more reasonable that the difficulty of these problems results from the necessity for the subject to remember (some of) the details of a stimulus, once it has been removed, than to any factor of pacing. Lack of clarity regarding procedural details in the original reports of these experiments may be in part responsible for this misunderstanding.

Part III (Chapters 7, 8, and 9) is concerned with computer simulation and its contribution to an understanding of conceptual behavior. Although weak in the sense that relationships between this section and the preceding two are not altogether clear, it was found by this reviewer, who admits to considerable naïveté, to be most illuminating and original. The entire section, incidently, is characterized by an objective, cautious and, therefore, refreshing attitude toward the usefulness of "computer models." Although obviously convinced of their importance, Hunt is continually alert to parallel arguments and cogent counter-arguments that may be generated from competing positions.

The book is not free of technical errors: There are several inconsistencies in the references and between reference and text citations; there is a fair number of typographical errors which, as is common, increase in frequency as the end draws near; and there is one blatant inversion of experimental results (p. 174, regarding the usefulness of positive and negative instances in the classic Hovland-Weiss study). These are no more than par, however, and should give the reasonably alert reader no difficulty.

In organizing and reviewing recent relevant material from psychology, logic and computers, Hunt has performed an exceptionally difficult task with care and skill and, in the course, accomplished a creditable contribution to the literature of experimental psychology. His product contains a host of new, solid ideas for future research and theorizing, which merit careful inspection, consideration, and investigation by scientists interested in the conceptual behavior of human beings.

University of Colorado LYLE E. BOURNE, JR.

*The Oedipus Complex: Cross-Cultural Evidence.* By WILLIAM N. STEPHENS. New York, The Free Press of Glencoe, 1962. Pp. x, 273. $5.00.

Stephens' interesting cross-cultural study follows a model familiar to readers of Whiting and Child's *Child Training and Personality.* On the basis of psychoanalytic theory he predicts correlations between certain customs of primitive peoples and checks his predictions through chi-squared tests of association between coder's ratings of culture traits for large numbers of societies (50 to 70 in some principal tests).

The author's hypothesis, a version of Freud's theory of the Oedipus Complex, is that: "Young boys, at least under optimal conditions (long post partum sex taboo), become sexually attracted to their mothers. This generates lasting sexual fears and avoidances. At least in one instance (castration anxiety) these fears are mediated by unconscious fantasy" (p. 182). This hypothesis leads Stephens to divide his theoretical argument and his data into two parts: variables interpreted as "causing," or at least intensifying, the incestuous mother-son tie, and a second set of "effects" of the ensuing Oedipal relation. He then demonstrates that ratings of the customs seen as being antecedent show significant associations with the second set seen as consequent.

There are three antecedent conditions, although the study only attempts systematic examination of the first: (1) the presence of a long (over one year) post partum sex taboo; (2) polygyny; and (3) the custom of the mother and her children living separately from the father. Stephens argues that these conditions must result in increased sexual deprivation for the mother and assumes that she will therefore be more seductive toward her sons, bringing about a confusion of the boy's concept of "mother" and of "sex," with a resulting generation of sexual fears and avoidances on his part. Stephens postulates that the boy's sexual fears and avoidances will have long lasting and widely ramifying effects on other aspects of the society's culture.

Stephens' hypothesis and his attempt to test it generates a large number of empirical findings which will interest many readers regardless of their views on the Oedipus concept and Stephens' conviction that the data strongly support the theory. His first set of findings include significant associations between the presence of a long post partum sex taboo and the following: presence of initiations for adolescent boys; severity of mother-in-law avoidance; severity of son's-wife avoidance; severity of brother-sister avoidance; and extensiveness of menstrual taboos. In addition to these predicted associations, the study turned up unpredicted associations between length of the post partum sex taboo and: ratings of intensity of sex anxiety (a composite score derived from six subscores); the custom of boys moving out of the house at adolescence; breasts not being considered to be sexual stimuli; and the presence of sorcery as an explanation of illness.

The second set of empirical findings are associations of the extensiveness of menstrual taboos with the following: severity of punishment for masturbation; over-all severity of sex training; and the presence of themes of physical injury in folk tales.

Stephens' acceptance of the Roheim interpretation of menstrual taboos as representing unconscious fears of castration led him to develop a composite index of "castration anxiety" (not part of his original prediction) based on 10 variables (duration of post partum sex taboo, extent to which the mother shares her child-care with others, severity of punishment for masturbation, over-all severity of sex training, severity of aggression training, the importance of physical punishment in discipline, general pressure for obedience in the culture, severity of punishment for disobedience, strictness of father's obedience demands, and whether or not the father is the main disciplinarian). Although only four of the elements making up the composite "castration anxiety" score have significant associations with severity of menstrual taboos, the composite score p-value for the chi-squared test is at the 0.000001 level of probability! These data have inherent limitations but it is likely that the general type of relationship Stephens reports among this complex of primitive customs associated with sex will be confirmed. At the least Stephens has shown that a surprising variety of cultural traits are related to one another in ways that go beyond what the layman would have anticipated.

While Stephens' findings are of great interest, and while the Oedipus hypothesis and his deductions from it undoubtedly stimulated the production of the findings, the relation between the data and the theoretical interpretations are not compelling. The division into "cause" and "effect" variables, the postulation that the antecedent ones create their effects by intensifying incest wishes, which in turn raise the level of sex anxiety, and the assumption that the resulting unconscious sex fears manifest themselves in the "consequent" variables are not necessary to explain the associations. They are all derivable from a simpler assumption that some societies are characterized by pervasive sex fears while others are not. Stephens' findings spell out some of the ramifications of these sexual fears. Of course this alternative hypothesis leaves unexplained *why* one society develops such sex fears while another does not. But Stephens' use of the Oedipus theory does not explain this either. For instance, the incest theory leads him to select the post partum sex taboo as a "cause" and "sex anxiety" as a result. But if we examine his "sex anxiety" composite score we find that one of its six elements is the duration of sex taboo during pregnancy. In both the post partum sex taboo and the pregnancy sex taboo the wife is deprived of normal sexual gratification. If Stephens argues that one can work to intensify incest interest he must apply the same argument to the other. One taboo cannot be a part of a "causal" syndrome and another virtually identical one part of an "effect" syndrome.

Freudian theory does not help as much as Stephens believes on specific points. For instance it leads him to interpret menstrual taboos as displaced fears of castration due to incest wishes. He goes to some trouble to show that the menstrual taboo is specific to the male-female relation and sees this specificity as evidence consistent with male castration fear. But Freudian theory does not leave out women in the castration concept. They could also be expected to be painfully reminded of the fact of their own castration by menstrual blood and to use avoidance mechanisms to deny castration. One can patch matters up by assuming that these are "mature" women who have given up childish denial of castration. One cannot be selective about the matter, however. "Mature" men according to Freud, do not merely repress the Oedipus Complex, they "destroy it in the id." Hence, except for neurotics, there should be no such continuing derivatives as menstrual phobias among 'normal' men. Stephens deals with such potential embarrassments by excluding girls from his personal version of Freud's theory.

This loose relation between fact and theory is not, in the reviewer's opinion, a critical fault in studies of this type at such an early point in the development of quantative, hypothetico-deductive research in anthropology. The theory leads to interesting examinations of facts and turns up associations that would not otherwise have been explored. In time other theories will be advanced to explain the same facts and these new theories will lead to further findings. The significant event here is the birth of science within a discipline that is beginning to pass beyond its first descriptive phases. Stephens has advanced anthropology as a science. He has also made fruitful suggestions about the probable origins of the Oedipus Complex, a matter that Freud largely left to "racial inheritance."

Princeton University PETER MADISON

*Psychology of Motivation*. By JOHN F. HALL. Chicago, J. B. Lippincott, 1961. Pp. x, 382. $6.50.

One characteristic that distinguishes Hall's book from much of what has been written on the psychology of motivation is that he approaches his task simply and directly. There is no didactic introduction on the nature and glories of psychological theory, nor is there a passionate polemic on the impossibility of such a theory. Instead, Hall begins by describing the kinds of facts that have led psychologists to do those sorts of experiments which, for lack of a better name, have been called experiments on motivation. As he points out, if we are to predict even the simplest sort of behavior we must first know not only what patterns of stimulation the animal is receiving from his environment, what responses this animal has made to such stimulus patterns in the past, and what consequences have followed these responses, but we must also know at the time the animal is tested what sorts of stimulation and what sorts of activity have been withheld from the animal prior to this test, and how long the animal has been so deprived. Differences in behavior that reflect the effects of differing conditions of deprivation are called "motivational differences," and it is commonly assumed that deprivations of food, water, receptive females, exercise wheels, exploratory arenas, etc., arouse some internal processes in the animal that modify the reception of stimulation from the environment, as well as influence the execution of responses that are associated with such stimulation. We have as yet no exact knowledge of these various sorts of internal processes, and one primary task of the psychology of motivation is to discover what we can about these processes from a study of how different conditions of deprivation influence different sorts of behavior. Because as yet we know nothing exact about these processes, it is pointless to search for any exact meaning for expressions like "the hunger drive," "the thirst drive," "the sex drive," and so on. Such expressions have only one use—they serve to signify possible answers to questions which we hope to learn how to ask. It follows from such a view that in our present ignorance it is pointless to argue about how to define such abstract expressions. For the present we should rather concentrate on discovering what we can about the various effects of different sorts of deprivations upon: (1) the equilibrium of those internal processes known to be vital to the organism; (2) the level of the animal's spontaneous overt activity; (3) the rate and probability of various consummatory responses; (4) the rate and probability of responses that have in the animal's past been instrumental in producing the sorts of stimulation or activity of which the animal is now deprived; and (5) the selection and classification of external stimulus input.

This is how Hall strips the study of motivation to its empirical roots. The first third of his book describes those deprivations that are believed to have some effect upon the organism, and the remainder of the book tries to describe how each of these various deprivations affects each of the five sorts of behavior we have just distinguished. Although his exposition is based on more than 650 separate experimental papers, few conclusions about the effects of deprivation on behavior can be reached. The outcomes of apparently comparable experiments are often contradictory and because of this one soon learns that the more research is devoted to some question the more likely are we to turn up contradictory facts, and the less certain we become of any answer to the question.

After reviewing over 50 experimental studies of the effect of deprivation on spontaneous behavior, Hall has only one conclusion to offer: "Whether or not activity occurs depends upon the measuring instrument." (p. 133). As we go on through the book we discover that whether or not deprivation increases eating depends upon what study is cited. The same is true of whether deprivation increases the probability of an instrumental response; whether the addition of an irrelevant deprivation increases the rate of an instrumental response; whether increasing size of the reward increases either the rate or probability of response; whether varied delay of reward increases the resistance to extinction; whether frustration produces aggression, regression, or fixation; whether food deprivation increases the rate or probability of verbal food responses; and whether the apparent size of an object increases with its apparent value. The experiments performed to answer each of these questions were sufficiently comparable that a conclusion could have been reached if the outcomes of these experiments had agreed. In contrast, the various experiments made to discover the effects of the so-called "ego-involved variables" or of the perceptual effects of promises of reward or threats of punishment are not even broadly comparable. This saves us the trouble of analysis because no conclusions could be drawn from these experiments no matter what might be their outcomes. In his last chapter, Hall reviews the recent work on the physiological conditions that control the onset and shutoff of eating and drinking. Again we discover how difficult it is to get consistent outcomes from experiments that are closely comparable.

Hall in his preface:

> "Since 1954, I have had the task of teaching a course in motivation for advanced undergraduate and graduate students, and for most of these years the only available text in the field was twenty years old. This lack of an up-to-date text was sufficiently annoying that within a few years, I decided to try to remedy this deficiency. The present text represents the culmination of these efforts. . . . In examining that which has been frequently described as motivational material, one cannot help but be impressed at the breadth of the subject matter and appalled at the confusion and lack of organization. As a result, a *primary objective* in writing has been to provide an *organization* which can incorporate much of the material traditionally found in this area. (p.v).

In the reviewer's opinion, Hall provided just the sort of organization that his material needed; the book, however, suffers from one serious lack. There is no final chapter in which the author tells us what can be concluded from his survey about the present status of the psychology of motivation. As I mused upon what such a chapter might say, I saw that here was a task that should appeal to the fancies of the personality psychologists. From an imaginary analysis of the various sorts of concluding chapters that I pictured my fellow psychologists might write, I saw that here was an ideally, natural, projective technique for distinguishing the four basic sorts of psychologists, the sanguine, the phlegmatic, the melancholic, and the choleric. On a sudden impulse toward self-analysis I began to write the sort of chapter I thought was called for. From the very opening sentence of my chapter

I saw that my temperament was incurably choleric. I began my chapter as follows: "When over 650 experimental reports yield no more certain information than this about the effects of deprivation upon behavior, we are justified in suspecting that something is seriously wrong."

University of California **BENBOW F. RITCHIE**

*Explorations in Cognitive Dissonance.* By JACK W. BREHM and A. R. COHEN, New York, Wiley and Sons, 1962. Pp. xiv, 334. $7.95.

Most of my friends who are acquainted with the current research literature agree that dissonance-theory is the "biggest thing" to hit social psychology in the past ten years, but they disagree on whether this is to the glory of dissonance-theory or the shame of social psychology. That the theory is no *succès d'estime* is indicated by the flurry of recent attacks: a lengthy one by the Chapanis' in the *Psychological Bulletin,* and others by Mowrer and by Bernice Lott in *Psychological Reports* and the *J. abnorm. soc. Psych.,* respectively. In contrast, it seems a definite *succès populaire: e.g.* in the issue of the *J. abnorm. soc. Psychol.* that carried Lott's criticism, 8 of the 17 articles dealt with dissonance-theory (some unkindly). One suspects that the acclaim of the many has played some part in drawing the attacks of the few. Yet previously, advocates and opponents passed like ships in the night: supporters hailing dissonance-theory as one of the few psychological approaches currently yielding non-obvious predictions; critics complaining that the theory is unclear and that its experimental foundations are faulty.

To this noisy and confused field, the Brehm and Cohen book comes as a beacon of partisan sanity, supplementing the oirginal 1957 Festinger book as the obvious rallying-point of the faithful and point of attack for the opposition. These two authors stand out as Festinger's most astute interpreters, as truly Stakhanovite producers of dissonance-research, and yet, as independent thinkers in expanding the theory (they here add the two variables of volition and commitment to Festinger's original variables of discrepancy and importance) and in recognizing the shortcomings of current dissonance-research, including their own. The book is definitely a case for the defense, but it gives fair consideration to most of the criticisms that have ever been directed against dissonance-theory.

Viewed as a work of scholarship, the book is truly commendable. Some idea of the currency of their review can be gained from the fact that among the 34 items on the first two pages of their reference list, 20 are from the 1960s and 8 were still "in press" when the book appeared. (All eight have since appeared.) Some dissonance-work is ignored, *e.g.* the "Prophecy Fails" studies and the Lawrence and Festinger work on rats. A few lines of related work by researchers from other traditions are neglected: *e.g.* Steiner's work on alternative modes of restoring balance and on personality-interactions; some work relevant to their discussions of the salience problem and the problem of indirect effects; the work of Abelson relevant to their discussion of defense-mechanisms. But in general, the coverage by these authors is excellent. On the other hand, the organization of these early review chapters is rather tedious and pedestrian.

To what extent have the authors confronted the criticisms directed against dissonance-theory? Their score here is high but not perfect. They point out at least one clear disconfirmation of a dissonance hypothesis, the selective-avoidance prediction (pp. 42, 48, 96, etc.). They admit many of their manipulations are complex and subject to alternative explanations (pp. 71, 136, 155, etc.) but argue tellingly that the *corpus* of work yields a convergence-of-probability argument for dissonance-theory as opposed to an untidy hodgepodge of alternative explanations. They admit that the theory suggests little about preferences among alternative modes of dissonance-reduction. They face up to the repeated criticisms that dissonance-studies have frequently been marred by the problems of small *N*s and of *S*s' self-selection and internal analyses involving elimination of *S*s, (pp. 35, 47, 121). A number of the statistical infractions for which they have been taken to task in the past are found again in the studies they report here (pp. 91, 120, 141) for the first time, but perhaps these authors are being criticized for infractions that are allowed to go unnoticed in less successful and less visible workers. Nor is it clear that more appropriate statistics would notably weaken their case. Yet in the last analysis, it strikes us as truly sad that the Festingerians, who formerly set the highest statistical standards for social psychological research, should now be unbothered by such infractions.

The authors present a number of interesting contrasts between dissonance-theory and other related conceptualizations. They put forward the interesting but debatable proposition that dissonance differs from other balance theories (Heider, Newcomb, Osgood, etc.) in that the latter posit an *ipso facto* motivation to reduce incongruencies, whereas dissonance posits such a tendency only to the extent that inconsistency frustrates another motivation. Dissonance-theory is also contrasted with various formulations of the "Yale" attitude-change group (Hovland, Janis, Kelman, Sherif, etc.) such as assimilation-contrast theory, improvision-theory, and conflict-theory. For example, dissonance and conflict theories are contrasted in terms of whether re-evaluation occurs after or before the choice, a distinction which this reviewer regards as rather uninteresting. In their contrasting of dissonance and psychoanalytic theory predictions, the authors became equivocal regarding whether dissonance posits an element of consciousness. On the whole, however, their confrontations of theories are impressive intellectual exercises and in a number of cases are heuristically provocative.

The book is a valuable convenience as a summary of the current theoretical and empirical status of dissonance-theory. Indeed it is indispensible for this purpose as many recent studies by the authors are reported nowhere else. While accentuating the positive, it documents the criticisms, and if it resolves few controversies, it clarifies many. A close reading would provoke and improve research by both sympathizers and opponents of dissonance-theory.

Columbia University

WILLIAM J. MCGUIRE

*Experimental Foundations of Clinical Psychology*. Edited by ARTHUR J. BACHRACH. New York, Basic Books, Inc., 1962. Pp. xii, 629. $10.00.

The dominant impression left by this edited collection of papers is best summed up by Jimmy Durante's comic lament, "Everybody wants to get inta de act." That a line of vaudevillians should be crowding in the wings to do their turn is something of a reverse twist from the time when the experimentalist aspired to the Met, meanwhile consigning clinical psychology's act to the less discriminating tastes of the Hippodrome. Now it seems that quite a number feel that vaudeville has somehow become respectable and are bringing forth their talents to contribute to its revival. If respectability killed vaudeville, one might offer a prayer for clinical psychology; but the more immediate task is to see what the new act looks like.

In truth, while there are some similarities to the clinical psychology we all knew, what has been added seems to be half the highbrow sophistication of the Met and half a watered down version about as compelling as Nelson Eddy singing *Trees*. The latter apparently is presented in the belief that the audience of old is too untutored and derives too much pleasure from the old fare to appreciate more than pop culture. There is more than a hint of disdain for clinical psychology in the new playbill, as such as compromise attests.

What is clinical psychology, anyway? In this volume, at least, nobody seems to have given much thought to an answer. Apart from the obvious—that clinical psychology is characterized by interests in abnormal behavior, psychotherapy, and diagnostic techniques and is anchored in personality theory—the question is not confronted. In a compendium of review articles such as this, the problems of articulation, continuity, and the preservation of a consistent theme are great. They are maximized here. To title a book, *Experimental Foundations of Clinical Psychology* would not unreasonably lead one to expect some delimitation or definition of the discipline whose empirical base is being offered. By default, however, clinical psychology becomes whatever the author of a chapter wishes it to be. Nor is it at all clear what is meant by "experimental foundations." Occasionally, "experimental" seems to mean simply research; elsewhere, it is more exactly reserved to characterize manipulative and controlled investigation.

In the first chapter, Watson does draw the distinction between the experimental and psychometric traditions, but his emphasis is not maintained. There is no attempt to consider the theoretical intricacies of the R-R paradigm as it is employed in personality research and the assessment of construct validity.

The book is loosely organized into four major parts: general and theoretical, learning and communication, psychophysiology, and behavioral modification. The first part begins with Watson's thoughtful historical analysis of clinical psychology's experimental ancestry. Hunt and Jones then approach the thorny problems of scaling clinical judgment by means of a psychophysical model, followed by a rather elementary critique of personality assessment by Berg and Adams. This much is familiar—in the last instance too familiar, perhaps, to warrant inclusion in this volume. A review of more general problems of personality theory and methodology is conspicuously absent.

Beginning with the second section, a strongly Skinnerian emphasis makes its appearance. Eschewing theory, by and large, and focusing on the control of behavior by its consequences, chapters by Hefferline, Sidman, Goldiamond, and Greenspoon among others, apply a Skinnerian analysis and methodology to a range of problems more or less within the domain of personality and clinical psychology—but which behaviors are controlled by what reinforcements remains an empirical question for which the hunch, not theory, is supposed to provide an answer. The role of cognitive variables—expectancies, for example—fails to receive consideration.

Levine's chapter on infantile experience summarizes some excellent animal research; the analogy to the early human experience so compelling to the clinician is, if presently difficult to draw, heuristic for future work with both infra- and human-organisms. In the remainder of the book, the chapters of Matarazzo, Wolpe, and Strupp, which appear in the section on behavior modification, deal with issues of obvious psychotherapeutic relevance and should prove to be useful summaries of research. In the preceding part (Psychophysiology), Malmo's analysis of activation and his attempt to account for symptoms of tension should promote some theoretical speculation by clinicians about the effects of noncognitively mediated arousal.

To summarize, much of this book is really on the periphery of clinical psychology and personality theory. Moreover, many of the chapters reflect an outright aversion to theory; the benefits of what is proposed as a substitute are, at this juncture, uncertain. *Experimental Foundations of Clinical Psychology* is, then, a collection of papers of varying interest: some are provocative, while others verge on irrelevance. The complexities of behavior and the intriguing implications of personality theories seem to be largely sacrificed in favor of experimental rigor—and, indeed, a very particular kind of rigor. To paraphrase an old saw, it seems a shame to throw out the whole act with the old buck and wing.

The Ohio State University

DOUGLAS P. CROWNE

*Somatosensory Changes after Penetrating Brain Wounds in Man.* By JOSEPHINE SEMMES, SIDNEY WEINSTEIN, LILA GHENT, and HANS-LUKAS TEUBER. Published For the Commonwealth Fund by Harvard University Press, Cambridge. 1960. pp. xiii + 91, $4.00.

It is now more than fifty years since Henry Head and Gordon Holmes first applied the psychophysical methods, at that time recently imported from Germany, to the clinical study of sensory defects. As a result of their most important work, the concept of "cortical sensory loss" acquired something approaching quantitative precision. Dr. Lukas Teuber and his co-workers have now carried the process a stage further. To Head's rather simple psychophysical procedures, they have added modern experimental design, normative control, and statistical significance tests. The outcome is a contribution of major importance to the growth of scientific neurology.

The authors carried out systematic sensory testing in 124 patients with penetrating wounds of the brain, all veterans of World War II or the Korean campaign. In 88 patients the injury was unilateral, equally divided between left and right hemispheres. Localization was achieved principally on the basis of radiological evidence. The tests used were concerned with pressure sensitivity, two-point discrimination, tactile localization and appreciation of passive movement, and were standardized upon a carefully matched group of healthy subjects. Details of experimental design and procedure are fully and clearly described.

The results, although not world-shaking, are of very considerable interest and importance. Contrary to expectation, the authors found evidence of a striking difference between left-sided and right-sided brain wounds in regard to cortical sensory loss. Whereas in cases of left hemisphere lesion, the highest incidence of contralateral sensory defect was found with postcentral or posterior parietal penetration, in cases of right hemisphere lesion the highest incidence occurred with pre- and post-central penetration. Further, while sensory defect in the right hand was found to be closely related to injury of the left somatosensory region (in accordance with classical expectation), sensory defect in the left hand was found not only with injury to the right somatosensory region but also with wounds located elsewhere in the right hemisphere or even in the left (ipsilateral) hemisphere.

These findings are puzzling. If one may argue from 'defect' to 'function,' they strongly suggest that the organization of cortical somatosensory function is not precisely identical in the two hemispheres.

Further, an interesting difference between the pattern of sensory defect on the two sides of the body was disclosed. In the case of the right hand, all scores on the sensory tests were found to be positively inter-correlated, suggesting a unitary form of disability; but in the case of the left hand, analysis of the correlations suggested that there are two types of sensory impairment, one relating principally to pressure sensitivity and maximal with lesions of the right sensori-motor region, and the other related to tactile localization and apparently uncorrelated with the locus or even the laterality of the lesion. Although interpretation is difficult, this finding might suggest that integration of somatosensory function is more complete in the case of the "preferred" hand. Further study, particularly of left-handed patients with focal brain wounds, is obviously required.

Taken together with other recent evidence bearing on cerebral asymmetry, the results of this study suggest that psychological defects arising from lesions of the left hemisphere have a more restricted anatomical basis than do comparable defects arising from lesions of the right hemisphere. It is as if function were represented more focally on the left, more diffusely on the right. This has obvious implications for cerebral dominance and related concepts of asymmetry of function in the cerebral hemispheres.

This work is so admirable that criticism might seem ungrateful. The present reviewer would, however, have liked to have seen rather more attention given to fluctuation and inconstancy of sensory thresholds, so well brought out by Head and Holmes and more recently given prominence by Bay in his concept of *Funktionswandlung*. The effects of practice, too, are little discussed, though it is well-known that these may be considerable, at all events in healthy subjects. Nor do the phenomena of extinction find mention.

The work of Dr. Teuber and his colleagues should do much to impress upon neurologists the need for up-to-date methodology in the examination of sensory defects. It should also do much to convince psychologists that study of the after-effects of brain injury is no mere "clinical" diversion but an important area of modern experimental psychology.

University of Cambridge, England O. L. ZANGWILL

*The Crisis in Religion and Psychiatry.* By O. HOBART MOWRER. New York. Van Nostrand Co., 1961. Pp. vii, 264. $1.95.

This book consists of reprint of thirteen lectures and papers previously published between 1957-1960, some of them intended for theological audiences. Although very different from Mowrer's more familiar writings, and not primarily intended for psychologists, this book does contain a revision and restatement of his views on neurosis and psychotherapy, and raises, but does not solve, some more fundamental problems of very great importance.

The revised theory of mental disorder is as follows: guilt is due to "real" sins (*e.g.* incest); this guilt leads to withdrawal from society to avoid revealing it (schizophrenia), self-punishment (depression), and so on; the breakdown is due to a revulsion by the super-ego at the guilt, and is itself an attempt at cure.

The central point, which is frequently reiterated, is that therapy should aim at removing guilt, not by dissolving the super-ego but by improving behavior. Specifically, the author suggests that there should be public confession, followed by restoration to fellowship, an improved way of life, restitution, and meaningful sacrifice. He quotes the methods of Alcoholics Anonymous and the Salvation Army with approval. He is now convinced that psychoanalysis is worse than useless, and for him the *crisis* in psychiatry consists of a very widespread demand for therapy in the United States, together with a failure of psychoanalysis to cure people.

The crisis in religion consists of the abandonment by the churches of earlier methods of dealing with the mentally sick, together with an acceptance of Freudian ideas about therapy. The author argues that it is better to regard guilt as due to sin than to look at it as unreal, as the analysts do. Acceptance of the Freudian ethic means a loss of moral purpose and children reared without character.

Mowrer makes a number of theological suggestions: that Hell should be interpreted as a state of mental disorder caused, as we have seen, by sin. He argues that the Holy Spirit is the most important aspect of religion, and that it is manifested through the unconscious—which is good and not evil. The doctrines of "cheap grace"—by indulgences, faith or psychoanalysis—must be abandoned for a doctrine of salvation by works, *i.e.* a changed way of life. There is a need for a further Protestant reformation, proclaiming the sanctity of the common life, and announcing that salvation can be obtained without professional help.

There are a number of serious difficulties with this approach. (1) When it is said that the Holy Spirit is in the unconscious, that Hell equals mental disorder, or that the psychological account fails to include moral questions, there are logical confusions brought about by using two language systems in the same sentence.

(2) The level of empirical evidence contrasts sharply with that in Mowrer's technical works. It is alleged that Catholics enjoy good mental health, that incest leads to mental disorder, etc., etc., with no evidence more than hearsay and occasional case-studies.

(3) In particular no real evidence is provided for the idea that guilt and neurosis are better dealt with by changing behavior than by weakening the super-ego. It seems very likely that there are some people for whom each approach works, and others for whom neither does.

(4) The account of morals and guilt is very confusing: guilt is said to be the result of real sin, yet it is virtually admitted that there is no guilt unless parents have implanted a super-ego—which rather destroys the objective account of morals which is assumed. Man may be "a moral and social being," but the actual character of the super-ego is determined by learning experiences.

(5) It is assumed that mental disorders are entirely environmental in their aetiology: nothing is said about genetic factors in causation or about physiological methods of therapy.

(6) The attack on psychoanalysis misfires, since some of it only applies to unintelligent applications of it (*e.g.* bringing up children without character) while some of it applies equally to all psychology (*e.g.* the belief that all behavior is lawful and predictable).

Nevertheless there are parts of this account which strike the reviewer as very important. The new approach to therapy certainly seems worthy of investigation. There are, furthermore, great difficulties in having one language system for psychology and another one for religion and morals—unless the neurotic delinquent can forget about psychological predictability and adopt the outlook of free-will and responsibility he is less likely to be cured.

Oxford University MICHAEL ARGYLE

*The Acquaintance Process.* By THEODORE M. NEWCOMB. New York, Holt, Rinehart and Winston. 1961. Pp. xv, 303. $7.50.

This book reports fully an extensive research project designed to test hypotheses, deriving mainly from Heider's concept of "balance" in interpersonal relations, in which Newcomb has been interested for several years.

The central theory is that (1) person A's attitude or attraction to person B, (2) A's attitude toward X, any object of mutual interest to both A and B, and (3) A's cognition of B's attitude toward X form a true system in the sense that change in any one of these elements forces change in another element such that the balance of the system tends to be preserved. (For example, A's attitudes are in balance if he favors X, is attracted to B, and perceives B as sharing his liking of X. If any one of these attitudes or the perception of B's attitude changes, the system becomes self-inconsistent.)

Such a system is located entirely within A himself, since it is composed entirely of his attitudes and cognitions. An analogous system, collective rather than individual, consists of both A's and B's attitudes toward each other, their attitudes toward X, and the attitudes toward X attributed by A and B to each other.

Since a growing acquaintance between A and B should give occasion for changes in both personal and collective systems, the reported research followed the evolution of such systems among a group of entering undergraduates at the University of Michigan who, for experimental purposes, were assigned to a single dormitory in which they could be continuously tested and observed; and the study was repeated on a similar group during a second year.

The research confirmed the general hypothesis. With increasing length of acquaintanceship, the subjects' early impressions of each other's attitudes on issues, or toward persons, important to them generally changed in the direction of greater accuracy. Correspondingly, and as required by balance theory, early personal attractions shifted and solidified in directions yielding greater consistency with these cognitions. Or, where attractions or repulsions remained essentially unaltered, estimates of the attitudes of others generally resisted unbalancing changes despite the opportunity for more realistic cognitions.

The research also makes clear that the self-balancing tendency of these attitudinal cognitive systems depends both on the strength of mutual attraction among the persons involved and on the importance of the objects-of-attitude for these persons—a pair of variables that have sometimes been overlooked in research or discussion about balance theory. The system, in short, becomes less well knit the lower the personal attraction (or repulsion) and the less important the objects-of-attitude for the individuals involved.

The study adds to the growing empirical material on balance theory, a test notable for its realistic conditions and for the span of time that it embraced—a period of observation that is not approached by the more usual work on laboratory groups arbitrarily assembled for a few sessions of intensive experimentation. It leaves in this reviewer only the wish that this rich opportunity for research might have gone beyond its pencil-and-paper tests of attitudes and estimates of attitudes to add interview material that might show how consciously or unconsciously the self-balancing system works in various subjects. Are there, for instance, people in whom the operation of the system is a patent and familiar fact of social life that they themselves recognize? Are there others for whom it works so subtly and defended from recognition that "I do not love thee, Dr. Fell; the reason why I cannot tell" rings true? Perhaps, now that balance theory is reasonably well established, the next job is to learn more about it from the inside out and to search for personal differences that might relate it to personality theory as well as to the averages of interpersonal research.

Vassar College

Dwight W. Chapman

*Mathematical Methods in the Social Sciences.* Edited by Kenneth J. Arrow, Samuel Karlin, and Patrick Suppes. Stanford, California, Stanford University Press, 1960. Pp. vii, 365. $8.50.

This book publishes a symposium held at Stanford. Economists, management scientists, and psychologists were present and interacted, and their papers were published. The three areas were parceled out among the editors who arranged papers alphabetically by author. The almost complete lack of connection between the papers made this a sensible alternative to organization.

The adept in mathematical psychology will want to see W. K. Estes' random-walk model for choice behavior. Estes works from the idea that several partial reactions are needed before a choice is consummated, and failure to progress puts the subject back at the beginning of his choice process. Response probabilities are calculated for several applications, but latencies (which show almost-geometric distributions in such models) are not considered. The model has been followed up and has proven to be of value. R. D. Luce takes up latencies in a study which assumes a process of elimination. A typical multiple-process distribution of response-times is derived and applied (but without a good empirical fit) to the information-theory choice-latency experiments. C. J. Burke gives a concise review of theory and experiment in two-person prediction experiments, but more complete accounts are now available. R. C. Atkinson discusses an unusual theory of discriminative learning in which the subject switches back and forth from using stimulus-patterns to using components; R. R. Bush hammers out some small results on Luce's mysterious beta model of learning; and Suppes develops stimulus-sampling theory for a continuum of responses. The above papers, perhaps with Galanter and Miller's criticism of reflex theory, form some kind of dimly-bounded unit. In addition, T. W. Anderson discusses stochastic models which give results looking like Guttman's simplex, circumplex, and radex models. There seems to be no reason to think that a time-dependent process underlies those models, but Anderson is satisfied to give the mathematical possibilities without psychological evaluation. Jacob Marschak has a paper on utility theory which gets into the psychology section because it deals with random-choice behavior. Marschak is more interested in the large system in which choices are embedded than in the psychology of choice. Herbert Solomon has a paper on item-analysis and test-design in which he shows that very basic questions of test-theory are still open, if the usual simplifying assumptions are not accepted. Solomon summarizes and applies a small but healthy modern literature on these statistical problems.

In summary, the psychological papers are heterogeneous and somewhat uneven in quality. The first 202 pages of the book were not helpful to this reviewer who does not know mathematical economics or inventory and queueing theory. There is no index and no organization to the book as a whole, and the papers do not gain in force or prominence by being published together. It is to be hoped that symposium volumes will never be thought to replace real books, especially the systematic treatises and introductory textbooks so badly needed in mathematical psychology.

Indiana University FRANK RESTLE

*The Development Psychology of Jean Piaget.* By JOHN H. FLAVELL. Princeton, D. Van Nostrand, 1963. Pp. xvi, 472, $8.75.

Jean Piaget is an exasperating, provocative, and influential psychologist who is poorly understood by the majority of American psychologists. Reading Piaget is a difficult and irritating venture, for he violates, with no air of concern, the rules of methodology, theory-construction, and exposition that are believed to be so important by American psychologists. Those who have been interested in Piaget have usually spent more time in trying to show where Piaget was wrong in detail than in trying to understand his total system. This has occurred, not because the system is conceptually so difficult, but because it is constructed and written in such a loose and impromptu style.

These problems are now greatly reduced. Professor Flavell has written an excellent book summarizing in a clear and thorough manner what Piaget has said and done. Even Piaget agrees "it *is* an excellent book." Flavell is sympathetic, but maintains a sober and realistic attitude towards the ideas and work of this important psychologist. He has managed to produce clarity by organizing, disentangling, and integrating Piaget's vast output of 25 books and over 150 articles.

The volume begins with an introduction to Piaget and traces his career from his early days in biology to the present. Since most readers are not ready to delve into the system all at once, Flavell eases them into it with a discussion of Piaget's goals and the basic concepts with which he has worked. Piaget has never made life easy for himself by tackling small problems. Instead, he has dealt with such grand topics as the emergence of the structure of intelligence and the development of perception. In dealing with these problems he has proposed an assimilation-accommodation or equilibrium model which operates through a series of stages. The core of the first section of the book is a discussion of the model and of the developmental changes that occur during the sensory-motor period, in preoperational thought, and in concrete and formal operations.

The second part of the volume is devoted to a discussion of the interesting and original studies conducted in Geneva by Piaget, Inhelder, and their colleagues. Piaget charges in the Foreword to the volume that Flavell gives more emphasis to the studies than to the theory, but agrees that this is probably a wise decision. The studies of language and thought, judgment, reasoning, reality, causality, and moral judgment—the old Piaget—are summarized in one chapter. The newer Piaget, less well known, but currently receiving the widest attention, is found in the chapter summarizing the work on quantity, logic, number, time, movement, and velocity. The newest, but not the latest Piaget (as Piaget emphasizes in the Foreword) is presented in the chapter on space, geometry, chance, adolescent reasoning, and perception. A review of other relevant research from Geneva and elsewhere concludes the second section of the volume. Flavell ends with a sensible and acute chapter of appreciation and criticism of the strengths and weaknesses of this enormously impressive man.

University of Minnesota HAROLD W. STEVENSON

*Social Science Approaches to Business Behavior.* Edited by GEORGE B. STROTHER. Homewood, Illinois, Richard D. Irwin, Inc., 1962. Pp. vii, 183. $5.00

This book contains papers by six leading social scientists who represent different perspectives of business behavior. In addition, the editor provides an introduction treating some of the dilemmas and historical trends in business education. The book is a product of the authors' service as visiting lecturers at the School of Commerce, University of Wisconsin. The papers illustrate contrasting conceptual and methodological approaches, but do not attempt to be complete or integrated. There are no surprises for those who follow current developments in the field, although some useful new material and some elaborations of earlier work are presented.

Robert Dubin, being wary of studies of attitudes, values, and motives, argues that social scientists should pay more attention to behavior and situations of behavior. Chris Argyris clarifies the value aspects of his propositions about the integration of individuals and organizations, and discusses the similarity between conceptions of organizational health and individual mental health. W. Lloyd Warner reports some newer material on the origins and characteristics of executives. William F. Whyte discusses the social processes involved in applying social science. R. Duncan Luce summarizes some ideas concerning risky decision-making. The terminal chapter by Mason Haire is on historic changes in conceptions of human motivation and of social power.

The book has value as a stimulating sampler of current ideas for the business man and for the general public. Journeyman psychologists will prefer other sources unless they have a special interest in the work of one or another of the countributing authors.

University of Michigan STANLEY E. SEASHORE

*Selected Readings on the Learning Process.* By THEODORE L. HARRIS and WILSON E. SCHWAHN. New York, Oxford University Press, 1961. Pp. xi, 428. $3.50.

Nowadays selections of readings from original sources abound. The present collection of readings on the learning process is designed for a course in educational psychology or, more specifically, for that portion of a course in educational psychology devoted to learning. The collection is distinguished by an emphasis upon "pure" rather than applied studies and by a welcome catholicity. These characteristics make the book appealing; it provides the sort of material to which one wishes students of educational psychology were more frequently exposed. The contributions vary from Ralph Gerard on the biological basis of learning, through Duncker on problem solving, Hull on concept formation to Robert Nichols analyzing the creation of one of his own poems. Missing are papers devoted to behavior theory, programmed learning and the learning of skills. Many of the selections are quite old, a feature which will discourage many potential users. As is nearly always the case, the editors could have done more to pull the various selections together into some kind of continuity. The editors content themselves with introductory paragraphs and brief introductions to some but not all sections of the book. Thus, the net judgment is simply that this is another book of readings. Breadth of selections and a high intellectual content do not quite compensate for a kind of quaint old-fashioned air and eccentricity of choice.

The Johns Hopkins University JAMES DEESE

*Encounters: Two Studies in the Sociology of Interaction*. By ERVING GOFFMAN. Indianapolis, Bobbs-Merrill, 1961. Pp. 152. $1.95.

If we customarily think of role-theory 'man' as being placed somewhere between clinical and ecological man, Goffman has, in *Encounters* seriously upset the tidiness of our design. On the ecologic side he has suggested proceeding more atomistically and with more voluntarism than is usual in role theory by using "focussed interactions" (encounters) for his ultimate units of determination. He engages in a discerning if primarily essayistic discourse on the distinguishing features of these most elementary units of social life: their rules for transformation, for excluding irrelevant considerations, for reducing tension, for heightening involvement. Unfortunately, his primary theoretical model for such encounters is a game (others are parties, love-making etc.), and it may be questioned whether a game which is itself already a cultural model of competitive interaction can be used naively as a theoretical model without conceptual confusion.

On the clinical side, Goffman suggests that many of what are usually thought of as idiosyncratic styles and defenses may actually be subsumed to typical role behaviors through the rubric of role distance. It is not normal, he says, to be contained entirely by the role you occupy at the moment, unless you are a neophyte or a fool. It is normal to maintain some degree of disidentification and there are, therefore, regularities of disidentification which can be sociologically described. This notion both normalizes much clinical subject-matter and at the same time illuminates with more clarity than has been provided by Erikson, Huizinga or Caillois, the serious part that playfulness has in adult behavior and social intelligence.

Bowling Green State University BRIAN SUTTON-SMITH

*Licht und Farbe*. By ECKART HEIMENDAHL. Berlin, Walter De Gruyter & Co., 1961. Pp. xvi, 284.

According to the publisher's notice, the author approaches color from a "psychological point of view." Actually, the point of view is quite foreign to most modern psychologists who work in the area of color vision. In some places Heimendahl's approach is more adequately characterized as 'metaphysical,' in others as 'phenomenological.'

The main topics discussed are the ontological status of color, the phenomenological ordering of colors, and the emotional, esthetic, and symbolic functions of color. The author has constructed several new color figures (in the sense of the traditional color circle) which are presented in beautifully produced color plates. These figures are said to represent such things as the "dynamic structure of the color order" and "the dynamic continuity of colors with their specific tensions" (p. 145).

Unfortunately, the author rarely defines his terms in a way that would permit a scientist to extract some empirical consequences from his statements. The large body of empirical work on color vision is largely ignored. There is virtually no overlap, for example, with the subject matter of Y. Le Grand's book which bears the very similar title *Light, Colour and Vision.* Psychologists interested in color vision are not likely to find this book very useful.

New School for Social Research

ERIC G. HEINEMANN

*Psychology as Applied to Nursing.* By ANDREW MCGHIE and MARGARET LAMB, 2nd ed., Baltimore, Md., Williams & Wilkins Co., 1961. Pp. 263. $4.50.

This volume represents an attempt to present to student nurses a commonsense, unbiased, useful review of present psychological theories. The first four chapters of the book deal with the development of the individual from infancy to old age and the final eight deal with the various motivating forces impinging on this individual both internally and externally. At the end of each chapter there is a section directly applying the psychological principles developed in the chapter to the work of the nurses. It is unfortunate that this section, which is well done and most pertinent, is not longer and is based on material which is fragmentary, confusing and frequently inadequate. The organization of material is arbitrary and there are frequent contradictions. For example, the authors suggest that the leader critically examine research on the advantages of breast feeding and the problems of parent-child separation in the hospital, but blithely assert that all prejudices or stereotypes are the results of neurotic needs. They refer to the individual's sexual development in most oblique terms; yet many of their very excellent illustrations have a somewhat rougish, or even ribald tone. Finally, it is inconceivable that anyone familiar with nurses could present Freudian theory on the id and the ego and never mention the other member of that famous troika, the superego. The best way of using this material may be with the aid of a competent instructor who can supplement information to handle such discrepancies.

Jacobi Hospital, Bronx, N.Y.

PEARL P. ROSENBERG

*La Vision en Lumière Intermittente: Lois ea Méchanismes de la Fréquence Critique de Fusion.* By HENRI PIÉRON. Paris, Centre Nationale de la Recherche Scientifique, 1961. Pp. 91. 8 N.F. 50.

In this short monograph, Piéron attempts to synthesie the findings of a number of workers in intermittent stimulation (flicker experiments). He points out the classes of investigation that have been carried out and then states the six laws he believes can be formulated from the findings. These laws have to do with the relation of critical flicker frequency and various features of stimulation. The law he states for the relation of phase ratio is questionable. He likewise discusses the physiological processes underlying flicker and fusion, but this is brief and leaves considerable to be desired. A great deal that an author might have said here is totally absent. It is not evident whether this omission is because he believes the material doesn't warrant discussion, whether it is too complex to deal with, out of place in the present monograph, or ignorable for some other reason. The basis for the selection and omission of titles is not clear.

Michigan State University

S. HOWARD BARTLEY

# BOOKS RECEIVED

(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review.)

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# EFFECT OF REDUCTION ON THE RELATIONSHIP BETWEEN APPARENT SIZE AND DISTANCE

By ALAN M. HARTMAN, VA Hospital, Roseburg, Oregon

The size-distance invariance-hypothesis proposes an invariant relationship between perceived size and distance such that the apparent size of an object is uniquely determined by an interaction of the visual angle subtended by an object and the apparent distance of that object.[1] In support of this hypothesis, the authors of a recent review of this subject cite research which demonstrates that estimations of size tend to follow the law of the visual angle when the conditions of viewing are impoverished.[2] The rationale here is that perceived size would be consonant with the area of the retinal image which is subtended by an object when the perceived distance of the object is indeterminant. The Holway-Boring study would be an example of this source of evidence in respect to the problem of perceived size of objects, while a study by Edwards demonstrates invariance in the perception of after-images when the cues to distance are reduced.[3]

The reviewers point out, however, two assumptions which are basic to the use of visual angle matches in support of this hypothesis.[4] First, apparent distance tends toward zero when the viewing conditions are completely reduced. Secondly, apparent distance takes on some consistent value other than zero which is the same for both the standard and the variable stimuli; and thus the equation of visual image size is a result of *S*'s assumption that the standard and the variable stimuli are equidistant. Epstein *et al.* reject the first assumption and believe that support for the second is inconclusive.[5] In this respect, the reviewers indicate the need for further studies of

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* Received for publication May 13, 1963. The author is now at Idaho State University.

[1] William Epstein, The known size apparent-distance hypothesis, this JOURNAL, 74, 1961, 344.

[2] William Epstein, John Park, and Albert Casey, The current status of the size-distance hypothesis, *Psychol. Bull.*, 58, 1961, 491-493.

[3] A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, this JOURNAL, 54, 1941, 30-32; see also Ward Edwards, Apparent size of after-images under conditions of reduction, this JOURNAL, 66, 1953, 449-455.

[4] Epstein, Park, and Casey, *op. cit.*, 492-493.

[5] Epstein, Park, and Casey, *op. cit.*, 493.

the effects of conditions of reduction on the perception of distance. Moreover, it seems reasonable to expect that systematic research of the effects of impoverished viewing conditions on perceived distance would be most relevant to the problem of the validity of the size-distance invariance-hypothesis if corresponding data on perceived size as a function of objective distance is gathered at the same time.

If the assumption of equidistance in apparent distance estimates is a valid one when cues to distance are reduced, and if the invariance hypothesis is of any merit for the prediction of perceived size and distance under such conditions of viewing, the following hypotheses are believed to be in order:

*(1)* Under highly impoverished conditions of viewing, judgments of distance will conform to the equidistance assumption, *i.e.* judgments of distance will be equal, although objective distance varies. Furthermore, judgments of size and distance should parallel the visual angle function.

*(2)* As cues to size are made available in the field of vision, the *Ss* will report corresponding variations in apparent size and apparent distance as would be demanded by the size-distance invariance-hypothesis.

The purpose of the present paper is to test these hypotheses.

## METHOD

*(1) Subjects.* The *Ss* for the present study were 48 men, patients at the hospital. The men volunteered in response to requests to participate in a study of vision. The basic criteria for selection were normal or corrected normal vision in the right eye and the ability to understand and follow simple instructions. The *Ss* were naïve in regard to the specific purpose of this study.

*(2) Apparatus.* The apparatus for the present research was set up in a 20 × 15-ft. dark room which opened into a second 5 × 7-ft. dark room. The larger room contained the last 12 ft. of the vision tunnel, and the smaller room contained the first 3 ft. of the tunnel, *S*'s seat, illumination controls, and the controls for the comparison devices.

*(a) Left-hand field.* The left field was 2 × 2 × 15 ft. and had spaces for screens set at 3, 6, 9, 12, and 15 ft. from *S*.

*(b) Right-hand field.* This part of the apparatus was 1 × 2 × 15 ft. and was separated from the adjacent left-hand tunnel by ⅛-in. masonite panels. The tunnel was joined together from 3 ft. sections, and the far right-hand wall of the right-hand tunnel had 6 × 24-in. openings cut in each section.

*(c) Measuring device for distance.* This unit consisted of a plastic ball, 1 in. in diameter, which was so attached to a string and pulley system that *S* could control the forward and backward movement of the marker. The movement of the marker ranged between 85 and 465 cm. from *S*. A 25-w. bulb was so mounted at the end of the right-hand tunnel that *S* could see the marker and the right side of the tunnel without having to look directly into the light-source.

*(d) Size-device.* The variable stimulus was mounted at the base of the left-hand reduction screen and was 40 in. from *S*'s face-mask. The unit contained an evenly

lighted ½ × 24-cm. line of light and a shutter which could be moved over the line to provide a variable length of light. *S* saw a line of light which he could vary from zero to 24 cm. by means of a string and pulley system. To the right of the shutter was a small luminous spot of light which served as a guide to the position of the shutter.

*(e) Targets and backgrounds.* The visual angles of the targets and the backgrounds were constant for all distance-settings. Where demanded by the design of the study, variation in the ratio of the targets to the elements of the background was obtained by placing the ⅛-in. hardware cloth completely over the front of the side of the milk-glass screen which faced *S*. As the distance between *S* and the screen was increased, the angles subtended by the small square elements of the background were decreased, while the visual angle subtended by the target square remained constant. The target-squares consisted of black flock paper. These were 2, 3, 4, and 5 in. wide and were set at 6, 9, 12, and 15 ft. from *S*, respectively. Each square was so taped to the front of the milk-glass panel that *S* saw a black square centered in the background. The visual angle subtended by the targets was 1° 34′, and the cue-reduction screen limited the visual angle of the background to 6° 20′.

*(f) Screens.* The background screens were constructed of milk-glass framed between 25 × 24-in. pieces of ⅛-in. masonite. All screens could be moved in and out of the left-hand field of view by *E*, who was stationed in the larger room. The target-squares and the background areas were so set that all stimuli were perpendicular to *S*'s line of sight.

*(g) Artificial pupil.* A-O plastic safety goggles were imbedded into a 24 × 36 × 18-in. masonite sheet. The masonite was cut and shaped to form a mask into which *S* could comfortably place his entire face and maintain a uniform position during the experimental procedures. Heavy black paper was attached to the inside of the goggles, and a 4-mm. aperture was cut into the right side of the front piece of the goggles.

*(h) Cue-reduction screen.* A reduction-screen was mounted in the left-hand field at a distance of 36 in. from *S*. The opening in the screen was 4 × 4-in. and it was in line with the artificial pupil and the screens.

*(i) Lighting.* The screens were illuminated from behind by light reflected from a wall behind the left-hand field. The source of light was a 500-w., 35-mm. slide-projector located 26 ft. on the diagonal from the back wall. Neutral density filters were used to equate the illumination of the screens. The densities were 0.0, 0.2, 0.6, and 0.9, and were selected to provide subjective equality of the illumination of the targets and backgrounds which were mounted 6, 9, 12, and 15 ft. from *S*, respectively.

*(3) Experimental design.* The *S*s were assigned randomly to the experimental groups as follows.

*(a) Group* $E_1$ *(N = 15).* Targets and backgrounds were all viewed in the left-hand field, but there was no variation in the ratio of target to background because the hardware cloth remained in position at 183 cm. from *S*. The firm position for *S*'s head provided by the face-mask and the small artificial pupil served to reduce the possibility that parallax could serve as an effective cue for distance.

*(b) Group* $E_2$ *(N = 15).* Targets and backgrounds were presented in the left-hand field, but in contrast with Group $E_1$ there was a small variation in the ratio of the

target to the background area due to the decrease in visual angles subtended by the ⅝ in. hardware mesh squares as the distance of the screens increased. For this condition, the mesh was suspended directly in front of the milk-glass screen.

*(c) Group* $E_3$ *(N = 9).* Black target-squares were suspended in the right-hand field at distances corresponding to the placements of the targets in the left-hand field. Similarly, the targets were constructed in such a manner as to subtend the same visual angle at each distance as did the targets for the left-hand field.

*(d) Group* $E_4$ *(N = 9).* This group differed from Group $E_3$ in that the target-squares were attached to the hardware cloth and then suspended in the right-hand field.

Estimations of size and distance were determined in accord with the method of average error. Therefore, judgments of size and distance were averaged from eight ascending and eight descending trials for each size- and distance-setting. The order of presentation of the objective settings, however, was randomized for each *S*.

*(4) Instructions and pre-training.* Each *S* was seated in front of the apparatus, and *E*, who was stationed in *S*'s compartment, proceeded to explain the components of the tunnel and gave a brief description of what *S* would see in the left- and right-hand fields. *S* was asked to place his face into the mask and view the screen which was located 15 ft. down the tunnel. He was then asked to describe what he saw in the left- and right-hand fields. If he failed to describe the screen in detail, *E* asked questions which concerned the gray background, the small squares which provided the texture for the background, and the solid black square which was located in the center of the field. He was questioned further about his ability to view the distance comparison-device. *E* explained to *S* that each time he would be asked to view the visual field, the tunnel, and after a 10-sec. interval, *E* would tell him to set the size-comparator to match the width of the target-square and then set the distance-comparator to the position at which he thought the marker was directly adjacent to the target. The sequence of the distance- or size-comparisons was randomized for each series of trials. *S* was allowed as much time and practice as he desired. When *S* reported that he was ready to begin the series of trials, the illumination of the tunnel was turned off, and the position of the target and background set according to the schedule; the approximate level of illumination was selected and the trial was begun. Similarly, after *S* had made the comparisons of size and distance, the illumination was cut, his measures were recorded, and the stimulus-complex was re-set to correspond to the pre-determined sequence of events.

## RESULTS

The initial aspects of the analysis for consideration are those results which bear most directly on the question of the validity of the assumption of equidistance. Since the strongest case for the perception of equidistance would be the congruence of perceived size with the matching data of visual angle, the first step in this analysis then was to demonstrate that the *Ss* in Group $E_1$ actually did make estimates of size which were comparable to matches in visual angle.

*(1)* If we assume that the expected matches in visual angle in this situa-

tion would be comparable to the mean of the population of perceived size-measures, which are based on the geometry of the elements of the field of view, then there is some basis for a test of the significance of the difference between the observed and the expected mean values.* For Group $E_3$, the differences between the mean of the observed measures of size (see Table I) and the estimate of the expected "population mean" were as follows: $t = 1.94$, 2.14, 2.26, and 2.26 for the targets set at 183, 277, 371, and 465 cm. respectively. For a two-tail test, a $t = 2.145$ would be significant at the 5% level. If this analysis is at all valid, it would appear that the *Ss* in Group $E_3$ are over-estimating the size of the target consistently if the match in visual angle is accepted as the best estimate of what perceived size should be under the most impoverished conditions of viewing. What is more, perceived distance under these impoverished conditions of viewing was overestimated in comparison with the objective distances of the targets at the nearest position.

TABLE I

VALUES OF PERCEIVED SIZE AND DISTANCE AS A FUNCTION OF OBJECTIVE DISTANCE

| Group | Objective distance | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 183 cm. | | 277 cm. | | 371 cm. | | 465 cm. | |
| | size | dis. | size | dis. | size | dis. | size | dis. |
| $E_1$ Mean | 4.47 | 237.7 | 4.58 | 256.9 | 4.79 | 253.3 | 4.85 | 271.1 |
| *SD* | 3.1 | 35.2 | 3.0 | 54.6 | 3.2 | 56.6 | 3.3 | 60.4 |
| $E_2$ Mean | 4.60 | 198.4 | 5.03 | 245.6 | 5.28 | 257.8 | 5.48 | 277.0 |
| *SD* | 2.9 | 39.1 | 3.4 | 68.3 | 3.6 | 76.5 | 3.9 | 85.9 |
| $E_3$ Mean | 5.4 | 193.6 | 7.3 | 287.5 | 10.0 | 371.2 | 10.4 | 407.7 |
| *SD* | 2.0 | 11.6 | 3.6 | 17.7 | 5.8 | 26.1 | 5.9 | 37.2 |
| $E_4$ Mean | 7.6 | 232.8 | 8.8 | 324.8 | 10.8 | 360.5 | 10.9 | 419.2 |
| *SD* | 3.1 | 53.4 | 3.7 | 30.4 | 5.5 | 30.5 | 6.6 | 32.9 |

*(2)* Despite the lack of evidence for correspondence of perceived size and the matches of visual angle, or the perceived distances with the objective distance of the nearest target or the distance of the size-comparator (see Table I), there still seems to be a fair degree of invariance in perceived size as well as perceived distance. Thus, a second approach to the evaluation of the validity of the assumption of equidistance attempted here is a within-group analysis of the trend for perceived size and perceived distance to deviate from a line parallel with the function of the visual angle.

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* A. L. Edwards, *Experimental Design in Psychological Research*, rev. ed., 1960, 87-88.

(a) For Group $E_1$ the value of $F$ was 5.84 for the estimates of distance, and this value is significant at better than the 1% level for 3 and 42 *df*. For the same group, the analyses of perceived size to vary as a function of objective distance was significant at the 5% level, $F = 2.99$ with 3 and 42 *df*. This result does not support the invariance-position. Further analysis of the source of the variance, however, casts a different light on these data. A series of *t*-tests for correlated means indicated that the differences in perceived size and in perceived distance are due to the comparisons of estimates for the 183 and 464 cm. distances. At and beyond 9 ft., perceived size and distance certainly do follow along with expectations based on the assumption of equidistance. It is a reasonable possibility that the estimates of size and distance at the nearest distance for viewing could have been influenced by factors ocular and otherwise which are not effective to the same degree at the further distances.

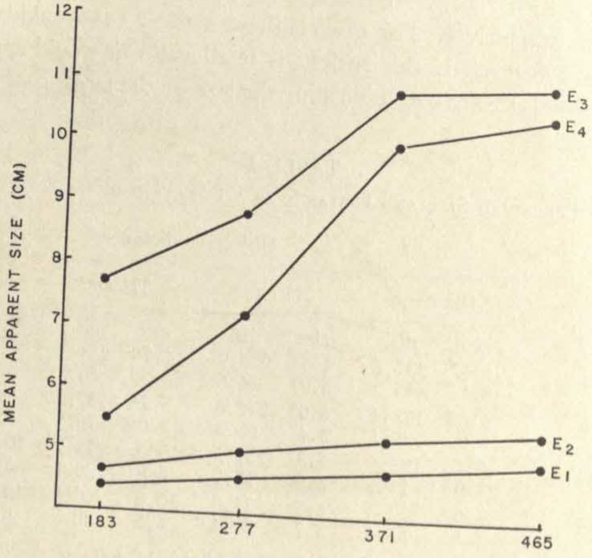

FIG. 1. THE TRENDS IN APPARENT SIZE FOR THE FOUR CONDITIONS OF CUE-REDUCTION

(Group $E_1$ through $E_4$ correspond to most through least degree of severity in the reduction of cues to size and distance.)

(b) For Group $E_2$, the introduction of cues to size and distance was followed by estimates of size and distance which showed trends toward increases in the direction of the objective values (see Table I and Figs. 1 and 2). For Group $E_2$, the analyses of variance of the trend for apparent distance and size to increase as objective distance increased showed significant differences for both size and distance. The value of $F$ for variations in distance was 29.59 and for 3 and 42 *df*, this value is significant at better than the 1% level.

(c) The trends for perceived size and perceived distance to vary with objective distance became highly significant as the conditions of viewing were made adequate to an ever increasing degree. For Groups $E_3$ and $E_4$ combined, the moderate cue-reduction groups, the value of *F* for perceived size was 17.29, and for 3 and 51 *df* this value is significant as better than the 1/10% level. Similarly, the value of *F* for perceived distance was 27.24, and for 3 and 51 *df* this value is significant at better than the 1% level. As was the case for Group $E_1$ compared with Group $E_2$, the slight variations in viewing conditions did not lead to significant differences between the groups at any or all of the objective distances.

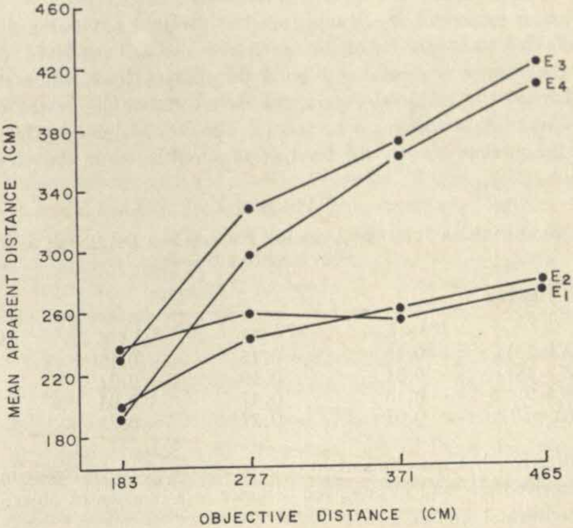

FIG. 2. THE TRENDS IN APPARENT DISTANCE FOR THE FOUR CONDITIONS OF CUE-REDUCTION

*(3)* While the trend-analyses within each of the conditions of viewing are supportive of the position of equidistance to some degree, another approach to the analysis might be helpful. If the equidistance assumption is valid when the conditions of viewing are most impoverished, then we might expect that differences between the means of the four groups would be significant for the more distant targets.

*(a)* At the 183-cm. viewing distance, the values of *F* were 2.49 for both perceived size and perceived distance. This value fails to meet the 5% level of significance for 3 and 44 *df*.

*(b)* The value for *F* for the perceived size and distance at 277 cm. was 3.46 and 4.62, respectively. These values are significant at the 0.025% and better than the 0.025% levels respectively for 3 and 44 *df*. Group $E_4$ differed significantly from all

other groups at better than the 1% level, and Group $E_3$ differed from Groups $E_1$ and $E_2$ at better than the 2% level as indicated by further *t*-tests.

*(c)* At the 371-cm. distance, the values of *F* for perceived size and perceived distance were 5.25 and 12.95, respectively, and for 3 and 44 *df* these values are significant beyond the 0.5% and 0.1% levels. Further analysis of the origin of the differences indicated that Groups $E_1$ and $E_2$ did not differ, and similarly, no differences were found between the remaining two groups. Groups $E_3$ and $E_4$ do differ from Groups $E_1$ and $E_2$ at better than the 0.1% level of significance.

*(d)* A similar pattern of results was found at the 465-cm. viewing distance. The values of *F* were 4.87 and 16.61 for perceived size and perceived distance, respectively; the former is significant beyond the 0.025% level, while the latter is significant beyond the 1% level. Again, the differences are due to the comparisons of Groups $E_1$ and $E_2$ with Groups $E_3$ and $E_4$. The data analyzed in this manner indicate that the attempt to vary the level of impoverishment in the viewing conditions was effective, but more important, this analysis reflects increases in the divergence in perceived size and in perceived distance as a function of objective distance for the experimental groups.

TABLE II

RHO CORRELATIONS BETWEEN AVERAGE ERRORS FOR DISTANCE- AND SIZE-ESTIMATIONS

| Monocular viewing conditions | Distance | | | |
|---|---|---|---|---|
| | 183 cm. | 277 cm. | 371 cm. | 465 cm. |
| Group $E_1$ ($N=15$) | −0.18 | −0.13 | −0.13 | −0.16 |
| Group $E_2$ ($N=15$) | 0.31 | 0.19 | 0.22 | 0.10 |
| Group $E_3$ ($N=9$) | −0.18 | 0.47 | 0.03 | 0.02 |
| Group $E_4$ ($N=9$) | 0.07 | −0.27 | −0.23 | −0.32 |

*(4)* Hypothesis 2, that the *S*s would report corresponding variations in apparent size and apparent distance, was not confirmed at any single setting of size and distance or any condition of viewing. The correlations between apparent size and apparent distance based on individual differences in the average error of estimate scores were low and insignificant (see Table II).

*(5)* We might examine a further, and perhaps the most important line of analysis which pertains to the validity of the size-distance invariance-hypothesis. Within the framework of the present study, invariance may be best expressed as a ratio of perceived size to perceived distance. The reliability of individual measures of size and distance was high, and as expected, the consistency of the ratios of measures of size and distance are correspondingly high. Moreover, the correlations between the ratios of perceived size to perceived distance for the various combinations of objective distances ranged from 0.90 to 0.97 for the full number of *S*s.

The goal of this analysis is to demonstrate an invariance in the ratios of perceived size to perceived distance due to the conditions of viewing and the differences within the groups attributable to variation in the objective distances. Due to the differences in the number of *S*s in the four groups, a mixed type of analysis of variance was not considered as feasible. Thus, the analysis was done in two separate stages.

(*a*) First, a simple analysis of variance was calculated to assess the significance of the variations in the ratios due to the groups. The ratios for each *S* were averaged across the four distances, and these values were used in the evaluation of differences due to the conditions of viewing. For this analysis, the value of *F* was less than unity, and for 3 and 44 *df* this result is not significant.

(*b*) The second stage of the analysis consisted of four separate analyses of variance of the trends within each of the groups for the ratios of perceived size to perceived distance to vary as a function of objective distance. The value of *F* for Group $E_3$ was less than unity. The remaining groups, however, did not follow suit. The value of *F* for Group $E_1$ was 2.94 and is significant at better than the 5% level for 3 and 42 *df*. The value of *F* for Group $E_2$ was 4.76 and is significant at better than the 1% level for 3 and 42 *df*. Similarly, the value of *F* for Group $E_4$ was 9.8 and is significant beyond the 0.1% level for 3 and 24 *df*.

Before we reject the size-distance invariance-hypothesis, however, it is well to look at Table III and check the magnitude of the differences in the means of the ratios per each individual. The data for each individual are consistent to a high degree, and the magnitude of the deviations is small. The values of *F*, then, are due in no small part to the high consistency of individual scores across the four objective distance-settings. Further, Figs. 1 and 2 are suggestive of the possibility that the variance within each of the groups' size and distance measures primarily may be due to the estimates of size and distance at the nearest distance. In view of this possibility and as a short cut in the analysis, the trend-analyses were re-done and included in the last three distance settings. Except for Group $E_4$, the values of *F* were not significant and support the notion that cues for size and distance may not have been invariant for all the distances for all the groups. Moreover, the data indicate that the ratios of perceived size to perceived distance are consistently invariant over the range of the further distances (see Table III). This seems characteristic of all the groups for the objective distance in excess of 12 ft. from *S*.

## DISCUSSION

The results of the present study show that when conditions of cue-reduction are most severe, judgments of apparent size of the targets consistently exceed the values expected on the basis of the matches of visual angle, but this result is borderline in respect to significance. Even if one

## TABLE III

### The Ratios of Perceived Size to Perceived Distance as a Function of Objective Distance

| Groups | *S* | Objective distance<br>183 cm. | 277 cm. | 371 cm. | 465 cm. |
|---|---|---|---|---|---|
| E₁ | 1. | .008 | .007 | .008 | .008 |
| | 2. | .022 | .020 | .024 | .022 |
| | 3. | .013 | .012 | .014 | .013 |
| | 4. | .015 | .014 | .014 | .013 |
| | 5. | .006 | .006 | .006 | .006 |
| | 6. | .036 | .031 | .031 | .034 |
| | 7. | .008 | .009 | .011 | .011 |
| | 8. | .009 | .009 | .009 | .008 |
| | 9. | .027 | .027 | .030 | .028 |
| | 10. | .015 | .013 | .016 | .013 |
| | 11. | .014 | .013 | .013 | .012 |
| | 12. | .029 | .025 | .026 | .024 |
| | 13. | .017 | .017 | .017 | .016 |
| | 14. | .015 | .013 | .012 | .010 |
| | 15. | .077 | .074 | .073 | .078 |
| | Mean | .021 | .019 | .020 | .020 |
| | *SD* | .011 | .011 | .011 | .011 |
| E₂ | 1. | .015 | .012 | .013 | .012 |
| | 2. | .018 | .018 | .018 | .019 |
| | 3. | .018 | .018 | .019 | .019 |
| | 4. | .015 | .015 | .015 | .013 |
| | 5. | .010 | .009 | .008 | .008 |
| | 6. | .010 | .008 | .008 | .007 |
| | 7. | .027 | .018 | .018 | .015 |
| | 8. | .036 | .032 | .034 | .035 |
| | 9. | .019 | .013 | .013 | .013 |
| | 10. | .016 | .016 | .016 | .014 |
| | 11. | .019 | .015 | .015 | .016 |
| | 12. | .033 | .029 | .029 | .026 |
| | 13. | .025 | .025 | .022 | .022 |
| | 14. | .034 | .038 | .041 | .041 |
| | 15. | .055 | .050 | .055 | .052 |
| | Mean | .023 | .021 | .022 | .021 |
| | *SD* | .012 | .011 | .012 | .012 |
| E₃ | 1. | .037 | .037 | .047 | .053 |
| | 2. | .020 | .013 | .011 | .011 |
| | 3. | .023 | .015 | .016 | .014 |
| | 4. | .016 | .013 | .013 | .012 |
| | 5. | .044 | .045 | .053 | .048 |
| | 6. | .028 | .035 | .042 | .040 |
| | 7. | .039 | .036 | .033 | .030 |
| | 8. | .018 | .013 | .011 | .009 |
| | 9. | .024 | .020 | .017 | .017 |
| | Mean | .028 | .025 | .027 | .026 |
| | *SD* | .008 | .012 | .016 | .016 |
| E₄ | 1. | .036 | .031 | .035 | .033 |
| | 2. | .063 | .051 | .051 | .050 |
| | 3. | .037 | .026 | .028 | .020 |
| | 4. | .047 | .040 | .050 | .043 |
| | 5. | .020 | .015 | .014 | .012 |
| | 6. | .013 | .011 | .011 | .009 |
| | 7. | .037 | .037 | .046 | .038 |
| | 8. | .023 | .015 | .013 | .012 |
| | 9. | .030 | .023 | .021 | .020 |
| | Mean | .034 | .028 | .030 | .026 |
| | *SD* | .012 | .012 | .004 | .013 |

should accept the validity of this analysis, the possibility still remains that the estimates of size are consistent with matches of visual angle. Like the findings reported by Wallach and McKenna, not even 10% of the *S*s in the severe cue-reduction group demonstrated matches of visual angle, although the *S*s in the present study did give estimates of size which varied about the expected values.[7] If we assume that the comparison of size which would be expected on the basis of the visual angle of the target is the best estimate of the population of comparisons of size, then it should be possible to test the significance of the differences between the observed and the expected comparisons of size.

The primary question for the present study, however, is whether the estimates of distance are perceived as equidistant. The orders of the estimates of distance are similar to those for size, and again, the analysis of the significance of the trend away from the function which would denote equidistance in the case of the severe cue-reduction group. Like the estimates of size, the distance-measures are over-estimates of the expected values at the near distance. These results are interpreted to indicate that when cues to distance were eliminated, judgments of target-size at least followed a function of visual angle, while the function for perceived distance was parallel. We conclude that the function of perceived size at least parallels the function of visual angle when conditions of viewing are reduced and are accompanied by parallel values for perceived distance. The major exceptions to this finding were attributable to comparisons of size and distance at the nearest distance. Further, as the severe conditions of cue-reduction were relaxed, measures of size and distance did deviate accordingly from the visual angle and the functions of equidistance. These data, at least, are consonant with the size-distance invariance-hypothesis.

The lack of correlation based on individual differences between perceived distance at any single distance is in agreement with Gruber's findings.[8] Moreover, the analysis is consistent with Epstein's view that the size-distance invariance-hypothesis is not supported in an overwhelming manner.[9]

On the other hand, the ratios between the estimates of sizes and distance do seem highly consistent for any one *S*, and these data, for the most part, are consonant with the size-distance invariance-hypothesis. Further, the invariances in the ratios of perceived size to perceived distance could be due to those viewing conditions and distance settings where there may have been some cues to distance present which were lacking for the other settings. This would be most likely at the nearest viewing distances; and accordingly, it is at 183 cm. that the measures of size and distance most significantly contribute to the variance of the ratios of perceived size to perceived distance as a function of the objective distance. In view of the argument that these data do support the size-distance invariance-hypothesis, how can the ratio of perceived size to perceived distance be consistent for individuals while the correlations between the estimates of size and distance are low and insignificant? After all, the invariance-hypothesis would lead us to expect that variations in the estimates of size should be related to variations in distance. One possible answer to this question is that the absence of a relationship between perceived size and perceived distance at any one distance may reflect the fact that deviations in perceived size are not accompanied by corresponding deviations in perceived distance. Thus, it is possible for an *S* to demonstrate an estimate of size which varies widely from his estimate of the distance of that same target in comparison with the performance of his fellow *S*s. As wide as this variation may be, there is a good possibility that the ratio may be consistent for the entire sample of distances. The invariance demonstrated in this study is due to the consistency of the ratios of perceived size to perceived distance for individual *S*s. Further, there is some evidence that the means of the ratios do not differ significantly from each other as a function of reduction. After all, the traditional interpretation of the invariance-hypothesis implies a constant ratio of size to distance.[10] Once we know the ratio or have evidence that the ratio is consistent throughout the range of distances for the individual, we should be able to predict perceived distance from knowledge of perceived size or vice versa. With this point in mind, the size-distance invariance-hypothesis would be most lacking if there is no consistency or invariance in the ratio of perceived size to perceived distance. For the most part, the present data do not detract from this position.

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[7] Hans Wallach and V. V. McKenna, Size-perception in the absence of cues for distance, this JOURNAL, 73, 1960, 458-460.

[8] H. E. Gruber, The relationship of perceived size to perceived distance, this JOURNAL, 67, 1954, 416-420.

[9] Epstein, Park, and Casey, *op. cit.*, 511-512.

There is, however, another aspect of the traditional statement of the invariance-hypothesis. This one has to do with the visual angle subtended by the object uniquely determining the ratio of perceived size to perceived distance.[11] The visual angle would be the unique determinant only if one accepts the presupposition that the visual angle subtended by the target is the only determinant of the ratio, or perceived size, for that matter. To argue for the validity of this premise is tantamount to assuming that the geometry of the viewing situation is of primary importance and that perceived size is not at all affected by the expectations and assumptions of the unique observer. The evidence provided by the present study does not support this assumption. Furthermore, there is evidence which amply demonstrates factors other than the visual angle which can affect perceived size and distance.[12] In view of this argument, it seems more reasonable to assume that the ratio of perceived size to perceived distance would conform in some manner to the invariance-hypothesis for any given individual. Given the ratio for the individual and his estimation of size, one would not be far wrong in predicting his estimation of the distance of that target.

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[10] Epstein, *op. cit.*, 344.

[11] Epstein, *op. cit.*, 344.

The ratios of perceived size to perceived distance demonstrated in the present study are not in accord with recent findings reported by Gogel, Wist, and Harker.[13] These investigators demonstrated a significant degree of variance in the ratios of perceived size to perceived distance as a function of objective distance, and the range of objective distance was quite similar to those used in the present research except that the nearest distance was nearer and the furthest distance was 20 ft. While direct comparison of the studies is difficult, due to the differences in the variable stimuli and the approach to the measures of size and distance, the present data strongly suggest the possibility that the estimations of size and distance at the nearer distances may be influenced by factors, ocular and otherwise, which do not operate equally for the further distance. In the present study, the comparison-measures at the nearest distance are suspected for this reason, and in view of the illustrations of the results given by Gogel, Wist, and Harker, this explanation may not be completely out of line. The results of the present study, among other things, suggest the necessity for assurance that first of all there is an invariance in the quantity and quality of the cues to perceived size and distance for the complete range of the objective values. Without such assurance or evidence, the validity of tests of the size-distance invariance-hypothesis would remain questionable at best. It would be helpful to have evidence of the degree of invariance in the difficulty which the *Ss* have in making their estimates of size and distance over the entire range of sizes and distances.

## SUMMARY

Forty-eight *Ss* made concurrent judgments of size and distance under three conditions of reduction of cues for monocular viewing. Under conditions of elimination of distance cues, 15 *Ss* made size- and distance-

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[12] R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 1955, 480-486.

[13] W. G. Gogel, E. R. Wist, and G. S. Harker, A test of the size-distance invariance hypothesis, U.S.A. Med. Rep. No. 545, Ft. Knox, Ky., 1962, 10, 21-22; (U.S.A. Med. Red. Lab. Rep. No. 54—Ft. Knox, Ky., 1962).

judgments. A second group of 15 *S*s made size- and distance-judgments under conditions of contrast or relational cues for size. A third and fourth group, each containing 9 *S*s, made size- and distance-judgments under two conditions of enriched, but still monocular, viewing.

The results were: Except for measures at the nearest distance, Hypothesis 1 was shown to be valid. Judgments of distance were equidistant under the severe conditions of reduction of cues to distance, and the corresponding size-matches, which did follow the values expected on the basis of the visual angles of targets, or at least the function of visual angle, were interpreted as valid support for the size-distance invariance-hypothesis. Hypothesis 2 was verified in part. It did appear that as cues to size and distance were added to the visual field, the *S*s did perceive increases in apparent size and distance as a function of objective distance. Individual differences, however, in size- and distance-judgments for any one distance setting were not correlated. Moreover, as the invariance-hypothesis leads us to expect, the ratios of perceived size to perceived distance were, for the most part, invariant for the conditions of viewing as a whole and as a function of objective distance.

We conclude that the present results, while limited in terms of sampling of objective sizes and distances, do support the size-distance variance-hypothesis. Moreover, there may be some basis for the thesis that variance in the ratios of perceived size to perceived distance may be due to factors—ocular and otherwise—which were not invariant for the full range of objective distances and sizes sampled in the present study.

# LEARNING TO LEARN IN RESPONSE-DISCOVERY AND IN PAIRED-ASSOCIATE LISTS

By CARL P. DUNCAN, Northwestern University

Fairly large amounts of learning to learn occur in a variety of tasks.[1] Greenberg and Underwood, however, found intertask improvement in four successive paired-associate lists only from the first to the second list.[2] In their lists, both the stimuli and the responses were adjectives, and different adjectives were used in all lists, *i.e.* the lists may be symbolized as A–B, C–D, etc. The present experiment is a further study of learning to learn in paired-associate tasks. Two types of lists were used. In one, hereafter called paired-associate lists (*PA*-lists), the stimuli were the same but the responses were different from list to list. Thus, the *PA*-lists conformed to the A–B, A–C paradigm, where negative transfer from list to list is usually expected. The other type was response-discovery lists (*RD*-lists). For each stimulus in every *RD*-list, three possible responses were available, and *S* had to discover which one was to be paired with the stimulus. As with *PA*-lists, the stimuli in *RD*-lists were the same in all lists. *RD*-lists were chosen for study in part because they are difficult and in part because *S* must discover the correct response, as in problem-solving. In some problem-solving tasks it has been reported that little or no practice effects occurred over a series of similar problems.[3]

The present experiment was also prompted by recent studies in which learning of a single task was analyzed into different stages. Several investigators have shown that learning may be divided into an earlier, response-learning stage, in which *S* learns what the responses are, and a

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[1] C. P. Duncan, Transfer after training with single versus multiple tasks, *J. exp. Psychol.*, 55, 1958, 63-72; Description of learning to learn in human subjects, this JOURNAL, 73, 1960, 108-114; J. A. McGeoch and A. L. Irion, *The Psychology of Human Learning*, 2nd ed., 1952, 306-309.

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[3] Duncan, Recent research on human problem solving, *Psychol. Bull.*, 56, 1959, 397-429; Duncan, Attempts to influence performance on an insight problem, *Psychol. Rep.*, 9, 1961, 35-42.

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later, associative stage, in which the responses are connected to the stimuli.[4] Although these stages are not completely distinct, they are sufficiently independent to be functions of different variables in some cases. Thus it is possible that different stages of intratask learning will be differentially affected by intertask practice.

## METHOD

*Tasks.* In all conditions the tasks were 10-item lists in which the digits 1–10, as stimuli, were paired with adjectives, taken from Haagen, as responses.[5] In *PA*-lists, only one adjective appeared with each digit. In *RD*-lists, three adjectives appeared with each digit. Five *PA*-lists and five *RD*-lists were prepared. The digits served as stimuli in all lists, but the adjectives were different in every list; thus, 50 adjectives were used to construct the *PA*-lists, 150 adjectives for the *RD*-lists. The same 50 adjectives were the correct responses in both kinds of lists. All adjectives both within and between lists were of low similarity. Digits were paired with adjectives randomly.

Each *PA*-list was typed on memory-drum tape in three different random orders of the 10 items. In the *RD*-lists the three adjectives appearing with each stimulus were typed in a horizontal row. Three random orders were also prepared for the 10 items in an *RD*-list. In each of these orders of the list as a whole, the three adjectives per item appeared once each in the three horizontal positions, left, middle, and right. In addition, the position of the correct adjective varied unsystematically from item to item within a list order.

Both *PA*- and *RD*-lists were presented on a memory-drum in which the stimulus for each item was first exposed alone for 2 sec., then the stimulus and the response or responses together for 2 sec. The intertrial interval was 4 sec.

*Conditions.* Two conditions, varying in the degree to which response-discovery was required of *S*, were provided by the two types of lists, *PA*- or *RD*. In addition, degree of intratask learning was varied; half the *S*s were given 10 trials, half given 25 trials, on each task. Thus, there were four groups, identified as *RD* 10, *RD* 25, *PA* 10, and *PA* 25.

*Subjects.* The *S*s were students from courses in introductory psychology. Students are required to serve in experiments as part of the course. There were 25 *S*s in each of the four groups. The *S*s were assigned to groups in turn.

*Procedure.* Each *S* learned five lists, one list per day for five days. Five completely different orders of assignment of lists to days were used for each block of five *S*s, then repeated.

On Day 1, *PA*-*S*s were shown a sample-card containing a digit and a word. It was explained that this was the kind of item they would learn, and they were given instructions for anticipation-learning. They were told that on the first or observation-trial they should simply study the pairs, and to begin anticipating on the next trial.

For *RD*-*S*s, a sample-card with a digit and three words was shown on Day 1. It was explained to them that they had to guess which one of the three words was correct, and that they then had to anticipate the correct word. They were told that on the observation-trial (Trial 0) they should wait until the shutter raised to expose the three words of each item and then guess aloud which word was correct. Beginning with the next trial (Trial 1, first anticipation-trial) they were to try to anticipate the correct word before the shutter raised. The *S*s were informed that if at any time after the observation-trial they waited until the shutter raised and then guessed, *E* would tell them whether or not the guess was correct but would not credit them with a correct anticipation. The instructions made it clear that correct anticipations were wanted.

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[4] B. J. Underwood, W. N. Runquist, and R. W. Schulz, Response learning in paired-associate lists as a function of intralist and similarity, *J. exp. Psychol.*, 58, 1959, 70-78; Underwood and Schulz, *Meaningfulness and Verbal Learning*, 1960, 93.

[5] C. H. Haagen, Synonymity, vividness, familiarity, and association-value ratings of 400 pairs of common adjectives, *J. Psychol.*, 27, 1949, 453-463.

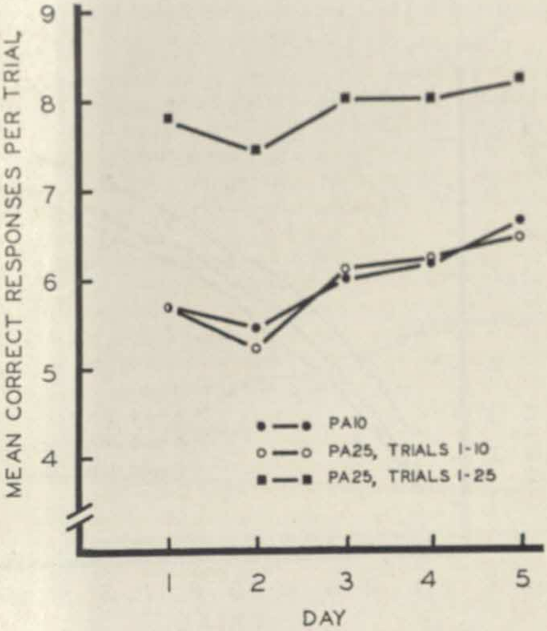

FIG. 1. INTERTASK PERFORMANCE CHANGES IN *PA*-GROUPS

On Days 2 through 5, both *PA*- and *RD*-*S*s were told that the list would be the same kind as before except that new words would be paired with the digits, and that beginning with the trial after the observation-trial they should try to anticipate as many correct responses as possible every trial.

## RESULTS

*Over-all performance.* A gross measure of changes in performance from day to day is mean correct responses (anticipations) per trial on each day. Fig. 1 shows this measure for both *PA*-groups. For Group *PA* 25, the means over Trials 1–10 only are also shown for comparison with the means of Group *PA* 10. All of the curves show the same pattern; there is overt negative transfer from Day 1 to Day 2, and there is over-all learning to learn from Day 1 to Day 5. Using the means for Trials 1–10 only, the loss from Day 1 to Day 2 for both *PA*-groups combined ($N = 50$) was significant; $t$ for related measures was 2.06 ($p < 0.05$). Despite the loss, instead of a gain, from Day 1 to Day 2, it can be seen in Fig. 1 that even by Day 3 there was a slight gain over Day 1. There was no *overt* negative transfer after Day 2 even though $S$ had to learn new responses to old stimuli every day. (Intrusions of old responses from previous days during learning on later days were too few to be worth analyzing.)

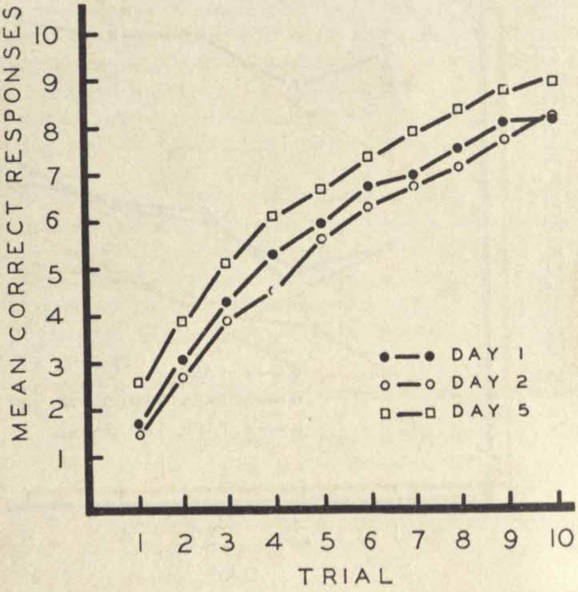

FIG. 2. LEARNING CURVES FOR GROUP *PA* 10

The over-all means of Fig. 1 do not reveal the locus of losses or gains in performance. So Fig. 2, showing learning curves for Group *PA* 10 on Days 1, 2, and 5, is presented. It is clear that the loss, or negative transfer, from Day 1 to Day 2 began on the first trial and persisted throughout the course of practice on Day 2. A similar graph (not presented here) for Group *PA* 25 shows the same picture; in fact, performance on Day 2 was lower than that on Day 1 on almost all of the 25 trials given this group. The learning to learn between Days 1 and 5, shown in Fig. 2, is also not a function of particular trials.

Over-all performance from day to day for *RD*-groups is shown in Fig. 3. Performance is at a considerably lower level than for the *PA*-groups of Fig. 1; note the different values on the ordinates of Fig. 1 and 3. In Fig. 3 there is no evidence of losses between days. The over-all picture is of learning to learn, with the possible exception of Group *RD* 25 in its performance on the first 10 trials of Day 2 versus Day 1.

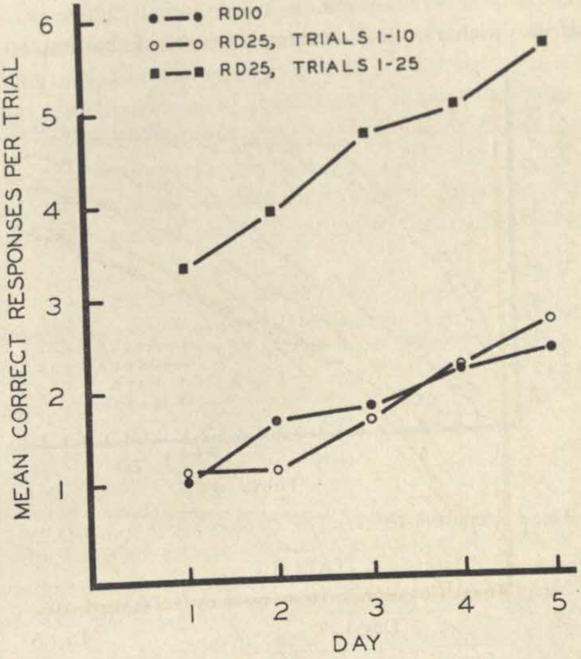

FIG. 3. INTERTASK PERFORMANCE CHANGES IN *RD*-GROUPS

Fig. 4 shows learning curves for Days 1 and 5 for both the *PA*- and *RD*-groups that were given 25 trials per day. These curves show the absolute difference in performance between *PA*- and *RD*-conditions, the relatively small amount of learning to learn in the *PA* group, and the relatively large amount in the *RD* group. It can also be seen that learning to learn is most evident on early trials in the *PA* group, but on later trials in the *RD* group. On the very early trials of any day, however, *RD Ss* were chiefly attempting to discover, rather than to anticipate, correct responses.

Table I shows mean total correct responses for Trials 1–10 only on Day 1 and on Day 5 for both *RD*- and *PA*-groups. None of the differences between the two *PA* groups nor the two *RD* groups was significant within Day 1 nor within Day 5. In fact, there was no evidence on this, or on other measures to be reported later, that 25 trials produced more learning to learn, than 10 trials a day. Because of this, most of the subsequent analyses are based on 10-trial and 25-trial groups combined within *PA*- or *RD*-conditions.

Analysis of variance of total correct responses in the first 10 trials was

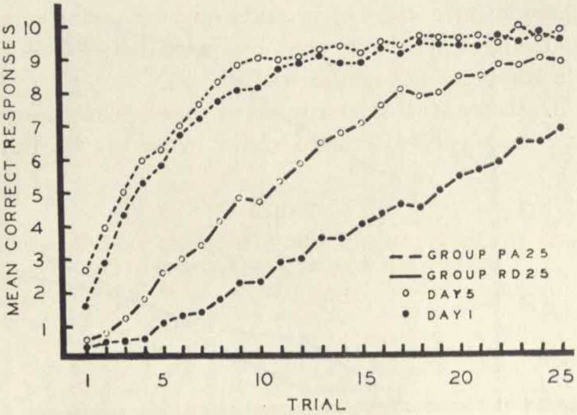

FIG. 4. LEARNING CURVES FOR GROUPS *PA* 25 AND *RD* 25

TABLE I

MEAN TOTAL CORRECT ANTICIPATIONS OVER TRIALS 1–10

| Group | Day 1 |  | Day 5 |  |
|---|---|---|---|---|
|  | Mean | $\sigma_M$ | Mean | $\sigma_M$ |
| *PA* 10 | 57.68 | 3.85 | 66.44 | 2.92 |
| *PA* 25 | 57.68 | 3.55 | 64.68 | 3.25 |
| *RD* 10 | 10.52 | 1.62 | 23.76 | 2.81 |
| *RD* 25 | 11.48 | 2.16 | 26.88 | 3.36 |

used to compare performance from Day 1 to Day 5 of the *PA* and *RD* groups (after combining 10- and 25-trial subgroups so $N = 50$ for each group). As might be expected, both Conditions and Days were highly significant; performance was very much better in *PA*- than in *RD*-condition (F = 274.3, *P* 0.001, 1 and 98*df*), and on Day 5 than on Day 1 (F = 74.3, *P* 0.001, 1 and 98*df*). The term of more interest is the Conditions X Days interaction ($F = 6.25$, *P* 0.02, 1 and 98*df*) ; there was greater improvement over days in the *RD* than in the *PA* condition. This difference in improvement is shown in Figure 4.

*Response-discovery.* One of the purposes of the experiment was to determine if the *S*s show any improvement with practice in discovering or guessing which of several responses is the correct one. The *S*s in *RD*-condition were credited with a correct guess, as distinguished from an anticipation (correct or incorrect), if they waited until the shutter of the drum raised, exposing the three possible response-words, then guessed the correct respones. The data to be presented here are based on recording the trial on which each of the 10 responses was *first* correctly guessed. Table II shows, for the 50 *RD-S*s, the mean total number of responses out of 10 that had received their first correct guess at three stages of practice, viz. at the completion of each of Trials 0, 1, and 2. Trial 0 is the observation-trial; *RD-S*s had been instructed to guess responses on that trial.

Table II shows some intertask improvement in response-discovery in that first correct guesses occurred earlier in practice on Day 5 than on Day 1. The locus of greatest improvement was at the end of Trial 1, *i.e.* after two opportunities to guess; at this point the difference between Day 1 and Day 5 means is 1.04, larger than at any other stage of practice. Although there was a numerical gain (mean difference = 0.36) even on Trial 0, it was not significant; $t$ for related measures was 1.32. After completion of Trial 1, the mean difference yields $t = 3.77$, $P < 0.001$. At later stages of practice the mean differences are smaller than at the end of Trial 1, in part because there were fewer responses, on Day 5, remaining to be guessed.

TABLE II

RESPONSE-DISCOVERY: MEAN TOTAL NUMBER OF FIRST CORRECT GUESSES BY GROUP $RD(N=50)$ AT THREE STAGES OF PRACTICE

| Stage | Day 1 | | Day 5 | |
|---|---|---|---|---|
| | Mean | $\sigma_M$ | Mean | $\sigma_M$ |
| Trial 0 | 3.02 | .24 | 3.38 | .17 |
| Trial 1 | 5.04 | .20 | 6.08 | .19 |
| Trial 2 | 6.12 | .18 | 7.00 | .16 |

Table II shows that there was improvement in response-guessing to the extent that correct guesses occurred earlier in practice on Day 5 than on Day 1. It can also be seen however, that on Trial 0, accuracy of guessing was as good as apparently could be expected. With three possible responses for each of 10 items, the theoretical mean correct guesses after one opportunity to guess would be 3.33. The means for Trial 0 do not differ significantly from this value either on Day 1 or Day 5. (Beyond Trial 0 the *S*s are devoting some of their efforts to anticipating, so mean correct guesses fall below expected values.) Thus, there was no evidence of intertask improvement in accuracy of guessing, presumably because accuracy, on Trial 0, was already at the upper limit on Day 1.

*Response-learning.* Following, in part, Underwood and Schulz,[6] response-learning was defined as the first anticipation of a response regardless of whether or not the anticipation was correct (paired with the correct stimulus). Using as a measure the total number of the 10 responses that had received their first anticipation at the end of the first five trials, an analysis of variance was performed. Conditions (*RD* vs. *PA*) yielded $F = 552.90$ ($P < 0.001$, 1 and 98*df*). Days gave $F = 30.44$ ($P < 0.001$, 1 and 98*df*). The interaction was also highly significant ($F = 12.80$, $P < 0.001$, 1 and 98*df*); again, absolute gain was greater for *RD*-lists. For *RD*-condition, the means were 1.60 and 3.10 for Days 1 and 5, respectively. For *PA*-condition, the corresponding means were 8.22 and 8.50.

TABLE III

RESPONSE-LEARNING: MEAN TOTAL NUMBER OF FIRST ANTICIPATIONS BY GROUP *PA* ($N = 50$) AT THREE STAGES OF PRACTICE

| Stage | Day 1 | | Day 5 | |
|---|---|---|---|---|
| | Mean | $\sigma_M$ | Mean | $\sigma_M$ |
| Trial 1 | 2.42 | .18 | 3.18 | .18 |
| Trial 2 | 4.40 | .27 | 5.28 | .23 |
| Trial 3 | 6.16 | .30 | 6.72 | .24 |

A more detailed analysis of response-learning is presented in Table III which shows the mean total number of the 10 responses that had received their first anticipation at the end of each of Trials 1, 2, and 3. Table IV is based only on the 50 *PA*-*S*s; *RD*-*S*s made very few anticipations, especially on early trials of Day 1. Table III, like Table II, shows both the improvement from Day 1 to Day 5 and the locus of greatest improvement.

In Table III, the gain (mean difference = 0.76) over days is significant even on Trial 1 ($t = 2.98$), but the maximum gain (0.88) had occurred at the end of Trial 2. Thus, the greatest improvement in response-learning over days was measured after two opportunities to anticipate response, the same stage of practice at which maximal gain was measured in response-discovery (Table II).

It was also determined if there was any change over days in the proportion of first anticipations that were correct or incorrect. Using the total first anticipations at the end of Trial 2, the ratio of *incorrect* first anticipations to this total was computed for the 50 *PA*-*S*s. For Day 1 the mean ratio was 0.355; for Day 5, the mean was 0.297. The difference is not significant ($t = 1.24$); at this stage of practice (the results are much the same if one uses the first five trials) about one third of first anticipations were incorrect (mainly overt intralist errors) on Day 1, and this percentage was not significantly less on Day 5.

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[6] Underwood and Schulz, *op. cit.*, 105.

*Associative learning.* The measure of associative learning that was chosen was, for each of the 10 responses, the trial on which the first *correct* anticipation occurred *after* the trial that had been used as the measure of response-learning. (Some responses were incorrectly anticipated early in a trial, correctly anticipated later in the same trial; in these cases the trial number that indexed associative learning was the same as that for response-learning.) By using as a score for each *S* the total number of responses that had met the criterion of associative learning in the first five trials, the usual analysis of variance was performed. As before, both Condition ($F = 417.67$, $P < 0.001$, 1, 98*df*), and Days ($F = 19.76$, $P < 0.001$, 1, 98*df*) were highly significant. But the interaction, Conditions × Days, was not significant ($F < 1$); in this case *RD* and *PA* did not differ in gain over days. For the *RD*-group, the mean was 0.70 for Day 1, 1.78 for Day 5. For the *PA*-group, the corresponding means were 6.00 and 6.96.

TABLE IV

Associative Learning: Mean Total Number of First Correct Anticipations, Following Response-Learning, by Group *PA* at Four Stages of Practice

| Stage | Day 1 |  | Day 5 |  |
|---|---|---|---|---|
|  | Mean | $\sigma_M$ | Mean | $\sigma_M$ |
| Trial 1 | .40 | .09 | .40 | .10 |
| Trial 3 | 3.34 | .27 | 4.16 | .26 |
| Trial 5 | 6.00 | .32 | 6.96 | .28 |
| Trial 6 | 7.16 | .31 | 7.86 | .27 |

Since *RD*-*S*s accomplished so little associative learning on early trials, only the *PA*-*S*s were used for a trial-by-trial analysis of associative learning similar to that for response-learning. Table IV shows mean total number of the 10 responses that had met the criterion of associative learning at the end of some of the first six trials. It is necessary to examine the means through Trial 6 in order to reveal the over-all pattern of improvement. Comparison of Day 1 and Day 5 means in Table IV shows that there was no gain at the end of Trial 1. (As a consequence of the measure used, gains in associative learning will necessarily occur somewhat later in practice than gains in response-learning, but at least a slight gain in associative learning could have occurred even on Trial 1.) By the end of Trial 2 the gain (mean difference = 0.62) was significant; $t = 2.86$. But the maximal difference between Day 1 and Day 5 did not occur until after five trials (mean difference = 0.96). Thus, as compared with response-learning, the point of maximum gain over days in associative learning was rather late, viz, after five opportunities to anticipate.

## DISCUSSION

When *S*s were required to learn new responses to old stimuli in a series of lists (*PA*-groups), performance over successive lists appeared to be a joint function of an interfering factor and a facilitating factor. The interference is inferred from the overt negative transfer that occurred from Day 1 to Day 2, and is presumably the result of competition between responses associated to the same stimulus. Such competition is usually expected with the A–B, A–C paradigm. The facilitating factor is that non-specific skill or skills, typically called learning to learn, that is acquired from repeated practice with similar tasks.

The problem is to account for the way the interfering and facilitating factors interacted to produce the pattern of loss and gain shown in Fig. 1. One possibility stems from noting that in the Greenberg-Underwood study, the amount of learning to learn was not great, at least not in comparison to other types of paired-associate tasks.[7] The Greenberg-Underwood study, in which A–B, C–D lists were used, should have been free of interference among the responses of successive lists. Thus, when both the stimuli and responses of successive paired-associate lists are familiar and meaningful, as in this and in the Greenberg-Underwood study, intertask gains in performance may be small in any case. This would mean that in the present study, the effects of interference could mask learning to learn initially, *e.g.* on Day 2. But eventually the slight but gradually increasing effects of learning to learn should become overt. In fact, learning to deal with or minimize competition among the responses of successive lists may well be part of learning to learn.

It is suggested that the main reason there was little intertask improvement with *PA*-lists is to be found in the nature of the stimuli and the responses of the lists. Both the stimuli (digits) and the responses (low-similarity common adjectives) were not only both meaningful and familiar to *S*, but should also have been highly differentiated. (Total errors, intra-, inter-, and extra-list, were few.) Since there was no need to differentiate among stimuli or among responses, there was less opportunity to learn nonspecific skills of differentiating, skills which elsewhere have been claimed to be important components of learning to learn.[8] (In the paper just cited, the prediction had been offered that learning to learn would be small for either high meaningful or low intralist similarity tasks.) Thus, it is suggested that with *PA*-lists of this type *S*'s task consisted very largely of the correct pairing of stimuli and responses. All of the data suggest that correct pairing, the associative process *per se,* is very little subject to learning to learn.

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[7] Greenberg and Underwood, *op. cit.*, 456; Duncan, 1958, *op. cit.*, 66; Duncan, 1960, *op. cit.*, 109.

For the *PA*-condition, the results with the two stages, response-learning and associative learning, were similar to the findings for over-all learning; there were statistically significant, though numerically slight, gains over days. For both stages these gains took the form that a particular level of performance was achieved earlier in intratask learning on Day 5 than on Day 1. An intertask gain at some stage of practice in the measure of response-learning (first anticipations, correct or incorrect) means that the *S*s made a greater total number of attempts to give the initial anticipation of at least some of the responses at that stage of practice. But this greater number of attempts at first anticipations on later days was *not* accompanied by a significant increase in the proportion of these anticipations that were correct. This is another indication that that portion of a *PA*-task which consists of correctly pairing the stimuli and responses is relatively resistant to learning to learn. In partial contrast to response-learning, the intertask gain in the measure of associative learning was very slight on early trials, and did not reach its maximum, though small, amount until the *S*s had had five opportunities to correctly anticipate responses. Thus, there was, in fact, some improvement in association *per se,* but the gain was small and slowly achieved.

With the *RD*-lists, evidence for intertask gains appeared both in response-guessing, and in over-all anticipation-learning. With regard to initial guessing, discovery of correct responses, the form that the intertask gain took was the same as that reported above for response-learning and associative learning in *PA*-lists. A given level of performance in response-guessing was achieved in earlier trials on Day 5 than on Day 1. But there was no evidence of improvement in accuracy of guessing, as measured by the number of responses guessed correctly on the observation-trial (where *S* had only to guess responses). In this respect the *RD*-lists, the response-guessing stage of which is considered to be a form of simple problem-solving, yielded results similar to the lack of improvement in response-discovery reported in some problem-solving studies.[9] It should be noted, however, that even on Day 1, accuracy of guessing on the observation-trial was as good as presumably could be expected, and remained at this level on each of the subsequent four days.

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[8] Duncan, 1958, *op. cit.*, 71.

The over-all intertask gain, measured by mean total correct anticipations, was greater, in absolute amount, for the *RD*- than for the *PA*-lists. The *RD*-Condition also showed greater improvement than the *PA*-Condition in response-learning, but not, it is worth noting, in associative learning. It is suggested that because the *RD*-lists required *S* to do more things, *i.e.* guess as well as anticipate, there were more opportunities to learn non-specific skills. Thus, the relatively small intertask gain found with *PA*-lists is attributed chiefly to the lack of variety or complexity (as distinct from difficulty) of the task; such lists appear to demand mainly associative learning, a process which seems to be little affected by intertask practice. The relatively large intertask gain found with *RD*-lists is attributed to the presence of complexity; the greater the variety of demands that tasks make on *S*, the greater the likelihood that *S* will develop generalizable skills to deal with the tasks.

## SUMMARY

In a study of learning to learn with verbal lists, the *S*s learned a different 10-item digit-adjective list each day for five days. Half the *S*s learned paired-associate lists (*PA*-lists): one response per stimulus. The remaining *S*s learned response-discovery lists (*RD*-lists): three possible responses, only one of which was correct, per stimulus. Within each of these two conditions, half the *S*s were given 10 trials, half given 25 trials, on each of the five lists.

There were no differences, on any measure, attributable to differences in amount of intralist practice. With the *PA*-lists, there was a loss (negative transfer) from Day 1 to Day 2, but an over-all gain (learning to learn) from Day 1 to Day 5. The amount of learning to learn, measured by gain in total correct anticipations per list, was small. Small intertask gains also occurred in response-learning, measured by first anticipations, and in associative learning, measured by first correct anticipations. Proportion of first anticipations that were correct did not show significant intertask improvement.

With the *RD*-lists, intertask gains in guessing the correct response were

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[9] Duncan, 1959, *op. cit.*, 420; Duncan 1961, *op. cit.*, 40.

shown in the form of more correct guesses on earlier trials of later days. Accuracy of guessing was apparently maximal on Day 1, and did not improve. As indexed by total correct anticipations, *RD*-lists showed considerable over-all learning to learn. The difference in over-all intertask gain between *PA*- and *RD*-conditions was attributed to the greater complexity, rather than the greater difficulty, of *RD*-lists; it was assumed that greater complexity provides the opportunity to learn nonspecific, transferable skills.

# THE DISCRIMINANT-FUNCTION AS A MODEL FOR PERCEPTION

By ALBERT S. RODWAN and HAROLD W. HAKE,
The University of Illinois

A broad area of perceptual theorizing is exemplified by Brunswik,[1] Bruner,[2] Bartlett,[3] and others,[4] who maintain that the perceptual system is, basically, a judgment-making system. These theorists concentrate on the functional relationship between the distal variable (the physical object) and the perceptual response. Points of similarity among their views are: (a) the *O* does not utilize all aspects of the information in the distal variable; (b) the perceptual response is mediated by some hypothetical process;[5] and (c) perception is probabilistic or statistical in that the functional relationship between the distal variable and the mediating process, Brunswik's 'ecological validity' of cues, is neither straightforward nor perfect.

A judgmental theory implies that perception is a judgment-making process; that is, the perceptual system operates on the input-information in making a judgment or inference. This inference is one of two types; either it is an estimation of some aspect of the physical object, or it is the classification of the object into a perceptual category. In either case, the major task of the perceptual system is to classify inputs, since estimation presumably cannot take place until the object is classified.

If this summary analysis is correct, most of these theories can be fitted

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[1] Egon Brunswik, *Perception and the Representative Design of Psychological Experiments,* 1956; for a lucid presentation of Brunswik's theorizing, see Leo Postman and E. C. Tolman, Brunswik's probabilistic functionalism, in S. Koch (ed.), *Psychology: A Study of a Science,* Vol. I, 1959, 504-564.

[2] Although Bruner has been very prolific, he incorporates most of his theorizing in: J. S. Bruner, On perceptual readiness, *Psychol. Rev.,* 64, 1957, 123-152.

[3] F. C. Bartlett, *Remembering: A Study in Experimental and Social Psychology,* 1932, 186-227.

[4] For the classification of perceptual theories, especially as related to perceptual learning, see James Drever, Perceptual learning, *Ann. Rev. Psychol.,* 11, 1960, 131-160.

[5] The process of mediation for Brunswik was the distributions of energy impinging on the receptors. These he called cues. Bruner also uses the word 'cue' but in a much more general sense; that is, for Bruner, a cue is something that gives the *O* information. Bartlett, on the other hand, implies that the mediating process is a concept, a schema, which the *O* has learned.

by one general mathematical model; the differences among the theories will then be differences either in the mediating process or in the coördinating definitions. The rest of this paper is concerned with the presentation of a general model and two experiments designed to test it.

*The model.* Fisher's Linear Discriminant Function[6] (*LDF*) can be considered as a solution to the following problem: given two categories and $m$ individuals measured on $n$ variables, find a linear function of the $n$ variables which allows for the maximal discrimination between the two categories. This function is then used to classify future individuals into one of the two categories. The following example may help to clarify this. Prospective students (the $m$ individuals) applying for admission to college are given a battery of tests (the $n$ variables). The admissions committee has, on the basis of previous data, defined two categories into which all prospective students are assigned: those who will successfully complete college and those who will not. The problem is, then, on the basis of his scores on the examination, into which category the applying student is to be assigned. This is the sort of problem the *LDF* was designed to handle. It is important to note that the two categories, 'passers' and 'failers,' are defined statistically since there will, in general, be some overlap or confusion. If we denote individuals who pass by $Y_b$ and those who fail by $Y_a$, then $Y_b$ has a multivariate distribution, $f_b(y)$, and $Y_a$ has a multivariate distribution, $f_a(y)$.

There exists a formal similarity between the problems which the *LDF* was designed to solve and the problems involved in complex perceptual judgments. Consider the problem facing the admissions committee and that facing an *O* who must decide whether or not a particular face is 'intelligent.' In both situations, there are two categories into which the input is placed, and each input has $n$-dimensions. Furthermore, in both situations, the goal is to discriminate maximally between the two categories. In the case of the *O*, two concepts are involved: *intelligent* and *non-intelligent*. Each may be defined in terms of several physical dimensions which specify the faces being judged. For each physical dimension there may or may not be a psychological correlate or attribute. Analogously, the tests used by the admissions committee may, or may not, aid in classifying the prospective students. This parallelism is sufficiently striking to justify the questions: Will the *LDF* serve as an adequate model for complex perceptual judgments, and can the *LDF* be derived from psychological assumptions?

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[6] R. A. Fisher, The use of multiple measurements in taxonomic problems, *Ann. Eug.*, 7, 1936, 179-188.

The following three assumptions are necessary and sufficient to derive the *LDF:* $A_1$, a concept is defined as a region in an *n*-space if it has *n* attributes; $A_2$, an *O* uses a linear combination of the *n* attributes to form the *LDF;* $A_3$, the *LDF* is so oriented that it will maximize the distance in the *n*-space between any two concepts (regions).

*Assumption $A_1$.* A stimulus-object with *n* physical dimensions can be considered as a point in an *n*-space if it has *n* attributes, *i.e.* if each physical dimension has a psychological correlate. For example, a color has three psychological attributes (hue, luminance, and saturation) and is a point in a 3-space; a tone has at least two attributes (pitch and loudness) and is a point in at least a 2-space, a plane. Similarly, if a rectangle has two attributes (subjective width and subjective height), it is a point in a plane. Some physical dimensions of a stimulus have no psychological correlate. In audition, for example, there is no correlate of phase-angle; that is to say, if two tones differ by only the phase-angle, a human *O* will not distinguish between them if they are presented successively. A particular subset of stimuli forms a subregion in the *n*-space. Thus, if luminance is decreased beyond a certain value, the color-space reduces to a line.

Assumption $A_1$ is formalized as follows: a concept is a region in an *n*-space and is defined by a multivariate probability-distribution in that space. The mean of this distribution is the centroid of that concept (subregion). Furthermore, as a stimulus (point) moves farther away from the centroid, the probability that it is an instance of that concept (belongs in that region) decreases as a function of the distance from that stimulus to its mean. The metric of this space is variance-units. From this definition of a concept, the distance between two concepts, *d,* is defined as the distance between their means, in variance-units.

*Assumption $A_2$.* The second assumption is that the more complex the stimulus, *i.e.* the more attributes it has (or, the higher the dimensionality), the less *O* attends to any particular attribute. Consider, for example, the class of rectangles. If an *O* is asked to discriminate between squares and rectangles, he may be able to attend to both the height and width separately. On the other hand, in the case of Brunswik and Reiter's experiment on the intelligence of schematic faces,[7] it is doubtful that an *O* could attend separately to each of the attributes that constitute his criterion for 'intelligence.' The assumption made in this paper is that in both of these cases an *O* does not attend separately to all the attributes; he acts as if he uses a linear combination of them, a weighted linear combination. The weights are assigned according to the value of that attribute in making the decision.[8] This assumption is illustrated in Fig. 1, where a 2-space is used for simplicity.

Looking at Fig. 1, we see that if the *O* uses only the axis marked $X_1$, or Axis $X_2$, as a decision-axis, he cannot discriminate between $\bar{Y}_a$ and $\bar{Y}_b$ as well as if he used both. But on the assumption that he does not attend to both individually, he uses a linear combination of them which is, in effect, inserting a decision-axis in the plane and projecting all the points (stimuli) in the two regions (concepts) onto it. The two lines $L_1$ and $L_2$ represent two possible axes that the *O* could use. It appears that

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[7] Egon Brunswik and Lotte Reiter, Eindruckscharaktere schematisierter Gesichter. *Z. Psychol.,* 142, 1938, 67-134.

[8] Brunswik uses the same language, see Postman and Tolman, *op. cit.,* 504-564.

$L_2$ is a better decision-axis than is $L_1$. The problem is to find what it is that makes $L_2$ better. Earlier, it was stated that the distance between concepts is given by the difference between their centroids; according to that criterion, $L_2$ is a better decision-axis, because the projection of this difference on $L_2$ is greater than it is on $L_1$. Mathematically, maximizing the variance between the two means, *i.e.* between *Var.*

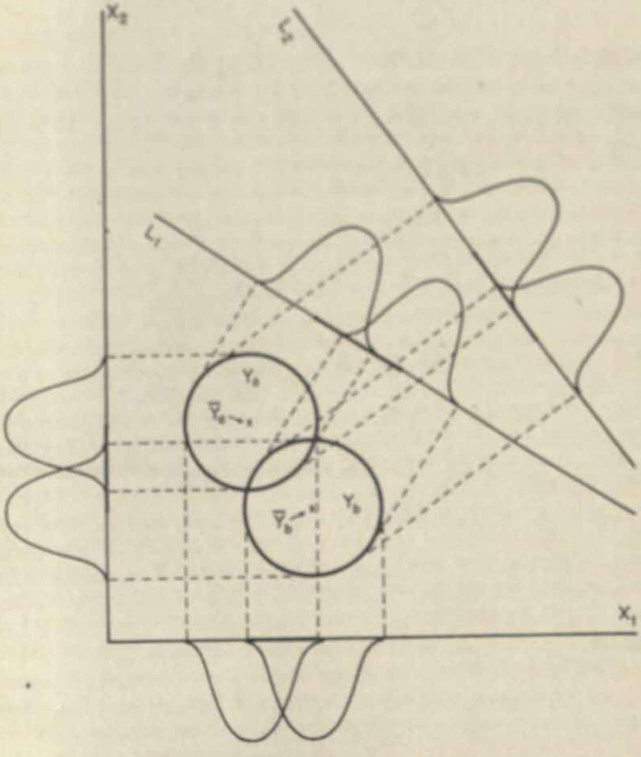

FIG. 1. A 2-SPACE AND TWO CONCEPTS DEFINED AS AREAS IN IT
$L_1$ and $L_2$ are two decision axes. See text for discussion.

$(\bar{Y}_b - \bar{Y}_a)$, maximizes this difference. Furthermore, since the unit of measure is the variance within-concepts, we should also minimize the variance within each concept.

*Assumption $A_3$.* A decision-axis is a weighted linear function of the $n$ attributes (the behavioral analogue of the *LDF*). The weight attached to each attribute is the value that $O$ has learned belongs to that attribute when he assigns an $n$-dimensional stimulus to a particular catagory, *i.e.* these weights are the reliabilities of the cues. In the study of Brunswik and Reiter on the intelligence of schematic faces, it was reported that height of forehead was by far the strongest attribute in the judged intelligence of the faces. We take that to mean that the weight attached to height of forehead was greater than the weight attached to any other attribute. The crux of the problem is to find the weights which will maximize the distance between two centroids. That is, we want to find a weighted linear function of the $n$ attributes which will discriminate better than any other linear function between the two subregions (concepts). We denote the $n$ attributes by $X_1, X_2, \ldots X_n$, and the two regions by $a$, and $b$. The linear function $Y$ has weights $v_1, v_2, \ldots v_n$, with the restriction that the sum of the $v$s after they are squared equals unity, and such that the distance, $d$, previously discussed, is maximized. This function is:

$$Y = v_1X_1 + v_2X_2 + \cdots v_nX_n \qquad [1]$$

The multivariate mean for concept $a$ is $\bar{Y}_a$ and for $b$ it is $\bar{Y}_b$; these can be computed by means of:

$$\overline{Y}_a = v_1\overline{X}_{1a} + v_2\overline{X}_{2a} + \cdots + v_n\overline{X}_{na} \qquad [2]$$

$$\overline{Y}_b = v_1\overline{X}_{1b} + v_2\overline{X}_{2b} + \cdots + v_n\overline{X}_{nb} \qquad [3]$$

where $\bar{X}_{1a}$ is the mean of concept $a$ on the first attribute, etc. The distance between the multivariate means is $\bar{Y}_b - \bar{Y}_a$ which is given by:

$$\overline{Y}_b - \overline{Y}_a = d = v_1d_1 + v_2d_2 + \cdots + v_nd_n \qquad [4]$$

where $d_1$ is equal to $\bar{X}_{1b} - \bar{X}_{1a}$, etc.

The *LDF* as defined may be meaningfully compared with a multiple-regression equation. The latter is a linear function which maximizes the correlation between a criterial variable and the weighted sum of $n$ predictor-variables. The *LDF*, on the other hand, is a weighted sum which maximizes the discrimination between two concepts. The multiple regression gives maximal predictability while the *LDF* gives maximal discriminability. In both cases the problem is to determine the optimal weights. For the correlational case, we want the 'beta' weights while for the *LDF* we want the 'discriminant' weights, and these two sets of weights are, as might be expected, related. We use the same procedure in both cases to compute the desired weights. That is, we need the within-concepts covariance-matrix. Thus, we need the sum of squares and sum of cross-products among all the attributes; for example, the sum of squares for attribute $X_1$ is:

$$SS_1 = [N_bX_{1b}^2 - (X_{1b})^2]/N_b + [N_aX_{1a}^2 - (X_{1a})^2]/N_a = w_{11} \qquad [5]$$

while the sum of cross-products between $X_1$ and $X_2$ is:

$$SP_{12} = [N_bX_{1b}X_{2b} - X_{1b}X_{2b}]/N_b + [N_aX_{1a}X_{2a} - X_{1a}X_{2a}]/N_a = w_{12} = w_{21} \qquad [6]$$

In general $w_{ij}$ is the cross product between attributes $X_i$ and $X_j$. To determine the weights, the $v$s, we construct the 'within' matrix which contains all the $w$s, that is, the matrix of variances and covariances. This matrix we denote by $W$. The weights, the $v$s, are then given by:

$$v = kW^{-1}d \qquad [7]$$

where $k$ is a constant of proportionality, $W^{-1}$ is the inverse of $W$, and $d$ is the set of $d$s $(d_1, d_2, \ldots d_n)$ previously defined. It would be much too laborious to compute the weights from this last equation. A simpler schema is available if the dimensions in the stimuli are all orthogonal to one another. In that case the equation reduces to:

$$v = kM_b \qquad [8]$$

where $M_b$ is the vector of means in deviation scores of $Y_b$, and $k$ is the same constant of proportionality. We can then determine $k$ from the restriction that the sum of the weights, when squared, must equal unity. In that case:

$$v'v = 1 = k^2M_b'M_b \qquad [9]$$

$$k = 1/(M_b'M_b)^{\frac{1}{2}} \qquad [10]$$

Let us return to the admissions committee. After they set up the rules for admission, the *LDF*, there is still another problem: How strict are they to be in the requirements for entrance? The stricter the criterion, the fewer failers will enter college; on the other hand, more potential successes will be rejected. As reasonable men they will probably use a somewhat flexible criterion. During periods of heavy applications they will use a more strict criterion, and when the rate of applications decreases the criterion will be somewhat relaxed. This can be done by use of the same *LDF;* the only quantity free to vary is the cutoff separating acceptable from non-acceptable applicants.

The same problem exists in complex perceptual judgments. For example, if an *O* is to decide which of a set of faces is intelligent, only the facial geometry ought to determine the decision-axis. Thus, whether the faces are seen on a college-campus where there are, presumably, more intelligent faces or in, say, a prison, the *LDF* ought to remain invariant, and only the cutoff should vary. This follows from almost all judgmental theorists who suppose that *O* infers or makes bets concerning the nature of the stimulus, *i.e.* where it belongs. If this is true, then a rational process of this sort is possible; but the process should involve the location of the cutoff on the decision-axis, and not the location of the axis itself. The decision-axis for a given judgment must be determined entirely by the nature of the sensory input; the *O* judges only whether it is 'X' or 'not-X.'

Grant for a moment that an *O* does have a constant decision-axis. When he changes his cutoff, he simply moves along that axis. In that case the *LDF* can answer several questions. First, can the distance between the two multivariate means be attributed to chance fluctuations, *i.e.* are the two concepts discriminably different? Secondly, if this distance is not zero, what is it? Third, and most important, if the answer to the first question is yes, the *LDF* allows us to say how an *O* combines the different attributes of a complex judgment, *i.e.* how he weights each attribute in making his judgment.

*Validation of the model.* Earlier, the question of whether or not the *LDF* could be derived from perceptual assumptions was asked and answered affirmatively. Another question raised was whether or not the *LDF* could serve as an adequate model for perception. The adequacy of the *LDF* is defined in terms of its invariance. That is, if the *LDF* is constant, and this can happen only if the two distributions (concepts) are fixed relative to one another in the *n*-space, then it can serve as a model. There is only one test for this invariance, and that is the coefficient of correlation. This follows, since the weights of the *LDF* are proportional to the directioncosines, they determine the orientation of the *LDF* in the *n*-space. Therefore, the *LDF* is invariant if and only if the weights obtained from two conditions are themselves proportional to one another. The first test (Experiment I) then, is: When the weights for the *LDF* are derived from one set of data, *i.e.* from the use of a particular cutoff, how well do the same weights fit data derived from different cutoffs? That is to say, are the weights derived from different cutoffs proportional to one another? This question, if answered affirmatively, would show that only the cutoffs vary, while the *LDF* remains constant. The cutoff was taken to be half the distance between the two centroids. A second test of the adequacy of the *LDF* would be to investigate the invariance of the *LDF* over relatively long durations of time, if it proved to be invariant for short durations.

The two experiments to be reported were variants of the study of Brunswik and Reiter.[9] In the first experiment, the *O*s judged the faces under three conditions defined by three different relative proportions of intelligent faces in a set of faces. These proportions forced the *O* to shift his cutoff, *i.e.* led him to respond "intelligent" with a certain frequency. The crucial questions become: Given his responses for each condition, (1) can we show that the two concepts (intelligent and non-intelligent) are discriminably different, (2) can an *LDF* be written for the judgments in each condition which allows us to reproduce his responses, *i.e.* how well does the *LDF*, including criterion, agree with the *O* in classifying faces, and (3) will the *LDF*s for the three conditions be the same, *i.e.* be proportional?

## EXPERIMENT I

*Stimuli.* The stimulus-objects were 81 photographs of schematic faces. Each photograph (face) varied along four dimensions: length of nose, $X_1$; length of chin, $X_2$; distance between the nasal edges of the eyes, $X_3$; and length of forehead, $X_4$. The only other component of the face was the mouth and its length was constant at 13.6 mm. The long axis of the face was 60 mm. from the tip of the chin to the top of the forehead. The other axis, taken at the widest part of the face, was 37.5 mm. Each face was mounted on a 6 × 9-in. sheet of white cardboard.

*Procedure.* Three men (students) served as *O*s; none had ever participated in a psychophysical or perceptual experiment. *O* was seated approximately 30 in. away from a 24-in. square piece of wood which was painted a flat white. *E* presented the photographs one at a time, against the white background. The duration of viewing was between 3 and 5 sec. and was followed by a 5-sec. interval during which *O* responded and *E* recorded the response. To control for biases, the 81 photographs were randomly assigned to 3 sets of 27 each, denoted by A, B, and C. Each set was represented to *O* as containing one of three ratios of intelligent to non-intelligent faces. Thus there were nine conditions: A-25%, A-50%, A-75%; B-25%, B-50%, B-75%; and C-25%, C-50%, C-75%. Each *O* went through the nine conditions.

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[9] Brunswik and Reiter, *op. cit.*, 71.

Each condition was presented at a different time, and the conditions were presented in a random order. As a result of this procedure, each *O* judged each face under the 25%, 50%, and 75% conditions. Thus, the total number of judgments was 3 (sets) × 81 (number of faces), or 243.

The following instructions were read to each *O* at the beginning of the first condition:

> I have here a set of drawings of faces. These faces were drawn according to certain specifications; namely, that some facial characteristics of intelligent faces and some of not so intelligent faces be represented. You will be presented with a face and are to decide whether the face is intelligent or not. If you think it is, you are to say 'yes,' otherwise say 'no.' Do you have any questions?

When *E* was satisfied that *O* understood the instructions, he told *O* what the ratio of intelligent to non-intelligent was for that condition and showed *O* a sample-face.

TABLE I

THE VALUES OF THE FOUR VARYING DIMENSIONS

Entries in m.m.

| Dimensions | | | |
|---|---|---|---|
| $X_1$ | $X_2$ | $X_3$ | $X_4$ |
| 11.7 | 6.7 | 6.7 | 11.3 |
| 13.3 | 10.0 | 10.0 | 14.6 |
| 15.0 | 13.3 | 13.3 | 17.9 |

Before each condition, *O* was told what the ratio of intelligent to non-intelligent faces was for that condition.

*Results.* There are two methods of analysis available; one is a direct test for invariance, while the other is more indirect but yields more information. The direct test is the correlational approach: if the *LDF*s for the three sets of data are proportional to one another, the weights ought to be perfectly correlated. This would demonstrate invariance. If the correlations are zero, for example, the *LDF*s would be independent of one another. The correlational approach, however, does not give any index of the adequacy of the fit of the *LDF*. That is to say, for any *O* the *LDF*s may be perfectly correlated and yet not give a good fit to the data. For this reason the second more indirect test was made in which only one *LDF* was derived for each *O* and was used to assign faces to intelligent and non-intelligent categories for all three conditions. This tests directly the adequacy of the fit of a stable *LDF* for all conditions.

Using Equation [8] we derived the weights shown in Table II. From this table the *LDF* for each *O* can be written. For example, the *LDF* for *JL* is $-0.2060\,X_1 + 0.6181\,X_2 - 0.0687\,X_3 + 0.7555\,X_4$. This function was derived from the 50% set of judgments, as were all the *LDF*s in this table. Table III shows, again using *JL* to illustrate, that when *JL* was told in a particular condition, that 75% of the faces were intelligent, he assigned 71% of those faces to the intelligent category. The rest of the table gives similar information for all three *O*s. Table IV gives the percentage of errors, obtained and expected (on the assumption of a constant *LDF*) for each *O*'s *LDF* for each category. That is, using the *LDF* in Table II, each face was reassigned to a category. A 'hit' for the *LDF* occurs when the *LDF* and the *O* assigned the same face to the same category under the same conditions; an error occurred when the *LDF* misclassified a face, *i.e.* did not agree with the *O*. Since there is no known criterion for the intelligence of schematic faces, the only criterion against which to judge the *LDF* was *O*'s judgments. It should be noted that the errors are approximately uniform in Table IV and that the *LDF* does much better than chance. A 2 × 2 contingency table with the *LDF*'s classification of the faces into intelligent and non-intelligent faces vs. the judgments of *O* yielded a Chi-square that was significant at the 0.0001 level. Thus we can accept the *LDF* as being an adequate model, *i.e.* it is invariant, and fits well.

TABLE II

THE WEIGHTS FOR THE *LDF* FOR THREE *O*S IN EXPERIMENT I

| | *O*s | | |
|---|---|---|---|
| Dimensions | *JL* | *MF* | *JJ* |
| $X_1$ | −.2060 | .1639 | .0878 |
| $X_2$ | .6181 | .1224 | .3073 |
| $X_3$ | −.0687 | −.0408 | −.2195 |
| $X_4$ | .7555 | .9796 | .9218 |

TABLE III

THE PROPORTIONS OF THE JUDGMENTS IN EACH OF THE THREE SETS OF DATA FOR THE THREE *O*S IN EXPERIMENT I

| *O*s | 25% | 50% | 75% |
|---|---|---|---|
| *JL* | .36 | .52 | .71 |
| *MF* | .31 | .48 | .60 |
| *JJ* | .37 | .51 | .69 |

TABLE IV

THE OBTAINED AND EXPECTED PERCENTAGE OF ERRORS ASSUMING A CONSTANT DECISION-AXIS IN EXPERIMENT I

| *O*s | 25% | 50% | 75% |
|---|---|---|---|
| *JL* | 5.69(4.27)* | 1.42(4.27) | 5.69(4.27) |
| *MF* | 4.89(5.13) | 4.44(5.13) | 6.22(5.13) |
| *JJ* | 7.91(6.66) | 6.66(6.66) | 5.83(6.66) |

* The expected number of errors in parentheses.

## EXPERIMENT II

*Procedure*. Three new *O*s, all women, were used in this experiment. These three *O*s were asked to assign the same faces used in Experiment I to either an intelligent or non-intelligent category. This task was repeated one week later, and in the third week each *O* was asked to assign each face into a 'stupid' or 'non-stupid' category. None of the *O*s expected to be asked for her second judgment, but all three expected the third. However, they expected a simple repetition of the judgments of intelligence, not of judgments of 'stupid' or 'non-stupid.'

TABLE V

THE *LDF* FOR THE THREE *O*S IN EXPERIMENT II

($I_1$ denotes the first judgment of intelligence; $I_2$ denotes the second judgment, and S denotes the judgment of stupid.)

| | *CD* | | | *KW* | | | *MT* | | |
|---|---|---|---|---|---|---|---|---|---|
| | $I_1$ | $I_2$ | S | $I_1$ | $I_2$ | S | $I_1$ | $I_2$ | S |
| $X_1$ | −.0457 | .0476 | .0001 | −.0485 | .1063 | −.1123 | .1365 | .0479 | .0001 |
| $X_2$ | −.8690 | −.9038 | .9668 | −.7752 | −.7972 | .9357 | .3640 | .3836 | −.2194 |
| $X_3$ | −.1830 | −.1903 | .2522 | −.2423 | −.2657 | .1497 | −.3185 | −.1438 | .2149 |
| $X_4$ | −.4570 | −.3806 | .0420 | −.5824 | −.5315 | .2994 | .8645 | .9109 | −.9508 |

TABLE VI

THE OBTAINED AND EXPECTED PERCENTAGE OF ERROR IN EXPERIMENT II

(The letters $I_1$, $I_2$, and S are explained in Table V.)

| *O*s | $I_1$ | $I_2$ | S |
|---|---|---|---|
| *CD* | 6.47(7.26)* | 7.65(7.26) | 7.65(7.26) |
| *KW* | 10.03(6.16) | 4.21(6.16) | 4.25(6.16) |
| *MT* | 1.57(3.37) | 4.55(3.37) | 3.99(3.37) |

* The numbers in parentheses are the expected percentages on the assumption of a constant decision-axis.

TABLE VII

THE PRODUCT MOMENT CORRELATION COEFFICIENTS AMONG THE THREE JUDGMENTS ($I_1$, $I_2$, AND S) IN EXPERIMENT II

| *r* | *O*s | | |
|---|---|---|---|
| | *CD* | *KW* | *MT* |
| $r_{I_1I_2}$ | .994 | .997 | .981 |
| $r_{I_1S}$ | .847 | −.941 | −.970 |
| $r_{I_2S}$ | −.938 | −.957 | −.981 |

The reasons for the second experiment were: first, the importance of providing a replication of the first experiment; secondly, and equally important, it was necessary to provide a check on the reliability and stability of the *LDF* over larger intervals of time. Since one of the assumptions is that the *LDF* is constant (only the cutoffs vary), a test-retest kind of check is a critical procedure of validation.

The results of the second experiment are given in Tables V, VI, and VII. Table V gives the *LDF*s for the three *O*s, for the three judgments. Table VI gives the expected (on the basis of a constant decision-axis) and obtained percentages of error; and Table VII gives the product-moment correlation coefficients among the three judgments for each *O*. Table VI is to be compared to Table IV, and the two, in conjunction, tend to confirm the proposition that the *LDF* is constant. Table VII is further confirmation.

*Discussion.* The ability of the *LDF* to 'fit' the data from the two experiments as well as it does confirms the three assumptions used in deriving it; (a) a concept can be defined as a region in an *n*-space if it has *n* attributes; (b) the *O* uses a linear combination of the attributes to form his 'decision-axis'; and, (c) the decision-axis is oriented in order to optimize his behavior by maximizing the 'distance' between the two concepts. An implicit, but very important and far-reaching, implication is that from these three assumptions it follows that the perceptual system involved here was a stable, stationary system which did not change over time.

The three assumptions are not novel and have been accepted in other contexts.[10] The implication of stationarity, on the other hand, has never been demonstrated in perception before, although it has been suggested by Gestalt theorists. The power of the implication of stationarity can be illustrated by an example from probability-learning theory. The Markovian model in probability-learning theory assumes that the probability of responding is a constant and is not affected by the number of the trial.[11] In perception, the assumption of stationarity entails that the concepts be fixed, relative to one another, and that all judgments with respect to these concepts are made on the basis of decision-axes which are proportional to one another. Consider, for example, the case when the same set of stimuli, the faces in this experiment, are presented to an *O* and he is asked to make judgments of 'intelligence' and of 'likeability.' If the decision-axes are proportional to one another, and if we assume stationarity, then we can assert that these two apparently different judgments are actually one and the same. It is important to note that the assumption of stationarity is necessary in order to assert equality of judgments from the proportionality of the decision-axes.

It can be seen from Table II that the *LDF*s for three *O*s are quite similar in both magnitude and sign. The only 'inversion' of signs occurs for *JL* on length of nose. For him this attribute is negatively related to intelligence but it is positively related for the other two *O*s. For the other attributes the three *O*s agree completely. The similarity among the *LDF*s extends to more than signs, however, in that the dominant variable for all *O*s is the length of forehead. The mean correlation among the three *LDF*s is approximately 0.80; and, if this were used as an index of similarity, it would imply a great deal of common cultural learning. The similarity among the *LDF*s in Table II should not be taken too seriously, however, since they are quite different from the *LDF*s in Table V. Only further work will determine the extent of similarity and differences in the culture with respect to 'intelligence.'

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[10] Osgood has defined concepts in the same way; see C. E. Osgood, and G. Succi, *The Measurement of Meaning,* 1958, 15-37. Similarly, these assumptions form the basis for factor analysis.

[11] Patrick Suppes and R. C. Atkinson, *Markov Learning Models for Multiperson Interactions,* 1960, 1-46. In this book the authors use probability-learning theory as a model for social psychological phenomena.

Regardless of the fluctuations among individuals, the striking thing is that within an individual there is a great deal of stability. This is shown in Table IV for Experiment I and in Tables V and VII for Experiment II. Table IV shows that the worst fit by the *LDF* of the *O*'s judgments in Experiment I was about 92%, while Table VI shows about the same degree of accuracy. The data in Table IV, in a sense, forced us to undertake Experiment II. The very high agreement led us to suspect that some artifact was operating. This is less likely after seeing Table VI. The possibility does exist that the goodness of fit may be due to the task. That is, perhaps judged intelligence, the common task, is very much overlearned, and that this is not typical of all perceptual judgments. One test for the generality of the *LDF* would be to use it in an experiment on form-discrimination. If it is as objective there as it was in this study in describing what *O* seems to be doing, its generality would be greatly enhanced.

It should be noted that although the *LDF* was derived from substantive assumptions about perception, it can serve as a formal model for any two-category experiment. A formally similar system known as the Theory of Signal-Detectability (*TSD*) was presented for audition, vision, and later as a general model for psychophysics.[12] There are two basic differences between the *TSD* and the *LDF:* the *TSD* contains normative propositions about ideal *O*s against which human *O*s are to be judged; and the *TSD* does not enable *E* to determine the number, and weights, of the attributes which an *O* uses in making his judgments. That is, *E* can never know the *O*'s decision-axis. Thus, the *LDF* represents an important step forward in perceptual methodology. The *LDF* permits *E* to know what *O* considers important in the stimulus; and, also, just how important each attribute is to the *O*. This is not possible in the context of the *TSD* since it is, in effect, restricted to single dimensions. Thus, in the *TSD* the location of the decision-axis is lost; the only information in the *TSD* is the distance between the two concepts, and the location of the cutoff. The *TSD* does not apply, as usefully, to complex perceptual phenomena as does the *LDF*. On the other hand, for unidimensional psychophysics, the *TSD* and *LDF* are formally identical.

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[12] W. W. Peterson and T. G. Birdsall, The theory of signal detectability, Electronic Defense Group, University of Michigan, *Tech. Rep.* No. 13; W. P. Tanner, Jr., and J. A. Swets, A decision-making theory of visual detection. *Psychol. Rev.*, 61, 1954, 401-409; and, J. A. Swets, W. P. Tanner, Jr., and T. G. Birdsall, Decision processes in perception, *Psychol. Rev.*, 68, 1961, 301-340.

## SUMMARY

Fisher's Linear Discriminate Function (*LDF*) was derived from three assumptions about the nature of perceptual judgments and used as a predictive model in the judgment of multidimensional stimuli. Two experiments involving schematic faces were performed to test the adequacy of the *LDF* as a model. The *LDF*s derived from *O*'s judgments of the 'intelligence' of these faces in one condition accurately predicted judgments made under other conditions at other times. Some implications for perception and psychophysics were discussed.

# ON THE NATURE OF BINOCULAR DISPARITY

By LLOYD KAUFMAN, Sudbury, Massachusetts

Stereopsis is known to be based upon binocular disparity. Fixated points on objects have common visual directions, but non-fixated points at other distances have different subjective visual directions. The latter points are said to be falling on disparate retinal points. When the disparity is uncrossed for a non-fixated point, then that point is localized beyond the fixation-point. Crossed disparity results in seeing the non-fixated point as closer than the fixation-point.[1] The naïve usually is unaware that disparity exists. Under special circumstances, *e.g.* when fixation is held constant, he sometimes can be made to see his double images, but, even so, disparities which are greater than the limits of visual resolution can go unnoticed. In this case we say that the half-images of the object-points have fallen within Panum's area and have fused. The fusion has occurred in spite of the fact that the half-images occupy discriminably different visual directions when they are viewed alternately. Fusion is not, however, necessary to stereopsis. A depth-effect can occur even when disparities are greater than the dimensions of Panum's area,[2] hence we say that fusion accounts for the singleness of binocular vision and that it is an accidental concomitant of stereoscopic depth-perception.

The foregoing paragraph summarizes roughly what is known about two of the most important features of stereopsis; namely, disparity and fusion. The ideas are essentially the same as those discussed by Helmholtz and Hering.[3] They have been studied intensively and have been found to be sound and applicable even today.[4] This paper does not represent an effort to discredit the prevailing concepts, but rather seeks to expand our understanding of what is meant by them.

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* Received for prior publication April 17, 1964. This work was done at the Sperry Rand Research Center. The author is indebted to John Duda and John Miller for their assistance in preparing and viewing the demonstrations reported in this paper.

[1] K. N. Ogle, *Researches in Binocular Vision,* 1950, 10-16, 40-41.

[2] See Ogle, *op. cit.,* 40 ff. for discussion of fusion. See also Ogle, Precision and validity of stereoscopic depth from double images, *J. opt. Soc. Am.,* 43, 1953, 906.

[3] Hermann von Helmholtz, *Handbook of Physiological Optics,* Vol. III, J.P.C.S. Southall (ed.), 1963, 297 ff.; Ewald Hering, *Spatial Sense and Movements of the Eye,* (Carl A. Radde, trans.), Am. Acad. Optometry, Baltimore, 1942.

[4] Ogle, Part II: The optical space sense, in Hugh Davson (ed.) *The Eye,* 4, 1962, 211.

## THE PROBLEM OF BINOCULAR DISPARITY

Koffka raised a tantalizing question about disparity.[5] He pointed out that when *P* and *P′* (Fig. 1) are superposed, *F* and *F′* may fuse and their binocular representation will be seen in depth. *F* and *F′* are in different visual directions and are therefore said to be disparate; but, Koffka asked, why should the disparity between *F* and *F′* be the determining factor? *F* also is disparate with respect to other points on the surface on which *F′* is drawn. Why is it that *F* does not fuse or otherwise interact with the point represented by a dot near *P′* to produce a depth-effect? Somehow, points not on *F′* do not interact with *F*. A selective process must be operating to cause the representations of *F* and *F′* so to interact that stereopsis results. Koffka suggested that this selective process is mediated by the simularity between *F* and *F′*. He could not define similarity in any precise manner, but proposed that it might be based upon the form-qualities of the stimuli. Hence, white contours on a black background may be superposed on black contours on a white background and stereopsis will result. It is disparity between similar shapes or contours that determines stereopsis.

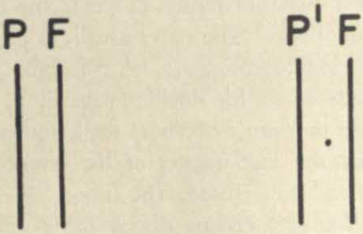

FIG. 1. STEREOGRAM TO ILLUSTRATE DISPARITY

Few authors have considered the role of the stimulus-character in rendering disparity a cue to depth. Woodworth, however, was aware of the problem.[6] He insisted that horizontal disparity is not essentially one of lines and points; he proposed that the disparity which counts is one of width. When the horizontal width of two stripes is different for the two eyes, and when it can be 'overcome' in binocular vision, a depth-effect will result. Woodworth's thesis suggests that, for him, disparity must be between forms for it to be an effective depth-cue. One would presume that the forms must be sufficiently congruent (similar?) that they could combine binocularly, rather than rival, for the depth-effect to appear.

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[5] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 267-269.

[6] R. S. Woodworth, *Experimental Psychology*, 1938, 661-662.

The problem of why disparity between some points in the visual space produces depth, while that between other points does not, has been neglected. Boring recognized the importance of this problem but, in spite of his clear restatement of Koffka's argument,[7] it is a problem that has remained relatively dormant. In all probability, the predictive power of the classical model which includes abstract object-points and their relations in binocular space made further discussion seem gratuitous. It is only recently that Wallach and Lindauer revived the issue.[8] They proposed that the figural properties of the monocular patterns—rather than the differences in the location on the retinas of the individual points that make up the patterns—determine stereopsis. They have reported a number of experiments using line-patterns which yield results consistent with this point of view. The effectiveness of retinal disparity in their experiments depends upon the conspicuousness of the differences in the configurations of the half-images. The following demonstrations provide further evidence that the problem is indeed a real one.

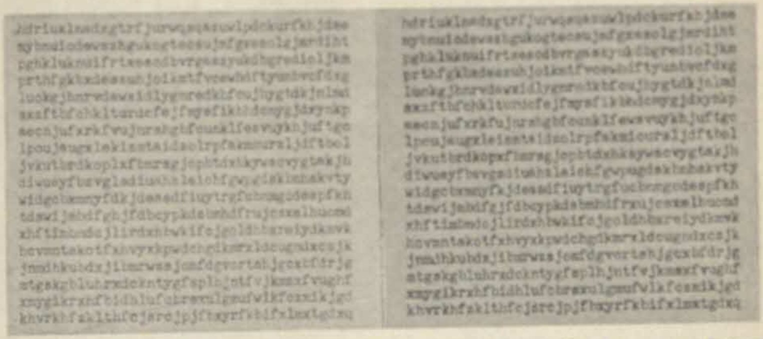

FIG. 2. STEREOGRAM COMPRISED OF IDENTICAL LETTERS EXCEPT THAT INNER PORTION OF LETTERS ON RIGHT SIDE IS DISPLACED ONE LETTER-SPACE TO LEFT, AND DISPLACED COLUMN MOVED TO RIGHT SIDE OF DISPLACED ARRAY.

### DEMONSTRATION I

*Materials.* Fig. 2 shows a letter-pattern analogous to Julesz's pattern.[9] It is comprised of two similar matrices of typed letters of the alphabet. The outer letters all are identical and located in corresponding places. A group of letters, 15 letters wide and 8 rows high in the right-hand pattern, has been shifted one letter space to the left in its matrix. The displaced column of letters was moved to the opposite side of the shifted array, giving a treated region which is 16 letters wide. When the outer regions are superposed, identical letters will occupy corresponding places. The identical inner letters will occupy disparate places. Moreover, each inner letter will be superposed on a dissimilar letter.

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[7] E. G. Boring, *Sensation and Perception in the History of Experimental Psychology*, 1942, 302.

[8] Hans Wallach and Judith Lindauer, On the definition of binocular disparity, *Psychol. Beit.*, 6, 1962, 521.

[9] Bela Julesz, Binocular depth perception and pattern recognition, Colin Cherry

*Results.* A depth-effect is obtained when the two patterns are superposed in the stereoscope. The depth-effect results from the disparity between the identical sets of inner forms relative to the background-letters. This cue is effective even though each letter is in register with a dissimilar letter in the central portion of the contralateral field. Hence it must be argued that the disparity functions as a stimulus to depth in this case because similar elements are involved. If disparity were merely a matter of object-points, then the stimulus-patterns would be ambiguous and depth would not result. This is because points on the superposed dissimilar letters would be as good a cue to the depth of the pattern as points on letters at disparate places. Somehow the nervous system takes the similarity of the disparate forms into account in resolving the pattern into depth. This process also would seem to underlie the depth-perception which occurs in the dot-patterns. Clusters of dots give the disparity-cue.

## DEMONSTRATION II

The foregoing demonstration, as well as Julesz's regular dot-pattern, is merely a more complex version of an ordinary stereogram. As Bridgman points out, the stereoscopic effect is, in itself, not new.[10] It also presents the same problem as that pointed out by Koffka.[11] The pattern does, however, lead to another problem, which is whether or not disparity between similar physical forms is the cue to depth. It is conceivable, for example, that the relevant cue is not disparity between specific identical letters, but disparity between discriminably similar patterns of individual forms.

*Materials.* Consider the following example: Suppose that the letters *wadwadwadwad* are presented to one eye. This pattern has a triadic character in that the three letters *wad* are repeated several times. This pattern can be used to produce a depth-effect when it is included in the inner region of a complex stereogram. The contra-

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(ed.), *Information Theory,* Fourth London Symposium, 1961, 212; B. W. White, Stimulus-conditions affecting a recently discovered stereoscopic effect, this JOURNAL, 75, 1962, 411.

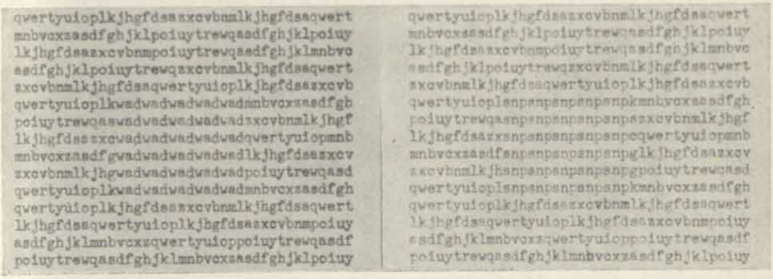
[10] C. S. Bridgman, Analysis of a 'recently discovered stereoscopic effect', this JOURNAL, 77, 1964, 138-143.

[11] Koffka, *op. cit.*, 267-269.

lateral member would have to contain the same triadic pattern except that it would be lagged one letter space relative to the first pattern, *i.e. adwadwadwadw*, to produce a depth-effect; but it is possible also to include a triadic pattern comprised of different letters, *i.e. snpsnpsnpsnp*, in the contralateral pattern. Here the entire array of triads would be shifted one letter-space with respect to background-letters. This is illustrated in Fig. 3, which shows a stereogram containing triads of letters in the inner regions along with the different letter-elements comprising the triads.

*Results.* Viewing this pattern in a stereoscope produces a depth-effect. Rearrangement of these letters, as well as grouping of other letters, also yields a depth-effect.

Two inferences may be drawn from this demonstration. The first is that stereopsis does not depend upon geometrical similarity between the individual letters, although this class of similarity can be used to produce effective disparity. At a more general level, patterning of arbitrary elements can be used to produce depth. This patterning may take the form of individual letters or it may take the form of groupings of letters. It is similar Gestalten which are relevant, not similar geometries of real contours. Secondly, these Gestalten can coexist with their elements. Individual letters within them still have lives of their own in the sense that rivalry is apparent occasionally in the elevated portion of the display. Form-disparity must reside in the patterning of elements and not in the geometry of the individual letters.



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lkjhgfdsazxcvbnmpoiuytrewqasdfghjklmnbvc
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asdfghjklmnbvcxzqwertyuioppoiuytrewqasdf
poiuytrewqasdfghjklmnbvcxzasdfghjklpoiuy
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FIG. 3. STEREOGRAM COMPRISED OF TRIADS OF DIFFERENT LETTERS IN THE INNER REGIONS.

The array of triads on the right side is one letter-space to the left in the background relative to the position of the array on the left side.

There are, presumably, limits over which form-disparity of this type can be effective. If, for example, the inner portions of both displays contained entirely dissimilar sets of letters, a depth-effect would not appear.[12]

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[12] Lloyd Kaufman, Suppression and fusion in complex stereograms, this JOURNAL, 77, 1964, 193-205.

Moreover, shifting one of these sets toward one side of its display also would fail to produce a depth-effect. It is necessary that the patterns contain a common inner structure, such as a periodicity, for a depth effect to occur. In the absence of a common structure in the two patterns, the individual letters would simply display rivalry.

## DEMONSTRATION III

There is yet another way in which to produce an inner patterning of the displays. This can be accomplished by means of a brightness-difference between letters.

*Materials.* Fig. 4 shows such a stereogram. The patterns are comprised of an outer matrix of similar letters. The inner regions contain a single letter which is replicated over all the inner cells. The single letter is at one of two different brightness-levels. The array of brightness-levels is haphazardly distributed over the inner region. The displays were produced by typing the dark letters with a black ribbon and the light letters with a red ribbon. These then were photographed through a red filter to get the red letters to transmit more light to the film than the dark letters. Exposures were selected empirically to insure discriminably different brightness-levels.

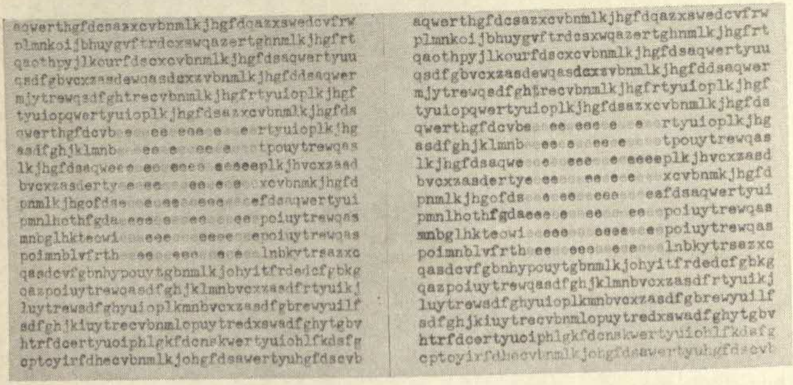

FIG. 4. STEREOGRAM CONTAINING ALL *e*S IN BOTH CENTRAL REGIONS
Left-hand member contains haphazard pattern of brightnesses. Right-hand member contains same pattern of brightness but it is lagged one letter-space to left.

It will be noted that all letters of both inner regions are geometrically the same. These letters cannot exhibit disparity, because similar letters are always superposed regardless of the binocular registration of the patterns. When, however, the background-letters are in register, the binocular array of brightnesses of the inner letters will exhibit a brightness-disparity. This results because the brightness-pattern of one of the displays is replicated in the other display, but shifted one space to the left.

*Results.* Viewing this pattern in a stereoscope produces a depth-effect. Once again we have a patterning of elements which produces a depth-effect in the absence of any disparity between the individual discriminable forms. As a matter of fact, the superposed similar geometrical forms should serve as a cue to the actual depth of the letters; *i.e.* they are coplanar with their background. Somehow this cue of fused similar geometrical forms is ignored, and the disparity of the brightness-pattern prevails in producing a depth-effect. The problem remains: Why does the nervous system seek out the disparity between different patterns of brightness, rather than the lack of disparity between similar forms, to produce the depth-effect?

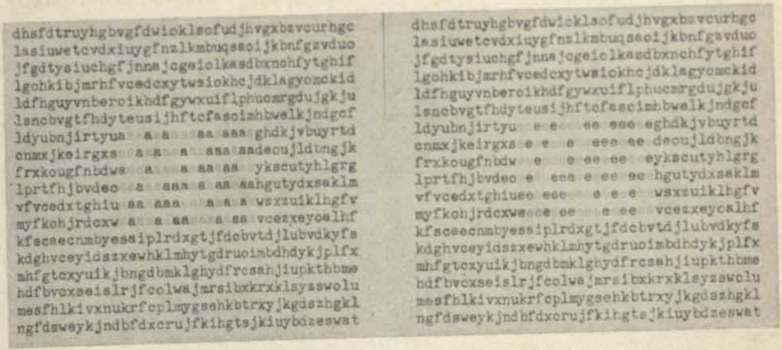

FIG. 5. STEREOGRAM WITH *e*S IN CENTER OF ONE SIDE AND *a*S IN CENTER OF OTHER SIDE.

Brightness is varied in two steps haphazardly among the *a*s. The same pattern of brightness is replicated among the *es* except that it is shifted one letter-space to the left.

## DEMONSTRATION IV

*Materials.* That the inner patterning by brightness can use arbitrary letter-shapes as a carrier for the brightness is illustrated in Fig. 5, which shows one pattern containing all *a*s in the inner region and the other pattern containing all *e*s. The brightnesses of the *a*-pattern vary haphazardly. The brightnesses of the *e*-pattern vary in the same way except that the brightness-pattern is lagged by one letter-space.

*Results.* Viewing this pattern in the stereoscope yields a very curious result. When the *e*-pattern is in the right eye, a depth-effect is achieved, but *O* sees mostly *e*s or *a*s elevated in depth, depending upon his eye-dominance. If *O* is right-eye dominant, he will tend to see all *e*s. If he is left-eye dominant, he will tend to see all *a*s. This selectivity is much more pronounced in the sharp original photographs than it is in relatively blurred half-tone reproductions. The selectivity points up an interesting property of binocular vision. It is evident that summative fusion of disparate stimuli, if it exists at all, is not represented in experience. It is certain, therefore, that apparent fusion of disparate stimuli is really an artifact of suppression. Summative fusion of disparate stimuli is not necessary for stereopsis, since one half-image in this pattern is seen in depth while the other is suppressed. The experience of stereopsis, at least in these patterns, represents the modification of the appearance of one monocular image by a suppressed contralateral image. This finding, is consistent with that of an earlier experiment in which stereograms were viewed through filters of different color. In spite of marked color-rivalry, a stable perception of depth was achieved.[13] It also is consistent with an experiment by Julesz in which the direction of depth in an ambiguous pattern was shown to be determined by prior exposure to an unambiguous pattern.[14] The ambiguous pattern masked the unambiguous stimulus which therefore remained unseen by *O*. The point here is that a suppressed or unseen stimulus-representation still can have perceptual consequences.

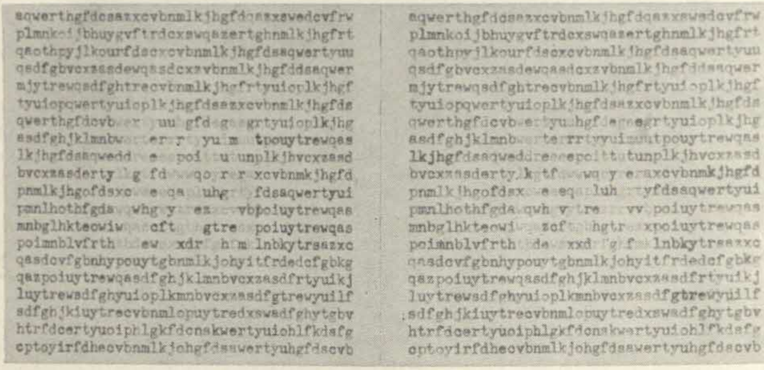

FIG. 6. TWO IDENTICAL LETTER-PATTERNS WITH A BRIGHTNESS-PATTERN IN THE LEFT-HAND MEMBER AND THE SAME BRIGHTNESS-PATTERN LAGGED ONE LETTER-SPACE TO THE LEFT IN THE RIGHT-HAND MEMBER.

This demonstration confirms once again the proposition that patterns within the matrices determine stereopsis, with physical form-congruence as a secondary factor.

## DEMONSTRATION V

This demonstration is similar to Demonstration III except that, instead of a single physical form as the brightness-pattern carrier, a random array of letters, identical in both members of the stereogram, is used as the carrier (Fig. 6). It is reasonable to assume that the random array of letters will tend to mask the brightness-patterning due to the complexity of the array of single forms. This is evident in the patterns themselves. Viewing these patterns in a stereoscope produces a very weak depth-effect. This result suggests that physical forms can interact with the disparate patterning by distorting it.

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[13] Kaufman, *op. cit.*, 196.

[14] Julesz, Binocular depth perception without familiarity cues, *Science*, in press.

## Demonstration VI

*Materials.* In this case, one member of the stereo-pair shown in Fig. 6 is presented in Fig. 7 together with a pattern containing identical outer letters and an inner array of a single letter, *i.e.* all *e*s. The *e*s carry the same brightness pattern as the random inner letters, but the pattern is lagged one cell-space to the left.

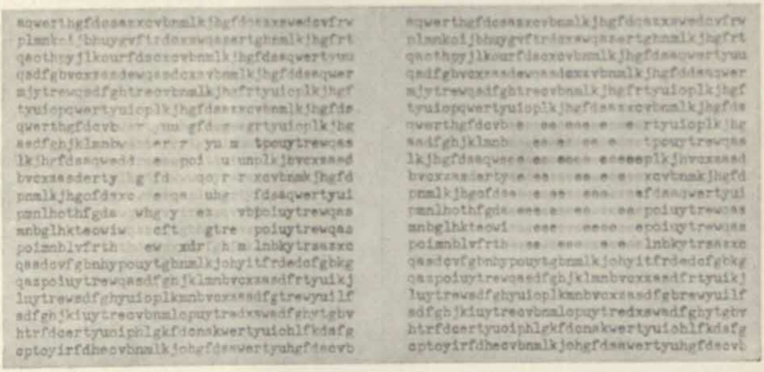

FIG. 7. STEREOGRAM WITH SAME LEFT-HAND MEMBER AS IN FIG. 6 AND ALL *e*S IN SAME BACKGROUND IN RIGHT-HAND MEMBER.
Brightness-pattern in left-hand member is replicated on right but lagged one space to left.

*Results.* This pattern produces a very marked depth-effect when viewed in the stereoscope. This demonstration illustrates the remarkable flexibility of the binocular system. As long as brightness-pattern is clearly discriminable to one eye, even though it is masked by form-complexity or by poor brightness-reproduction in the other eye, a depth-effect will occur.

## Conclusion

It must be concluded that any discriminable pattern can produce an effective disparity. Contours or physical forms are involved only insofar as they carry the patterning. Subjective contours, produced by adjacent physical-contour segments, cannot be the crucial stimuli, because contour can be pitted against brightness-patterning, and patterning will prevail in producing a depth-effect. Brightness, and organizations of different but commensurable physical forms, can carry the patterning. The object-point model works because the two eyes normally see the same objects in the visual field. The objects have forms or contours which display pattern-disparity. Stereopsis, in these terms, is the reaction of the visual system to transverse phase-differences in the information contained in the stimulus-patterns in the two eyes. This reaction is the modification of the experience of one monocular image by a suppressed contralateral image. It need not be a fused image.

# THE BISECTION OF A VISUAL EXTENT

By James Russell Nazzaro, Columbia University

The problem of the present study was to determine the variable and absolute error of the bisection of perceptual space made under conditions well above the discriminative thresholds of acuity and brightness. The parameters varied were luminance, visual area, and separation of the fiducial or standard lines of reference demarcating the visual area. The data were analyzed in terms of Hecht's equations for brightness-discrimination.[1]

## Method

*Apparatus.* The apparatus, a monocular viewing device shown in a schematic diagram in Fig. 1, presented *S* with a circular visual field of 8° 7′ in diameter. The distance between the 3-mm. artificial pupil (*P*) and the target (*T*) was 13.8 in. Illumination of the field was provided by a 100-w. clear, incandescent bulb (*B*) operated at approximately 110-v. D.C. and kept constant at 0.8 amp. To diffuse the light, an opal plastic sheet (Ops), upon which the adjustable line was engraved, was inserted between the light-source and the target (*T*). The opal sheet presented *S* with a luminance of 538 mL. A filter-box (*F*) was interposed between the target (*T*) and the artificial pupil (*P*) that the luminance of the source could be discontinuously varied with Wratten neutral-density filters.

The target-plates consisted of optically correct Plexiglas glued to copper frames. The fiducial or standard lines of reference were engraved on the Plexiglas 0.025-in. wide and 0.002-in. deep. The lines were filled in with elastic glazing compound blackened with India ink. The separation between the target (*T*) and the opal plate (Ops) was approximately 0.006 in., a distance that nearly obviates monocular-movement parallax. The angular separations of the vertical, parallel fiducial lines subtended the following visual angles at the eye: 6.0′, 9.0′, 13.2′, 17.1′, 21.0′, and 25.0′. The target-plates (*T*) were fitted into a slotted holder (*H*) and had to be recalibrated by a micrometer every time they were inserted to determine the exact center between the fiducial lines. They were calibrated by placing a lens (*L*), which magnified the line 1.6 times, in the viewing tube and then placing the bisecting line against the inside of the left fiducial line so that no light could be seen. Then the bisecting line was brought against the right fiducial line so that no light could be seen. The procedure was repeated three times and the center was found by adding one-half of the average interval to the lower micrometer reading.

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* Received for publication August 7, 1962. Supported by a contract between Columbia University and the Office of Naval Research. The author wishes to thank Professor Clarence H. Graham and Drs. William J. McGill and Yun Hsia for their assistance in this study.

[1] Selig Hecht, A theory of visual intensity discrimination, *J. gen. Physiol.*, 18, 1935, 767-789.

*S* adjusted the position of the bisecting line by turning micrometer (*M*) with a large wheel (*W*), part of which fitted into the dark room through a slot in the wall. In this manner, *S* could directly and easily displace the position of the micrometer-shaft to 0.0001 in. with minimal proprioceptive cues. Rotating the wheel edge 47-mm. displaced the bisecting line 0.001 in.

The discontinuous luminance-steps used were —1.75, —0.52, 0.16, 1.08, 1.56 and 2.73 log mL. These luminances were used to keep the contrast well above the threshold of brightness-discrimination for visual fields over 1°. The width of the lines (subtending 6′ of arc at the eye) was so chosen as to keep the objects well above the threshold of visual acuity and, consequently, the lowest separation between lines (6′) was the limiting case.

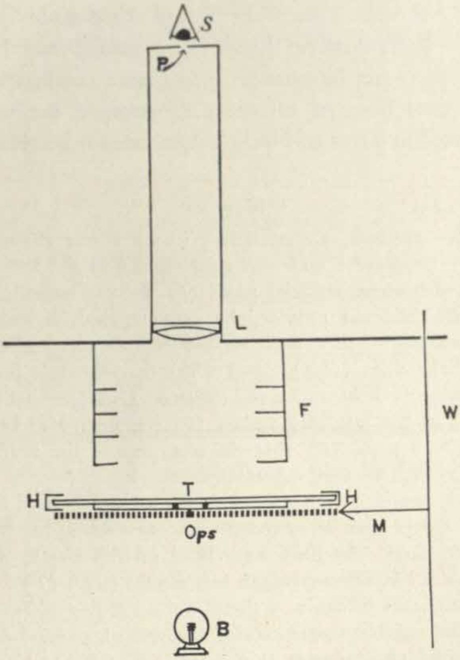

FIG. 1. SCHEMATIC DIAGRAM OF THE APPARATUS

The area of the visual field was reduced by means of different apertures inserted in the filter-box (*F*). This arrangement also reduced the lengths of the lines and provided the following angular sizes of visual field-diameter: 6° 29′, 4° 8′, 2° 22′, and 1° 12′. *S*'s head was held in place by means of a chin-rest.

*Subjects*. Four *Ss* with normal vision, ranging in age from 23 to 46 yr., were used in this study; additional readings were taken from the 23-yr. old *S*. Prior to the experiment, each *S* was asked to make 200 practice settings in two days to familiarize himself with the apparatus and to clarify the task.

*Procedure*. *S*, using an eye patch, was dark-adapted for 20 min., and then light-adapted for 5 min. to the prevailing luminance. The first five bisections made under each condition were discarded to insure complete light-adaptation and to stabilize the criteria of bisection. After each setting, the bisecting line was displaced to the right or left in random order.

A completed condition consisted of 25 judgments at one separation of the fiducial lines, and one luminance-level. Two conditions were completed in one sitting, with a 15-min. rest between conditions and a 5-min. readaptation to light prior to undertaking the second condition. Four conditions in all were conducted in one day with an hour rest-period between sittings. Monocular vision was employed, each *S* making judgments with his dominant eye.

To minimize practice effects, the 36 experimental conditions were randomized for each *S*, using Snedecor's method for random permutations.[3] The standard deviation of the 20 settings for each condition was used as an indicator of the variable error. The mean of the deviations was used as an indicator of the average absolute error.

TABLE I

MEAN VARIABILITY (STANDARD DEVIATIONS IN SECONDS OF ARC) FOR 4 *S*S UNDER DIFFERING FIELD LUMINANCES IN LOG MILLILAMBERTS AND FIDUCIAL SEPARATIONS IN MINUTES OF ARC

| Luminance | Separations | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 6′ | 9′ | 14′ | 17′ | 21′ | 25′ | 46′* | 60′* |
| 2.73 | 8.11 | 3.64 | 5.77 | 7.49 | 8.75 | 9.72 | 11.66 | 18.38 |
| 1.56 | 7.62 | 4.91 | 6.05 | 7.37 | 7.59 | 7.79 | 13.15 | 19.88 |
| 1.08 | 6.91 | 4.88 | 6.06 | 8.00 | 9.50 | 10.82 | 13.90 | 22.27 |
| 0.16 | 7.19 | 4.82 | 6.50 | 8.07 | 10.12 | 12.39 | 15.28 | 27.95 |
| −0.52 | 10.42 | 5.52 | 7.47 | 8.57 | 8.93 | 12.33 | 15.99 | 26.75 |
| −1.75 | 18.62 | 11.39 | 12.70 | 11.18 | 12.48 | 17.15 | 20.92 | 33.93 |

* Data from one *S* only

## RESULTS

The results may be divided into two parts: (1) those dealing with variable errors and (2) those with average absolute errors.

*(1) Variable errors*. Table I summarizes the data of four *S*s. Each standard deviation is derived from 20 settings, taken from each *S*. Fig. 2 holds the separation of the fiducial lines constant and the plot shows the change in variability as a function of changes in luminance. The curves at 46′ and 1° are curves for one *S* only. It can be seen that increasing the luminance of the field decreases the variable error of the bisections. Decreasing the separation of the fiducial lines has a similar effect. It will be noted that the slopes of the two narrowest separations are slightly steeper than the other separations.

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[3] W. C. Cochran and G. M. Cox, *Experimental Designs*, 1950, 569-571.

The curves fitted to the data-points are portions of Hecht's curve for brightness-discrimination:[3]

$$\Delta I/I = c/a^2k_2[1 + 1/(KI)^{\frac{1}{2}}]^2 \qquad [1]$$

where $I$ is the prevailing luminance; $\Delta I$, the increment in luminance; and $K$ and $c/a^2k_2$ are constants. No special theoretical significance is attached to the curves except for the fact that they may be taken to represent the similarity of these findings to data on brightness-discrimination.

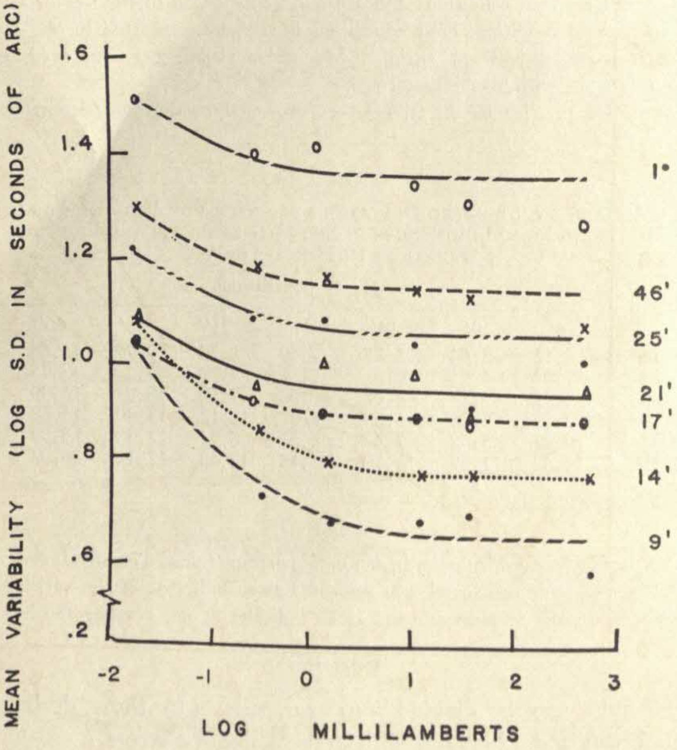

FIG. 2. LOG MEAN VARIABILITY OF THE BISECTION SETTINGS FOR 4 *S*s AS A FUNCTION OF LUMINANCE IN LOG MILLILAMBERTS

(The 46′ and 60′ curves are for one *S* only. The values on each curve indicate the separations in minutes of arc between fiducial lines.)

Fig. 3 is another plot of the same data, showing the effect of holding luminance of the field constant and varying the separation of the fiducial lines. The increase in variability is slightly curvilinear in trend from 9′ to

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[3] Hecht, *op. cit.*, 767-789.

60′ and would most likely be fitted by a power polynomial. The variability at 6′ is greater than it is at 9′. The former constitutes a limiting case since the width of the bisecting line was just equal to the separation of the fiducial lines. Generally, the least variability is seen with small separations and with high field luminances.

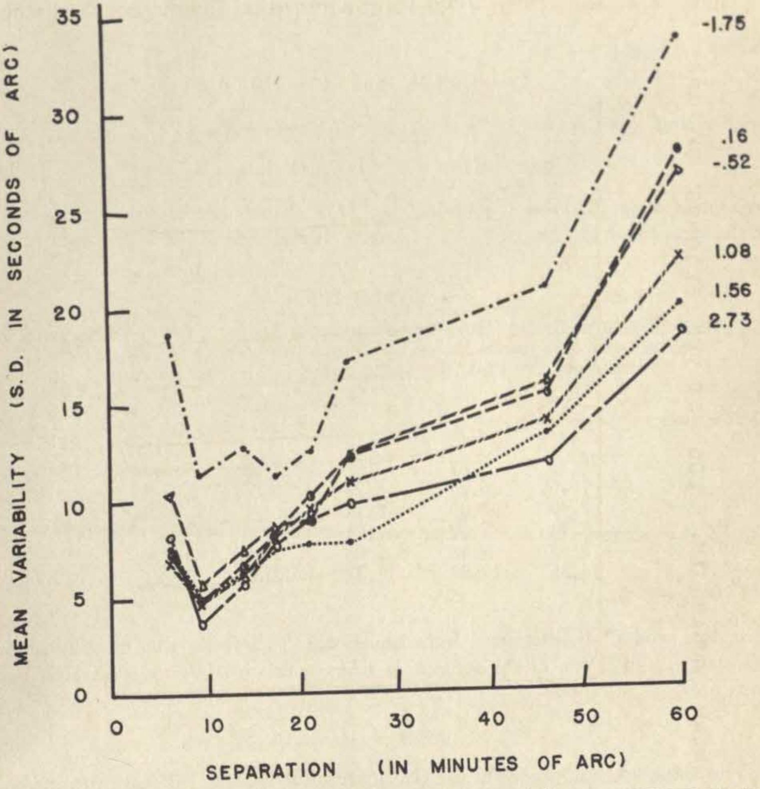

FIG. 3. MEAN VARIABILITY OF THE BISECTIONS FOR 4 *Ss*, AS A FUNCTION OF THE SEPARATION OF THE FIDUCIAL LINES
(The 46′ and 60′ points are for one *S* only.)

To provide an additional test of the fit of the points to Hecht's curve, a transform devised by Smith and by Mueller was performed.[4] Beginning with Hecht's original Equation [1] for the fovea, one may take the square root of both sides and obtain

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[4] J. R. Smith, Spatial and binocular effects in human intensity discrimination, *J. gen. Psychol.*, 14, 1936, 318-345; C. G. Mueller, Numerical transformations in the analysis of experimental data, *Psychol. Bull.*, 46, 1949, 198-223.

$$(\Delta I/I)^{\frac{1}{2}} = (c/a^2k_2)^{\frac{1}{2}} + (c/a^2k_2)^{\frac{1}{2}}[1/(k)^{\frac{1}{2}}\cdot 1/(I)^{\frac{1}{2}}] \qquad [2]$$

which can be simplified to

$$(\Delta I/I)^{\frac{1}{2}} = C + C'/(I)^{\frac{1}{2}} \qquad [3]$$

where

$$C = (c/a^2k_2)^{\frac{1}{2}}, \quad \text{and} \quad C' = C/(K)^{\frac{1}{2}} \qquad [4]$$

multiplying both sides by $(I)^{1/2}$ and taking logarithms we obtain

$$\log_{10}((\Delta I)^{\frac{1}{2}} - C') = \log_{10}(I)^{\frac{1}{2}} + \log_{10} C \qquad [5]$$

A plot of $\log_{10}((\Delta I)^{\frac{1}{2}} - C')$ against $\log (I)^{\frac{1}{2}}$ should give a straight line with a slope of 1.0 if the data fit Hecht's curve. $C$ is evaluated as the square root of the asymptote, and $C'$ is determined from Equation [1] above. In this transformation, the standard deviation of the settings is taken equal to $\Delta I/I$, *i.e.* $\sigma = \Delta I/I$, and therefore

TABLE II

MEAN ABSOLUTE ERROR (DEVIATIONS FROM THE ACTUAL CENTER IN SECONDS OF ARC) FOR 4 *S*S UNDER DIFFERING FIELD LUMINANCES IN LOG MILLILAMBERTS AND FIDUCIAL SEPARATIONS IN MINUTES OF ARC

| Luminance | Separations | | | | | |
|---|---|---|---|---|---|---|
| | 6′ | 9′ | 14′ | 17′ | 21′ | 25′ |
| 2.73 | 8.35 | 3.87 | 4.95 | 7.09 | 8.56 | 8.91 |
| 1.56 | 7.16 | 5.44 | 5.23 | 6.56 | 9.44 | 12.01 |
| 1.08 | 7.40 | 8.09 | 8.11 | 6.51 | 9.96 | 14.76 |
| 0.16 | 8.14 | 7.40 | 7.71 | 7.38 | 9.51 | 12.97 |
| −0.52 | 9.85 | 5.48 | 7.68 | 7.19 | 15.54 | 19.20 |
| −1.75 | 14.35 | 11.42 | 11.42 | 13.31 | 15.16 | 20.72 |

$$\sigma \cdot I = \Delta I \qquad [6]$$

The data, except probably for the lowest luminance, fall on an approximately straight line with a slope of 1.0, as shown in Fig. 4. They therefore may be taken to justify the application of Hecht's equation as a possible fit for the data. As the separation of the fiducial lines is increased, the intercept ($C'$) also increases.

*(2) Average errors.* Table II summarizes the errors for 4 *S*s as calculated by subtracting the settings made by the *S*s from the true center, without regard to sign. As the luminance increases, the amount of error decreases; and, as the separation increases, the error increases. The results approximate those of the variability parameter in slope and acceleration, the smaller separations showing the greatest amount of change.

Fig. 5 shows the results of a visual field study carried out on one *S*. In this case, the only change made was reducing the size of the illuminated field (also thereby reducing the length of the lines) for three different luminances. Within each luminance, three separations were used. Reducing the field from 8° 7′ to 1° 12′ seemed to have no effect on the variable error of the bisections. As the field-luminance is reduced however, the amount of variability is increased, and the range of this variability is also increased. Increasing the separation also produces increases in variability.

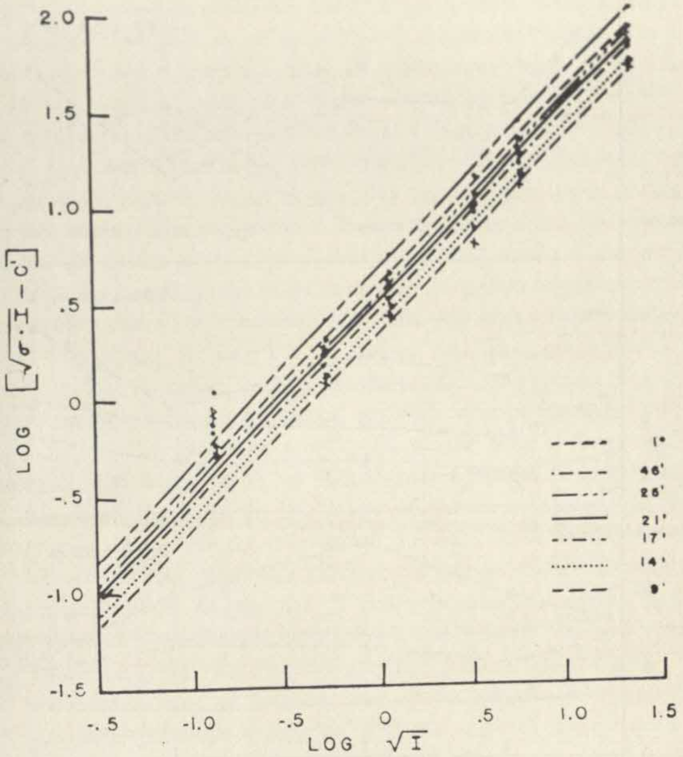

FIG. 4. TRANSFORMATION OF THE DATA OF FIG. 2 BY MEANS OF SMITH'S METHOD

An analysis of variance of the data shows that for both variable and average error, variance due to separation, variance due to luminance, and variance between *S*s are highly significant ($p < 0.01$ in each case). There were no significant first-order interactions found between conditions and, interaction between *S*s and conditions was negligible. Therefore, averaging of the *S*s' raw data was taken to be permissible.

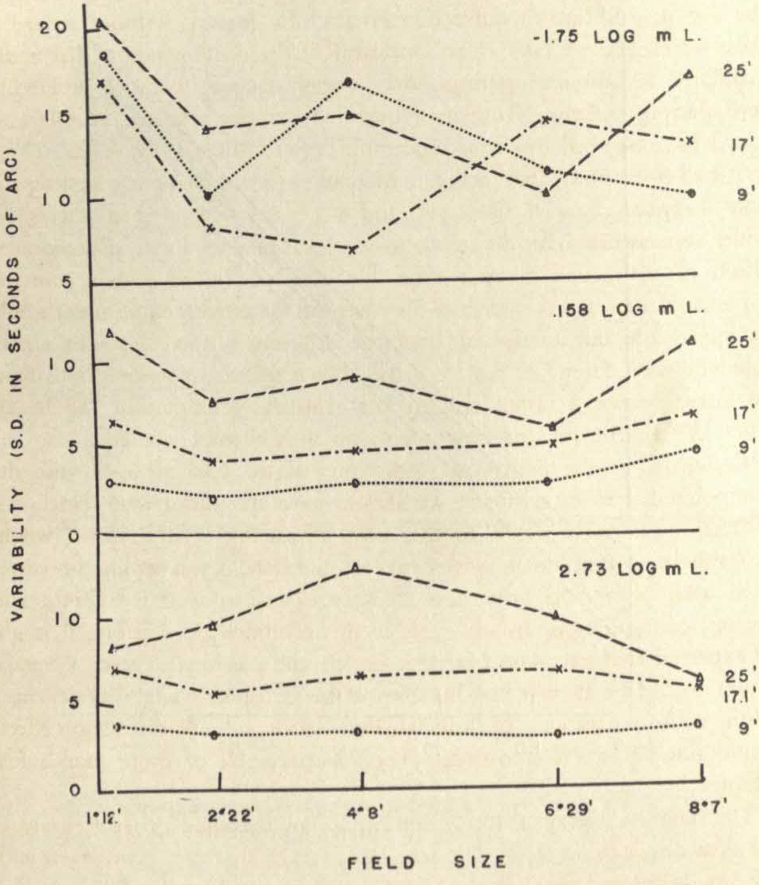

Fig. 5. Results of Changing the Size of the Visual Field Upon Variability of Bisections

(Represented are data for three different luminances and three separations of fiducial lines indicated by the figures beside the curves.)

## Discussion

Hecht and Mintz have postulated that the mechanism in the resolution of fine lines is the ability of the eye to distinguish between different retinal illuminances in different parts of the diffraction pattern of the retinal image.[5] On this basis, they equated the reciprocal of visual angle for line resolution, $1/\alpha$, to $f(I)$, *i.e.* $1/\alpha = f(I)$, where the latter term is the same as the right-hand term of Equation [1]. It appears certain, too, that visual-acuity situations also involve scattering of light by the dioptric medium of the eye in addition to diffraction effects.[6] In general, without regard to these variables, we take as an empirical fit the demonstration that $\sigma$, the standard deviation of settings, may be taken equal to $\Delta I/I$ in Hecht's formulation, thereby giving us a useful descriptive relation. Mueller and Lloyd have applied this equation empirically to stereo-acuity data.[7]

It had been noted that in Fig. 2 the curves for the two narrowest separations between fiducial lines (6′ and 9′) were steeper than curves for wider separations. This difference in steepness may be due to differences in effects of diffraction if we assume that other conditions remain constant. To estimate the actual spread of the image in the present experiment would be impossible due to the fact that the diffusing surface provides a non-coherent wave-front. At best, Fry has shown that many assumptions must be made before a rather lengthy mathematical computation can be attempted.[8] In the present experiment, no such attempt is made.

Certainly, at the narrowest separations of the fiducial lines with the interpolated dark target-figure we should have optimum circumstances for diffraction-effects to occur. In the range of narrow separations, *S* would probably find that small movements of the hand knob would be correlated with observable brightness changes as well as spatial differences in the test-configuration. In any case, if this condition does apply, it might be expected that precision (variable error) and possibly accuracy (average error) would be at their best because of the increased availability of 'cues.' With wide separations of fiducial lines, such aids as diffraction-effects would not be expected to occur over a visual angle of more than a few minutes.

The data on average error of setting follow the same general curves as the data on variable error. The smallest average absolute error given with the smallest separation for 4 *S*s was 3.87″ of arc. One *S* (*RK*) showed (under the same separation) the remarkably low absolute error of 1.27″ of arc.

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[5] Selig Hecht and E. U. Mintz, The visibility of single fine lines at various illuminations and the retinal basis of visual resolution, *J. gen. Physiol.*, 22, 1939, 593-612.

[6] G. A. Fry and P. W. Cobb, A new method for determining the blurredness of the visual image, *Trans. Acad. Ophthal. and Otolaryngology*, 1935.

[7] C. G. Mueller and V. V. Lloyd, Stereoscopic acuity for various levels of illumination, *Proc. Natl. Acad. Sci.*, 34, 1948, 223-227.

[8] Fry, *Blur of the Retinal Image*, 1955, 159-163.

Area is an important variable in practically all types of visual performance (for example, see Steinhardt, Graham and Bartlett, and Blackwell in the specific case of brightness discrimination).[9] In the present study, reducing the size of the visual field had no apparent effect on the variable error or average error of bisecting. The *S*s were able to bisect intervals as well with an 8°-field as with a 1°-field.

The problem may be thought of in terms of border-effects. Fry and Bartley maintain that borders exhibit two characteristics: (1) they interfere mutually if they are sufficiently close to each other; and (2) they prevent spread of activity.[10] It is the second property which may conceivably be operative in our situation. If the opaque fiducial lines prevent activity from spreading, it would matter little for bisection what the overall area is. The results of this study are therefore not inconsistent with interpretations in terms of border-effects.

A final and more general topic for discussion is also of interest. To date, luminance has not been seriously considered a factor in studies of spatial bisection, prevailing opinion implying that if a spatial interval can be seen, it can be bisected. That this is not the case and that, in fact, this sort of 'judgmental' performance does depend upon luminance has been demonstrated by the present experiments. Dependence on luminance is demonstrated not only for conditions (narrow fiducial-line separations) where one can infer an important effect of diffraction (and hence discrimination of brightness) but also for conditions (*i.e.* wide separations of fiducial lines) where diffraction presumably plays a negligible role. In a sense then, it is shown here that such a primarily 'sensory' variable as luminance does have an important influence in what is usually considered to be a primarily 'judgmental' process.

## SUMMARY

This study is concerned with the variable error and the absolute error of an *S*'s performance in bisecting a visual space bounded by two vertical fiducial lines. The influences of the following variables were examined: the luminance of the visual field, the separation of the fiducial lines, and the area of the visual field.

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[9] Jacinto Steinhardt, Intensity discrimination in the human eye, I. The relation of $\Delta I/I$ to intensity, *J. gen. Physiol.*, 20, 1936, 185-209; C. H. Graham and H. R. Bartlett, The relation of stimulus and intensity in the human eye: III. The influence of area on foveal intensity discrimination, *J. exp. Psychol.*, 27, 1940, 149-159; H. R. Blackwell, Contrast thresholds of the human eye, *J. opt. Soc. Amer.*, 36, 1946, 624-643.

[10] G. A. Fry and H. S. Bartley, The effect of one border in the visual field upon the threshold of another, *Amer. J. Physiol.*, 112, 1935, 414-421.

The results show that:

*(1)* Increasing the luminance of the field decreases the variability of the settings and also decreases the average absolute error of the settings.

*(2)* Increasing the separation of the fiducial lines increases the variability of the settings and also increases the average absolute error of the bisections.

*(3)* Effects due to luminance-changes were analyzed in terms of brightness-discrimination considerations and Hecht's curve of brightness-discrimination was taken to provide a convenient empirical description, useful for indicating formal similarities among various types of data.

*(4)* Variability of settings was found to be a slightly curvilinear function of fiducial line separation.

*(5)* Changing the area of the visual field seemed to have no effect on the variable error of bisections. Interpretation of the lack of area-effect is given in terms of the concept of the 'restraining' properties of borders.

# NEW AUDITORY PHENOMENA FROM SIREN EXPERIMENTS

By MAX F. MEYER, Springfield, Virginia

In experiments with the sirens described below, either a *faint* third or a *faint* fifth harmonic to the fundamental, or both, are heard; but no regular series of harmonics. An hypothesis assigning the origin to the middle ear would agree with the absence of *even*-numbered harmonics. Seebeck's definition of a physical tone is shown to be preferable to that of Ohm.

*Experiment* I. Around the circumference of a Seebeck siren disk, 21 points are irregularly distributed as follows (see Fig. 1). The circle is first divided into seven equal arcs. Each primary arc is then subdivided into seven auxiliary arcs. At the first three of these points holes are drilled, but the next four points are left intact. Then again three holes are drilled and four points are left intact; and so around the disk.

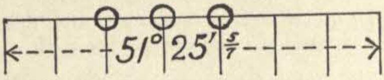

FIG. 1. DISTRIBUTION OF 27 HOLES AROUND A CIRCLE

The disk is rotated (with most success) at the speed of 9.3 rotations per sec. The circle is blown with lung power, through a 3/16-in. inner diameter nozzle. The following pitches are heard by several musically competent observers: $C_2$, plainly; $B\flat_4$ weakly; and $G_3$ faintly. The first two pitches mentioned are not new; the third is. I quote: "It is undeniable that the brain can [and often does] accept groups of charges as if they were unit charges."[1] The group of three holes in our case is repeated seven times. The product of 7 and the rotary speed 9.3 is 65.1, that is, $C_2$. In another of its ganglions the brain counts the three charges as occurring at the *rate* of 7 per sub-arc. The product of 49 and the rotary speed 9.3 is 455.7, that is, $B\flat_4$. The third pitch, the *new phenomenon,* is the faintly heard pitch $G_3$. Its hypothetical explanation will be offered below, in a later section. The inability of my listeners to discriminate *any other* pitch of considerable *loudness*

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[*] Received for publication October 15, 1962. The author's address is 5308 Danville St., Springfield, Virginia.

[1] M. F. Meyer, Neurological theory of beat-tones, *J. acoust. Soc. Amer.,* 28, 1956, 878 a; W. R. Thurlow and A. M. Small, Jr., are there quoted in support of this point.

but $C_2$ and $B\flat_4$ supports Seebeck's contention against Ohm.[2] (The hydrokinetic description of a siren puff is given in the appendix of this article.)

*Experiment* II. On a circle of a siren disk, 36 perforations are irregularly distributed in the following manner (see Fig. 2). Two holes are drilled with a distance (center to center) of 6°. Two such holes will be hereafter referred to as *one duplex.* The next distance equals 17°. The following distance, until we reach a further duplex, is again 17°. The sum of 40°, *i.e.* from the center of a duplex to the center of the next duplex, is repeated 9 times around the circle.

Starting with the high rotary speed of 26 c.p.s. we hear a strong $A\sharp_4$ (18 times the rotary speed) and a weak $A\sharp_3$ (9 times the rotary speed).

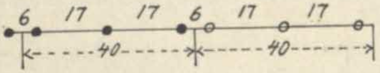

FIG. 2. SHOWING ALTERNATELY THE SINGLE HOLE AND THE DUPLEX
These are spaced over 40° and thus repeated 9 times around the circle.

Neurologically this means that the brain in one of its ganglions takes the duplex as a unitary charge and counts "18" but that in another of its ganglions the brain counts only the rather prominent duplexes, neglecting the intermediate single air-puffs, and counts "9" (multiplied with rotary speed).

We reduce the rotary speed to 14⅔, *i.e.* until we hear a strong $C_4 = 261$ plus a weak $C_3 = 131$. To our surprise, now something unexpected is added, *the new phenomenon* as spoken of in the title of this article: We faintly hear $E_5$, which is the fifth harmonic of $C_3$. The same relative intervals continue for a while. For example, at the speed approximating 11 we hear $G_3$ plus $G_2$ plus $B_4$ which is the fifth harmonic of $G_2$.

I introduce here a control that appears necessary. I have on the same disk a circle of 30 holes quite regularly distributed. With the pitch heard at this circle I now compare the pitches heard at the experimental circle. In the past the experimental circle and this control circle gave an interval of the ratio $18:30 = 3:5$, that is, the major sixth.

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[2] Poggendorff's *Ann. phys. Chem.,* 53, 1841, 417; 59, 1843, 513; 60, 1843, 449; 62, 1844, 1; 63, 1844, 353, 368. Seebeck insists, correctly in the judgment of the present author, on rejecting Ohm's unproved assertion that there *must* exist in mathematical physics *equations* between each pitch heard and a *sinusoidally* formed air wave—and the reverse. Compare another man's judgment in Footnote 6 at the end of this article.

At 7⅓ rotations the control circle lets one hear $A_3$-220. One should expect then to hear on the experimental circle the major sixth below 220, but the interval is definitely a mere minor sixth, *i.e.* we hear approximately 139 instead of 132. This seems to me a neurological monstrosity, admittedly beyond any attempt on my part to explain it. *A priori* we would expect that a lowering of the rotary speed of a siren disk would lower every pitch in proportion, but here it is a small amount less than the drop of rotary speed. (One *might* think that the "17°" distances substituted for "20°" have something to do with it. I leave it to the curiosity of the reader.)

Whatever may be the neurological cause that changed 132 into 139, the odd harmonic is not likewise affected. It is still a true fifth harmonic. Thus it must have a *physical* rather than a neurological origin. The physical side of the problem is taken up in the final section of this article.

I do not report what happens on continued reduction of the rotary speed to 6 cps. and less. The pitches became unclear. Air puffs of low frequency are very weak stimuli. Compare the hole of a siren with the size of a bass viol or a bass tuba and the enormous air masses pushed by them.

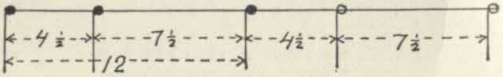

FIG. 3. TWELVE DEGREES ARE REPEATED 30 TIMES AROUND THE CIRCLE

*Experiment* III. A large circle of 15-in. diameter is divided into 30 arcs of 12° each. Then between each two adjacent points a further point is marked, but *not in the middle.* It is advanced 1.5° forward from the middle, so that it has 4.5° before it and 7.5° behind it. Then the 60 holes are drilled, as shown in Fig. 3.

Beginning with a high rotary speed of 14.7 c.p.s. and down to about 8.7 c.p.s. we hear only the pitch "30" (times the rotary speed) plus something suggesting very high overtones. We do not have a word in English for the sound; only the German phrase "Trompeten *schmettern,*" describes it. The fact of there being 60 holes around the circle is ineffective, apparently because the brain rather counts 30 duplexes than also single charges in this case, and 4.5° constitute the duplex. This is nothing new. Further reducing the speed to 4.36, we hear the expected 131 = $C_3$ *but now in addition* $G_4$, though weakly. This third harmonic is *the new phenomenon; unpredictable* by examining the siren disk. While we gradually further reduce the rotary speed, the third harmonic gains steadily in loudness. When the tone "30" reaches $C_2$ we *not only* hear its *third* harmonic $G_3$ but in addition, though faintly, its *fifth* harmonic, that is, $E_4$. Nevertheless, there is the complete *absence* of any even-numbered harmonics to our ear.

*Experiment* IV. August Seebeck in 1844 writes that in his siren experiments he occasionally heard *Beitöne*. The prefix *bei* signifies a small added quantity that is considered as being of little importance. Regrettably, Seebeck made no effort to report *the musical interval* of such a *Beiton*.

I guessed at two causes for his Beiton: The use of a wrong-sized nozzle and an excessive mutual separation of the holes. I therefore computed (1) how the size of the nozzle must be related to the size of the hole in order that *within* the air puff dy/dx was never a constant; and (2) how close together the holes must be in order that *between* two air puffs there was never the vacancy of a constant dy/dx inserted. In other words, I proposed that the curve telling the air quantity passing through the disk should never run horizontal.

Accordingly I built a disk as follows. On a circle of 6.4-in. diameter there are 46 holes of ¼-in. diameter and the nozzle used is not smaller but has the same size of ¼-in. diameter. Easy computation shows that one forty-sixth fraction of the total length of a 6.4-in. diameter circle is equal to 0.437 in. On the other hand, to fully open and exactly close itself a ¼-in. nozzle before a ¼-in. hole must deprive the circle of a half inch of the circumference. But here it is not given 0.500 but only 0.437 in. It follows that every air puff begins 0.063 in. before the preceding air puff is ended. In other words, by that amount the air puffs overlap. Fig. 4 shows this overlapping through the fact that the *valley* of the curve has a smaller measure of curvature than the curvature at the *summit*. (That thus logically it is *no sinusoid*, does not block our reasoning, or Seebeck'e either.)[3] In compliance with the Conditions (1) and (2) of the previous paragraph, no part of the curve runs *horizontal*. (In developing my explanatory hypothesis in the final section of this article I refer in the last sentences by saying: "There is in this case *no knocking* of the tympanum.")

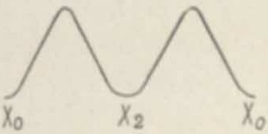

FIG. 4. THESE OVERLAPPING AIR QUANTITIES DO NOT CONSTITUTE A SINUSOIDAL WAVE

The auditory tests shows that there is neither an odd-numbered nor an even-numbered harmonic audible. A single pitch; no *Beiton;* but now come the violations of those conditions.

*Experiment* V. The same disk as in Experiment IV is used, but a 3/16-in. nozzle is substituted. This violates only Condition (1) in Experiment IV. There is constancy of dy/dx between $X_1$ and $X_2$ in Fig. 5, although the third harmonic of 46 holes is heard *very faintly*. This is *the new phenomenon.* No other pitch but this and its loud fundamental is audible. As Seebeck pointed out, his sirens produce no 'overtones' in the air.

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[3] Cf. Footnote 2.

*Experiment* VI. On a circle of 8-in. diameter, 48 holes of ¼-in. diameter are drilled, center to center 0.5236 in. apart. A nozzle of 3/16-in. diameter, gives each air puff a duration of 0.4375 in. on the circle. The vacant distance from the end of an air puff to the beginning of the next is then 0.0861 in. The quotient of the vacant distance by the length of the air puff is 0.0861/0.4375 = 0.1968. Fig. 6 approximately illustrates it. Both between $X_1$ and $X_2$, and also between $X_3$ and $X_4$ there are time spans during which the air quantity passing neither rises nor falls, *i.e.* dy/dx is a constant. *Both* the conditions marked (1) and (2) in the description of the fourth experiment are *violated*. The period, also called 'wave-length,' is of course the distance from $X_0$ to $X_4$ in Fig. 6.

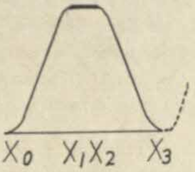

FIG. 5. BETWEEN $X_1$ AND $X_2$ THE DERIVATIVE DY/DX IS A CONSTANT

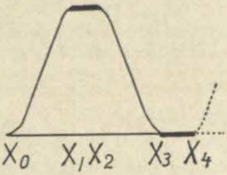

FIG. 6. BETWEEN $X_1$ AND $X_2$ AND ALSO BETWEEN $X_3$ AND $X_4$ THE DERIVATIVE DY/DX STAYS CONSTANT

I now make the auditory test. The fundamental pitch 48 (times the rotary speed) is accompanied for the listener by the third and fifth harmonics, Seebeck's *Beitöne,* presumably. (Of probably minor importance: during the lower half of the range when the fundamental rises from 90 to 350 cps. the fifth harmonic is louder than the third one, and during the upper half of the range the third harmonic is louder than the fifth one.) This is *the new phenomenon* mentioned above. No pitches but those specifically mentioned are audible; which contradicts Ohm.

*Experiment* VII. The same disk is used as in Experiment VI but every alternate hole is stopped flush with wax. The vacant distance from the end of one air puff to the beginning of the next is then 0.6097 in. The quotient of the vacant distance by the length of an air puff is now 0.6097/0.4375 equal to 1.393. Thus each air puff is followed by a *vacant* distance that is about three times as long as the air puff itself (0.4375). The acoustical period (wave-length) of course is equal to the distance from $X_0$ to $X_4$ in Fig. 7.

Blowing through a nozzle of 3/16-in. diameter, I make the auditory test. I slowly increase the rotary speed of the disk and hear the fundamental "24" appearing and rising in pitch, and being *accompanied* by the third and the weaker fifth harmonic. This is *the new phenomenon* mentioned above. When, however, the fundamental approaches the pitch 156 cps., the accompaniment begins to fade. Later the fundamental is heard *alone*, though it is difficult to fix a definite point for that event.

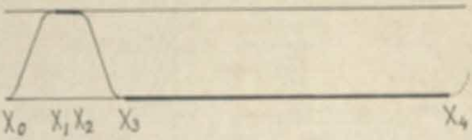

FIG. 7. THE VACANCY BETWEEN PUSHED AIR-COLUMNS IS VERY LONG

This ends the description of the physical stimuli (siren puffs), and their conscious auditory responses. In what follows, I endeavor to explain *the new phenomena* reported in six of the seven experiments presented above.

*Mathematical physics.* When a ballet dancer, holding a vellum tympanum with a hard frame in one hand, knocks it with the other hand, that tympanum can oscillate only with the fundamental for which it was "once-for-good" tuned and with a variety of odd *and even* harmonics. Let us regard the living tympanum as if it were a similar musical instrument, that is, one that may be knocked. It differs by being conical. Thus it can never get to the other side of the plane of its periphery. In that respect, if knocked (I am *not* speaking of its common function as a forced transducer) its case is analogous to an open organ pipe into which a demonstrating physics teacher has pushed a stopper to the middle of the pipe.[4] Only odd-numbered harmonics can come from the pipe.

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[4] Herman von Helmholtz, *Tonempfindungen,* 5th ed., 1896, 152.

Secondly, the living tympanum is not tuned "once-for-good." Being a physiological instrument, it can tune and retune itself. For that it is equipped with two muscles: The musculus stapedius served by the nervus facialis, and the musculus tensor tympani served by the nervus tensoris tympani.

My hypothesis is that the tympanum, though impassive to smoothly rising and falling waves sent through it by *common* musical instruments, "feels" as if it were being *knocked* when it is affected by such 'irregularities' as are not uncommon in *siren* experiments. It then tunes and retunes itself to the frequency of the *momentary* irregularity, and through mechanical spontaneity issues *odd* harmonics, a third or a fifth or both, and *sends* them into the cochlea, beginning after 1/10 sec. or there-a-bouts.[5]

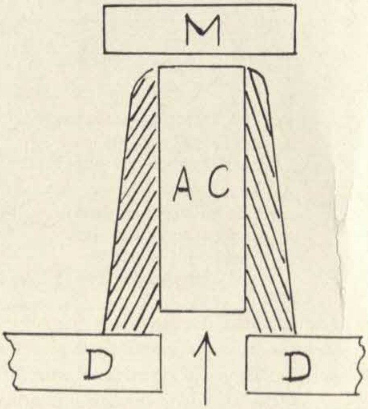

FIG. 8. M MICROPHONE WHICH REGISTERS THE PRESSURE
AC a pushed air column temporarily surrounded by a low pressure air mantle; DD a siren disk through a hole of which AC has been pushed.

In Experiment I, the irregularity would be represented by the fact that three pulses serve in the place of a single pulse. In Experiments II and III, the duplexes constitute the irregularity assumed. In Experiments V, VI, and VII, the irregularity would be represented by the fact that the first derivative of the quantity curve, dy/dx, again and again happens to have a constant value over short or over longer times.

In Experiment IV, there is no irregularity, *i.e.* no lack of smoothness, and consequently no faint harmonics are audible.

An important fact is that in accordance with my observations this third harmonic, faintly heard, stays inside the region of 200 to 1000 cps., and the fifth inside 500 to 1000, *thus limiting* the hypothesis on spontaneous vibrations of the tympanum.[6]

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[5] Spontaneous as "free" oscillations are defined in *Standard Acoustical-Terminology*, 1960, 15.

[6] For a brief résumé of my view of the hydraulic functioning of the cochlea, see *J. acoust. Soc. Amer.*, 34, 1962, 120a.

## APPENDIX

The hydrokinetic description of an air puff follows.

The *pressure* variation resulting from *the passing of air through* a hole can be understood by the brief hydrokinetic discussion which I now offer. Fig. 8 shows an air column which has just been pushed through a hole and is now left to its own physical resources. For simplicity of discussion I speak of this column as if it were a straight cylinder. During the time the cylinder was *in motion* it was subject to the law of hydrokinetics: "Its surface had a lesser pressure normal to the direction of the motion than parallel to the direction of the motion." (Compare a 'sprayer.') Accordingly it surrounded itself with a sidewise mantle of reduced pressure (cross-hatched). Before the column we may have placed a microphone to register the pressures received, so that we may examine them. At the moment when the column's motion is diminished or stopped, the column's hydrokinetic suction fails; the "mantle" begins to be refilled *from* the column, and the microphone registers a pit following the hump, see Fig. 9. Whoever has built a hydraulic cochlea model as I have, containing a leathery partition,[7] and applies to it a pressure curve like Fig 9, quickly convinces himself by his eyes that the sense organ must convey to consciousness a single loud pitch free from overtones,—as everyone well knows who since August Seebeck has experimented with such sirens.[8] The *inaudibility* of overtones in the fourth experiment helps to demonstrate it. Since the curve in Figure 4 is quite other than sinusoidal, a trigonometric series would demand the hearing of loud overtones there à la Ohm.

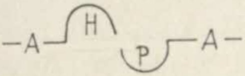

FIG. 9. DIAGRAMMATIC REPRESENTATION OF THE PRESSURES RECEIVED
A = atmospheric pressure; H = pressure-hump in microphone; and P = pressure-pit in microphone.

The *faint* odd harmonics (Beitöne à la Seebeck) are most certainly something *else* than overtones as ordinarily regarded.

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[7] For specifications see Meyer A working model of the cochlea, this JOURNAL, 72, 1959, 293-296. For photographs depicting the outside of the cochlea, see Meyer, The hydraulic principles governing the functions of the cochlea, *J. gen. Psychol.*, 1, 1928, 261 f.: *ibid.* On page 264, one reads, "The concept of a *traveling* bulge is not a part of the theory; the bulge merely lengthens." Nevertheless, during subsequent years, more than once one reads in the literature whereof the following figment is an example: "According to Max F. Meyer a *travelling* wave is extinguished by *damping*" (G. V. Békésy, *Experiments in Hearing,* 1960, 355, 405). The concept of "damping" is non-existent in my publications.

[8] I quote from the publication of an acoustical engineer the following: "We cannot but immensely admire Seebeck to have realized by means so simple as his acoustic siren the short-comings of Ohm's law." See J. F. Schouten, The residue, a new component in subjective sound analysis, *Proc. Konigl. Akad. Wetensch. Amst.,* 43, 1940, 356-365.

# THE PHYSIOLOGICAL LOCUS OF THE SPIRAL AFTER-EFFECT

By SHELDON L. FREUD, Office of Aviation Medicine,
Federal Aviation Agency, Washington, D.C.

It has long been known that if an Archimedes spiral is rotated, an illusory motion of swelling or shrinking, depending on the direction of rotation, will be perceived. If, after the spiral is rotated, it is stopped and *S* looks at a stationary spiral, an after-effect of motion opposite to that produced by the moving spiral will be seen. This after-effect is called the Spiral After-Effect (*SAE*).

Although numerous studies on the use of the *SAE* as a diagnostic test for organic brain-damage have been published, these studies have largely ignored the theory underlying the effect and have not investigated the crucial question of the physiological locus of the *SAE*. Theories that have been proposed to explain the appearance of the effect fall into two general categories: (1) peripheral theories, which attribute it entirely to processes within the retina itself; and (2) central theories, which take the position that the effect is a result of processes within cortical or sub-cortical structures.

A demonstrated transfer of the effect from a stimulated to an unstimulated eye has been generally accepted as strong evidence of centrality. Results of a number of studies on interocular transfer, however, have been far from consistent and the question of this transfer has remained an unresolved subject of debate in the literature since 1871, when Dvorak published a report of observed transfer.[1] Among the early studies, Exner, Von Szily, and Ehrenstein also reported transfer while Budde, Hunter, and Durop failed to find it.[2] More recently, Holland reported 70% transfer,[3] and Day cites Seagrim as having obtained transfer.[4]

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* Received for publication July 11, 1962. This paper is based upon a portion of a Doctoral dissertation completed at the University of Connecticut. The writer is indebted to Dr. A. Robert Rollin for his assistance in this study.

[1] V. Dvorak, Versuche über Nachbilder von Reizveränderugen, *Sitz. Akad. Wiss. Wien*, 61, 1871, 257-262.

[2] S. Exner, Ueber das Sehen von Bewegungen und die Theorie des zusammengesetzten Auges, *Sitz. Akad. Wiss. Wien*, 72, 1875, 156-189; A. von Szily, Zum Studium des Bewegungsnachbildes, *Z. Psychol. Physiol. Sinnesorg.*, 42, 1907, 109-114; Walter Ehrenstein, Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrnehmungen, *Z. Psychol.*, 96, 1925, 305-352; E. Budde, Ueber metakinetische Scheinbewegungen und über die Wahrnehmung von Bewegungen, *Arch. Anat. Physiol.*, 1884, 127-152; W. S. Hunter, The after-effect of visual motion,

It is difficult to evaluate these studies, since many variables now known to be important in determining the intensity of the *SAE* were often not reported (*e.g.* speed of rotation, lighting, size of spiral, testing distance, and duration of fixation). It is likely that the conflicting results stem from incomplete control of these variables.

A carefully controlled study of interocular transfer might have resolved the question. By applying the technique of hemiretinal transfer, however, it was possible to obtain the same information as in simple interocular transfer and, in addition, to relate the effect more precisely to the known neural pathways of the visual system. In the hemiretinal technique, one hemiretina is stimulated by a rotating spiral and a stationary spiral is then projected upon a different hemiretina. If cortical processes are crucial, transfer of the after-effect should occur between homonymous hemiretinas (*i.e.* those which project to the same cortical hemisphere) and transfer should *not* occur between non-homonymous hemiretinas (*i.e.* those which project to different cortical hemispheres). On the other hand, if the crucial processes take place at the retinal level, transfer would be expected between hemiretinas of the same eye rather than between hemiretinas in different eyes. This latter explanation is based upon the well-known fact that there are multiple and complex interconnections within a single retina but no known direct interconnections between the two retinas.[5]

Two experiments were carried out. In the first, transfer of the *SAE* was determined by having *S* report 'yes' or 'no' if the effect appeared in the unstimulated hemiretina after stimulating a homonymous hemiretina. In the second experiment, *S* reported presence of effect by pressing a button connected to an interval-timer. In this way, a measure of the duration of the effect was obtained.

### EXPERIMENT I

*Subjects.* Ten *S*s were selected from among undergraduate students, graduate students, and clerical employees at the University of Connecticut. Included in the sample were 7 men and 3 women with an age-range of 17 to 49 yr., a mean age of 29.0 yr. and a median age of 28.5 yr. None of the *S*s had a history of recent illness, convulsions, or serious injury.

*Apparatus.* The apparatus consisted of a table, 50 in. long and 30 in. wide, and 30 in. high, which had mounted vertically upon it a sheet of peg-board enclosed in

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*Psychol. Rev.,* 21, 1914, 245-277; G. Durop, Le problème des impressions de movement consecutives d'ordre visuel, *Année Psychol.,* 29, 1928, 1-56.

[3] H. C. Holland and H. R. Beech, The spiral aftereffect as a test of brain damage, *J. Ment. Sci.,* 104, 1958, 466-471.

[4] R. H. Day, On interocular transfer and the central origin of visual after-effects, this JOURNAL, 71, 1958, 789.

[5] S. L. Polyak, *The Retina,* 1941, 319-348.

a frame 17 × 13 in. Through the center of the peg-board protruded the drive-shaft of the Spiral After-Effect Apparatus. Attached to the shaft was a standard (920°) Archimedes spiral, 7¼ in. in diameter. A DC power-supply provided the power to rotate the disk. Speed of rotation was fixed at 80 r.p.m. by means of a Strobotac, and recalibration was done by adjustment of the readings of voltage and amperage on the power-supply prior to testing each *S*. Starting and stopping of the spiral-rotation was controlled by an X-ray timer built into the main control-panel. On either side of the spiral, at a distance 6½ in. from the center of the spiral, was a 6-volt bulb mounted flush into the peg-board. These bulbs were wired to an on-off-on switch mounted on the control-panel, thus allowing *E* to switch lights rapidly. An AC step-down transformer set at 4 v. served as an independent constant source of power for the lights. Built into the frame surrounding the apparatus was a sliding door which, when closed by *E* after each trial, blocked the entire apparatus from *S*'s view. All of the exposed portions of the apparatus, with the exception of the spiral-disk itself, were painted a flat light gray. *S* was seated behind a table 8 ft. from the spiral. Attached to the table on an adjustable shaft was an eye-piece with a sliding shutter of the focal-plane type, so constructed that *S* had one eye blocked at all times. By pulling a cord, *S* exposed the previously covered eye and simultaneously covered the previously exposed eye.

*Procedure.* The general procedure was to have *S* focus upon one of the lights with one eye, thus causing the image of the rotating spiral to fall upon a predetermined hemiretina. Then, at the end of the period of rotation, *S* was switched to a particular eye and a particular focusing light so that the image of the stationary spiral fell upon either the same or a different hemiretina, as required by the experimental design.

*S* was given a demonstration of the after-effect and shown how the eye-piece of the apparatus was operated. He was instructed to maintain his focus on the selected light and to avoid looking directly at the spiral itself. General instructions were given on the procedure for switching eyes and lights, and detailed instructions were given for all eye-light combinations. *S* reported presence or absence of after-effect by responding with 'yes' or 'no.'

For purposes of identification, the four hemiretinas were labeled in the following manner: left temporal, A; left nasal, B; right nasal, C; and right temporal, D. Hemiretinas A and C correspond to the homonymous pair projecting upon the left cortical hemisphere, and hemiretinas B and D correspond to the homonymous pair projecting upon the right cortical hemisphere.

The following procedure was employed for stimulating and testing the hemiretinas:

| Hemiretina | Eye | Light |
|---|---|---|
| *A* | left | left |
| *B* | left | right |
| *C* | right | left |
| *D* | right | right |

The 16 conditions (8 homonymous and 8 non-homonymous) were given in random order to each *S*, with a 15-sec. rest-period between trials. A rest-period of 1 min. was given at the end of the eighth trial.

*Results.* Transfer occurred in all 80 of the homonymous hemiretinal conditions and in only 4 of the non-hemiretinal conditions. The Chi-square for these data is 144.76; $p < 0.01$.

*Discussion.* The results of Experiment I clearly demonstrate that homonymous hemiretinas transfer the *SAE,* while non-homonymous hemiretinas fail to do so. The report of transfer in four of the non-homonymous trials can be accounted for by *S*'s inability to maintain perfect focus upon the light. If *S* should glance momentarily at the spiral instead of maintaining his focus upon the light, the after-effect could appear. Another possibility is the general tendency on the part of the *S*s, while engaged in a difficult discriminative task, to report the presence of an effect when it is actually absent (false positives). It is to be noted that these four reports of after-effect in the non-homonymous conditions were spontaneously described by the *S*s as "weak" or "different" compared to the effect seen in the demonstration prior to beginning the experiment.

The almost perfect correspondence between these results and the predictions one would make by assuming that the effect takes place at the visual cortex can be considered as extremely strong evidence for at least a major central component of the *SAE.* They do not, however, conclusively rule out the possibility of a retinal contribution.

In our conditions of transfer, one hemiretina was stimulated with a moving spiral; when the spiral was stopped, another hemiretina was exposed to the stationary spiral. Transfer was defined as a report of the *SAE* under these conditions.

Two physiological explanations for this transfer will be considered. Presumably, the *SAE* results from the interaction of neural firings produced by the present stimulation of the stationary disk with some 'trace' in the nervous system of the previous stimulation produced by the moving disk. One possibility is that the 'trace' represents a neural change or process at the cortical level and interaction takes place there. An alternative possibility is that the trace is at the retinal level and these neural changes or processes result in a particular pattern of firing to the cortex. The *SAE* would then occur upon a meeting at the cortex of these firings of the trace from one hemiretina. It is clear that either explanation requires a cortical or central process. With the latter explanation the possibility of a major retinal contribution remains.

One alternative experiment suggests itself, which, while not a conclusive test of the absence of a retinal contribution as discussed above, could rule out the possibility of any interaction at the retinal level (*i.e.* firings of the trace and firings due to present stimulations interacting in the retina). If the intensity of the after-effect resulting from the stimulation and testing of a pair of homonymous hemiretinas (*e.g.* *A–C*) could be shown to be equal to the intensity of effect obtained when the *same* hemiretina (*e.g.* *A–A*) was both stimulated and tested, it would follow that no part of the effect was due to stimulated hemiretina *A*'s interacting with tested hemiretina *A*. What is required, then, is a comparison between the strength of effect for 'same' and 'different' hemiretinas. This comparison was included in Experiment II.

TABLE I

MEAN DURATION OF SAE

| | Homonymous hemiretinas | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | *AA* | *BB* | *CC* | *DD* | *AC* | *CA* | *BD* | *DB* |
| *M* | 7.50 | 7.65 | 7.93 | 8.08 | 3.92 | 3.93 | 3.67 | 4.08 |
| *SD* | 2.06 | 2.73 | 1.95 | 2.29 | 1.95 | 1.71 | 1.27 | 1.58 |
| | Non-homonymous hemiretinas | | | | | | | |
| | *AB* | *BA* | *CD* | *DC* | *AD* | *DA* | *BC* | *CB* |
| *M* | .05 | .02 | .05 | .03 | 0 | 0 | 0 | 0 |
| *SD* | .14 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

## EXPERIMENT II

In an attempt to rule out any retinal contribution to the *SAE* as discussed in Experiment I, a test of the *SAE* for the 'same–different' conditions was conducted. In addition, a measure of the *SAE* made it possible to evaluate more precisely the non-homonymous retinal responses.

*Method.* The *S*s were the same as in Experiment I. The same apparatus was used, except for the addition of an interval-timer attached to a button which *S* used to report duration of the *SAE*. The procedure was basically the same as in Experiment I, except that *S* reported presence and duration of *SAE* by means of button and timer described above.

*Results.* The results of Experiment II are shown in Table I. The mean duration of the *SAE* for the 'same' (non-transfer) hemiretinal condition was 7.79, as opposed to a mean value of 3.90 for the 'different' (transfer) condition.

A comparison of these means resulted in a $t$ of 3.89 ($p < 0.01$). This finding demonstrates that the *SAE* is significantly greater for the 'same' (non-transfer) condition.

The difference between homonymous and non-homonymous hemiretinas found in Experiment I was confirmed. The after-effect occurred between the homonymous hemiretinal pairs in all 80 of the trials, but between non-homonymous pairs in only 5 of the 80 trials. The range for the mean duration of the *SAE* for non-homonymous responses (0.02–0.05) was clearly of a different order of magnitude than the range for the mean duration of the *SAE* of homonymous hemiretinal responses (3.67–8.08).

*Discussion.* With respect to the attempt to rule out retinal interaction by comparing 'same' (non-transfer) and 'different' (transfer) hemiretinal conditions, it is apparent that it is not supported by the results since 'same' conditions yield significantly greater durations of the *SAE* than 'different' conditions. In the process of transferring the *SAE* from one hemiretina to a different (homonymous) hemiretina, some of the effect is lost.

Two possibilities will be considered to explain the reduced after-effect under the condition of transfer. The first, previously considered, is that retinal interaction contributes to the *SAE* in the 'same' (non-transfer) conditions, but obviously cannot do so in the 'different' (transfer) condition. An alternative explanation may be made on the basis of very recently published anatomical evidence regarding projections from the retina to the visual cortex. According to Teuber, homonymous hemiretinas do not project to precisely the same points on the visual cortex.[6] Thus, if hemiretina *A* (left-temporal) projects to Point 1 on the cortex and its homonymous hemiretina *C* (right-nasal) projects to Point 2 (rather than to the same point), differences such as those obtained might well be expected.

The findings regarding homonymous hemiretinal transfer obtained in Experiment I are confirmed. The amounts of transfer are almost identical. In every case of the 80 homonymous hemiretinal pairs, transfer occurred, and in only 5 of the 80 non-homonymous trials was transfer reported. This was of very brief duration (0.20 to 0.48 sec.) and of a completely different order of magnitude from the homonymous scores (1.22 to 14.80 sec.). Thus again, we have evidence of a major central contribution to the *SAE*.

## SUMMARY

It was the purpose of this study to investigate the physiological locus of the Spiral After-Effect (*SAE*). Experiment I demonstrated 100% transfer between homonymous hemiretinas and almost total absence of transfer between non-homonymous hemiretinas. In Experiment II, duration of the *SAE* was found to be longer for the conditions of non-transfer than for the conditions of transfer; hence, the possibility of a retinal contribution to the after-effect could not be conclusively ruled out. In this experiment, a quantitative study of hemiretinal transfer confirmed the results obtained in Experiment I. It was concluded from these experiments that a major central component exists for the Spiral After-Effect.

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[6] H.-L. Teuber, W. S. Battersby, and M. B. Bener, *Visual Field Defects after Penetrating Missile Wounds of the Brain,* 1960, 113-116.

# A MODEL FOR DECISION-MAKING UNDER RISK

By S. S. Komorita, Wayne State University

In a choice-situation involving risk, there is general agreement that the relevant variables are a prize to be won, a stake to be lost, and the probabilities associated with winning and losing. Accordingly, most studies dealing with single-choice behavior as contrasted with sequential decisions have used wagers and lotteries as stimuli because the relevant variables may be easily manipulated. Such studies indicate that neither utility of prizes and stakes nor subjective probability of events is a linear function of their objective values.[1] Thus, when an individual makes a decision under risk, it might be interpreted that he so behaves as to maximize subjective expected utility (*SEU*) rather than to maximize expected value.[2] Many studies, therefore, have attempted to measure utility of money and subjective probability, and then have proceeded to test whether individuals maximize. *SEU*.[3]

One of the major difficulties of this approach, however, is that independent measures of subjective probability and utility are necessary. An alternative, but similar, approach is to assume a family of functions between the objective and subjective values. Such an assumption would greatly simplify this problem of measurement. In this connection, Galanter used the method of direct magnitude-estimation and scaled utility of money and subjective probability.[4] For utility of money, his results were consistent with Stevens' power law for Class I or prothetic continua, and he proposes a utility power-function of the form, $U = k\ M^n$, where $U$ and $M$ denote utility and money, respectively, and $k$ and $n$ are parameters.[5] Galanter's data, therefore, suggest a power-function relating utility and money, and,

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* Received for publication January 25, 1963. From the Center for the Study of Cognitive Processes, Wayne State University.

[1] Frederick Mosteller and Philip Nogee, An experimental measurement of utility, *J. polit. Econ.*, 59, 1951, 371-404; Cf. M. G. Preston and Philip Baratta, An experimental study of the auction value of an uncertain outcome, this Journal, 61, 1948, 183-193.

[2] Ward Edwards, Prediction of decision among bets, *J. exp. Psychol.*, 50, 1955, 201-214.

[3] Donald Davidson, Patrick Suppes, and Sidney Siegel, *Decision Making: An Experimental Approach*, 1957, 1-80.

[4] Eugene Galanter, The direct measurement of utility and subjective probability, this Journal, 75, 1962, 208-220.

[5] S. S. Stevens, On the psychophysical law, *Psychol. Rev.*, 64, 1957, 153-181.

moreover, such a function has the desirable property of a decreasing marginal utility. Accordingly, the purpose of this study was to test a model for decision-making which assumed a power-function relating utility and money.

*The model.* To reduce the number of parameters which must be estimated, the model, in its present form, is restricted to a pay to play situation, such as lotteries, rather than a situation involving wagers.[6] Equation [1] represents the basic axiom of the model,[7]

$$S = c\, p^a P^b;\ a, b, c, > 0 \qquad [1]$$

where $S$ denotes stake, the amount of money an individual would be willing to pay for a chance to win a given amount of money; $p$ denotes probability of winning; $P$ denotes prize, the amount of money which can be won; and $a$, $b$, and $c$ denote parameters. For rational man, $a = b = c = 1$, and expected value equals zero.

One simple test of the model consists of determining whether probability and prize are multiplicative. Taking the logarithm of both sides of Equation [1], we have Equation [2]:

$$\log S = a \log p + b \log P + \log c \qquad [2]$$

It is readily apparent that log $S$ is linear with log $p$ and log $P$, and if a large number of judgments based upon varying combinations of probability and prize is obtained, the three parameters may be estimated by the method of least squares. If probability and prize are multiplicative, for constant probability, log $S$ should be a linear function of log prize, and similarly, for constant prize, log $S$ should be a linear function of log probability. Secondly, it was hypothesized that the $b$-parameter would be invariant over varying values of probability, and that the $a$-parameter would be invariant over varying values of prize.

## METHOD

Judgments were obtained from 54 undergraduates, 24 men and 30 women, enrolled in an introductory course in psychology. The stimuli consisted of 16 prize-values, ranging from 40 cents to 48 dollars, which were combined with probabilities of 0.25, 0.50, and 0.75. The three probabilities were defined by the cut of an ordinary deck of cards; *i.e.* 'cut a spade,' 'cut a red card,' and 'cut a club, diamond, or heart,' (i.e. *not* cut a spade). The 48 lotteries, consisting of combinations of 16 prize-values and 3 probabilities, were then presented to the *S*s in a random order in a booklet.

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[6] In a wager, an individual loses the stake only if he fails to win, while in a pay-to-play situation, the individual pays a fee to play and loses this fee regardless of the outcome.

[7] Note that this model is very similar in form to one proposed by C. H. Coombs and David Beardslee, On decision-making under uncertainty, in R. M. Thrall, C. H. Coombs, and R. L. Davis (eds.), *Decision Processes,* 1954, 255-285. Although their model dealt specifically with choice between wagers, the proposed model may be generalized to the case of choice between lotteries. For example, if we have two lotteries, 1 and 2, with probabilities $p_1$ and $p_2$ and prizes $P_1$ and $P_2$, respectively, the proposed model would predict that an individual would prefer lottery 1 over lottery 2 if and only if, $c\, p_1^a P_1^b > c\, p_2^a P_2^b$.

The *S*s were instructed that they would make judgments in a lottery-type situation, and to imagine that they would actually pay for a lottery-ticket. A sample stimulus was as follows: 'Win $6.40 if a red card is cut. Pay———[to be filled out]." Thus, for each of the 48 lotteries, the *S*s' task was to indicate how much they would be willing to pay for the opportunity to play the lottery.

In addition to the 54 *S*s in the imaginary lottery-situation, the same stimuli were presented to 8 men, all volunteers, from another class. These volunteers, in individual sessions, were informed that they would be participating in a gambling experiment in which they might actually win or lose some money. These *S*s were also informed that a large number of persons would be participating in the study and that everyone would be making judgments on the same lotteries. Accordingly, for each of the lotteries, the person willing to pay the highest amount, *i.e.* making the highest bid, would be given the opportunity to play the lottery. Finally, to make the situation as realistic as possible, it was emphasized that if one of them was the highest bidder, all would be committed to pay the amount they had put down, and then the lottery would be played. After they had completed their judgments, the purpose of the study was explained to them.

## RESULTS

For the 54 *S*s under the imaginary condition, the judgments for men and women were analyzed separately; for the 8 volunteers, each individual's judgments were analyzed separately. Presenting first the group-data for the 54 *S*s, Fig. 1 shows log $S$ as a function of log $P$ for the three probability-values. It is quite obvious that a good linear fit was obtained. The slopes of these functions correspond to the $b$-parameter, and hence, if this parameter is invariant over varying probability-values, these lines should be parallel. The $b$-parameters for men were 0.98, 0.98, and 0.99; for women, 0.87, 0.92, and 0.94. Thus, there is considerable support for the hypothesis of invariance.

Fig. 2 shows log $S$ as a function of log $p$ for each of the 16 prize-values. For each of these functions, an $F$-test described by Alexander was used to test for significant deviations from a linear trend.[8] For men, each of the 16 tests was based upon 1, 23 *df*, and for women, each of the 16 tests was based upon 1, 29 *df*. None of these tests came close to being significant at the 5% level. Since there is no reason to believe that these functions are non-linear,, a simple test for homogeneity of their slopes consists of testing

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[8] H. W. Alexander, A general test for trend, *Psychol. Bull.*, 43, 1946, 533-557.

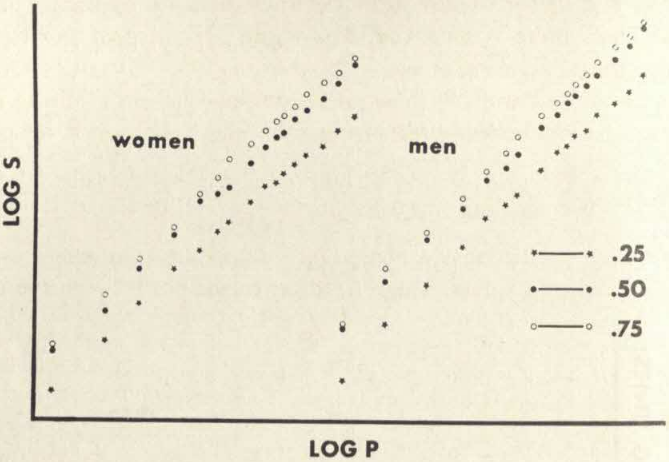

FIG. 1. LOG STAKE AS A FUNCTION OF LOG PRIZE

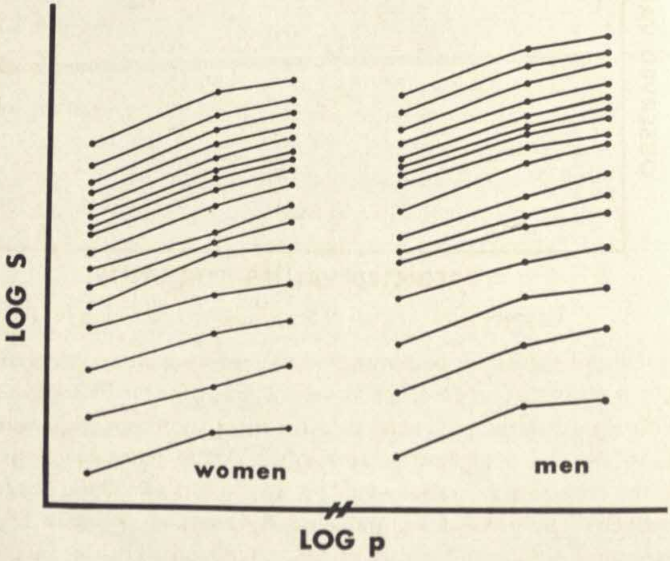

FIG. 2. LOG STAKE AS A FUNCTION OF LOG PROBABILITY

the interaction between probability and prize. With 30, 690 *df* for men, and 30, 870 *df* for women, neither of these tests was significant at the 5% level. Thus, there is some support for the hypothesis of invariance concerning the *a*-parameter as well.

Equations [3] and [4] show the regression-equations resulting from the least-squares estimates of parameters for men and women, respectively:

$$\text{Men: } \log S = 0.755 \log p + 0.994 \log P - 0.309 \qquad [3]$$

$$\text{Women: } \log S = 0.766 \log p + 0.922 \log P - 0.321 \qquad [4]$$

Using these prediction-equations, Fig 3 shows observed values as a function of predicted values. The correlation coefficients between the observed and predicted values for both men and women were all greater than 0.990; thus, it is clear that an excellent fit was obtained for the data of the group.

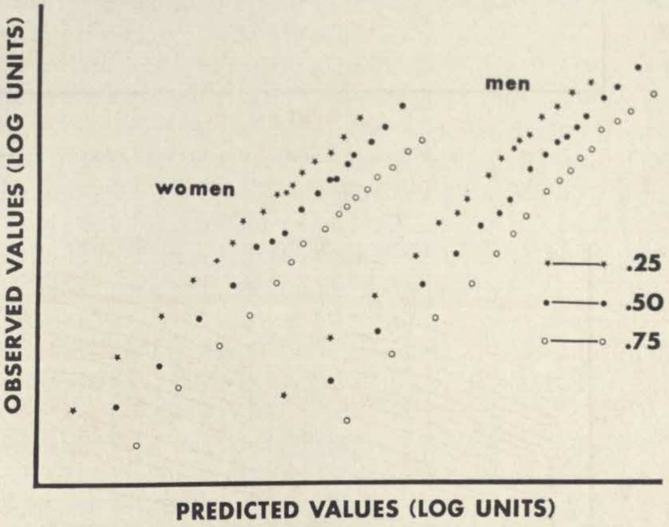

FIG. 3. OBSERVED VALUES AS A FUNCTION OF PREDICTED VALUES

With regard to the individual data for the 8 volunteers, the mean values of the *a*-, *b*-, and *c*- parameters were 1.072, 0.928, and 0.847, respectively, while the ranges of values were 0.623-1.369, 0.850-0.987, and 0.491-1.187, respectively. The plots of log *S* against log *p* and log *P* for each of the 8 *S*s were quite similar to the data of the group, and all of the functions appeared to be linear. Deviations from the lines of regression were slightly larger, however, and there were some irregularities for two of the *S*s. When it is considered that these plots are based upon a single replication, the results seem very encouraging, and it is plausible that if the mean of several replications had been used, a much better fit would have been obtained.

## DISCUSSION

The results of this study provide considerable support for the validity of the proposed model. The *b*-parameter values of 0.994 and 0.922, for men and women, respectively, are, however, sufficiently close to unity that one might reasonably ask if the model fits the data better than a simpler model of the form, $S = c\ p^{a}P$, where the *b*-parameter is assumed to be unity. This simplification reduces the number of parameters which must be estimated, and assumes that utility and money is linearly related. Clearly, the value of 0.994 for men may be attributed to random error; however, there is reason to believe that the value of 0.922 for women is significantly less than unity. Therefore, an *F*-test suggested by Alexander was used to test for the significance of the difference between the *b*-coefficients for men and women.[9] The three *F*-tests, one for each level of probability, each with 1, 728 *df,* were all significant at the 5% level. Since the value of 0.922 for women is significantly different from the value of 0.994 for men, we may reasonably conclude that the *b*-parameter for women differs significantly from unity. Hence, the results indicate that the linear model of utility does *not* fit the data as well as the non-linear model. Furthermore, the data for the 8 volunteers as well as for the 24 men and 30 women indicate large individual differences in the *b*-parameter. Therefore, if individual predictions are to be made, the non-linear model of utility is clearly better than the linear one.

With regard to the three parameters, the *a*- and *b*-parameters may be interpreted as measures of the weights assigned by an individual to probability and prize, respectively. It is plausible that the *c*-parameter is a measure of what Von Neumann and Morgenstern have called utility of gambling or utility for risk.[10] That is, the larger the value of *c,* the greater an individual's utility for risk, and an individual with a *c*-parameter greater than 1.0 enjoys gambling and taking risks relative to his utility for the lottery. In terms of Equation [1], it is as if *S* first evaluates probability and prize with the Weights *a* and *b,* and then makes a judgment regarding what he feels the lottery is worth.

It should be noted that the model provides a method of measuring utility of money and subjective probability. In Fig. 1, assuming that the slopes of these functions are invariant over varying probability-values, the distances on a line perpendicular to these functions should be a measure of subjective probability. For example, Fig. 1 indicates that the distance between probabilities of 0.25 and 0.50 is considerably larger than the distance between probabilities of 0.50 and 0.75. Similarly, in Fig. 2, assuming that the slopes of the functions are invariant over varying prize-values, the distances on a line perpendicular to these functions should be a measure of the utility of money. This proposal for the measurement of utility reduces to the form, $k(\log P_1 - \log P_2)$, and unlike the ratio-scale of utility proposed by Galanter and Stevens, this method yields a logarithmic interval-scale of utility.

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[9] Alexander, *op. cit.,* 552.

[10] John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior,* 2nd ed., 1947, 1-649.

One of the interesting results of this study is that there is no evidence to indicate that subjective probability and utility interact. That is, if subjective probability influences utility and vice versa, the slopes of the lines in Figs. 1 and 2 should not be parallel. Feather has shown that for certain types of tasks, expectation of success (subjective probability) and value of reward or valence of outcome (utility) do interact.[11] He conjectures, however, that for tasks in which the *S* has no control, *e.g.* some random process, the two variables do not interact. Accordingly, if probabilities based upon some game of skill had been used, it is plausible that the *a*- and *b*-parameters would not have been invariant over varying levels of probability and prize-values.

Finally, turning to the evaluation of the model, we find that one of the major limitations of this study is that only three values of probability were used; hence, the adequacy of the model must be restricted to the range of probabilities of 0.25 to 0.75. If the range of probability-values were to be extended, it is conceivable that the *a*-parameter might be a complex function of probability itself. This would be consistent with the results of Coombs and Pruitt's study showing a complex relationship between preference for variance and preference for skewness.[12] If so, the model must be modified to incorporate such preferences.

## SUMMARY

A model for decision-making in a pay-to-play situation was proposed and tested. Judgments were obtained from 54 undergraduate students in

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[11] N. T. Feather, Success probability and choice behavior, *J. exp. Psychol.*, 58, 1959, 257-266; Subjective probability and decision under uncertainty, *Psychol. Rev.*, 66, 1959, 150-161.

[12] C. H. Coombs and D. G. Pruitt, Components of risk in decision making: probability and variance preferences, *J. exp. Psychol.*, 60, 1960, 265-277.

an imaginary gambling situation, and 8 additional volunteer students in a real gambling situation. For the 54 *S*s, grouped data for men and women were analyzed separately, and for the 8 volunteers, judgments of each individual were analyzed separately. The model yielded an excellent fit of the grouped data, and a relatively good fit was obtained for the data of individual *S*s. Although additional tests are indicated, the model appears very promising. A method of measuring a logarithmic interval-scale of utility was also proposed.

# THE EFFECT OF ONE-TRIAL DELAY IN KNOWLEDGE OF RESULTS ON THE ACQUISITION AND RETENTION OF A TOSSING SKILL

By J. J. LAVERY, Toronto, Canada

The effect on the acquisition of a simple motor skill of delaying knowledge of results by a certain number of trials was investigated by Lorge and Thorndike and by Bilodeau.[1] In their ball-tossing experiment, Lorge and Thorndike found that the accuracy with which a ball could be tossed back over the shoulder at a target did not improve when knowledge of results on each trial was delayed by an intervening trial. Bilodeau found that her *S*s were able to improve their proficiency in lever-positioning when knowledge of results was given after as many as five intervening trials. Her results showed that both rate of acquisition and level reached at the end of learning decreased as the number of trials by which knowledge of results is delayed increased. Lavery and Suddon have extended the trial-delay technique to other simple motor skills.[2] Not only were they able to replicate Bilodeau's findings, but they found also that retention was better when knowledge of results was delayed.

The disagreement between the conclusions of these experiments raises an interesting problem. Although there may be a number of reasons why Lorge and Thorndike found no improvement with the trial-delay method, the most probable appears to be their training and testing procedures. Not more than 40 consecutive trials under 'trial-delay' were given, and the total number of trials under this condition in three days was 80. Acquisition was measured in terms of the gain or loss in accuracy resulting from 40 throws under a given training condition. Furthermore, 'trial-delay' was always introduced after at least 40 throws had already been done with immediate knowledge of results. The data, therefore, did not permit any assessment of the effectiveness of trial-delay with naïve *S*s.

On the other hand, it is possible that learning did not take place in Lorge and Thorndike's 'delay' technique because the task was too difficult. In the ball-tossing task, the *S*s could miss the target by throwing to the right or left as well as by over- or under-shooting. This task was made no less difficult by knowledge of results

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* Received for publication March 5, 1963. From the Defence Research Medical Laboratories, Research Paper No. 554.

[1] Irving Lorge and E. L. Thorndike, The influence of delay in the after-effect of a connection, *J. exp. Psychol.,* 18, 1935, 186-194; Ina Bilodeau, Accuracy of a simple positioning response with variation in the number of trials by which knowledge of results is delayed, this JOURNAL, 69, 1956, 434-437.

[2] J. J. Lavery and F. H. Suddon, Retention of simple motor skills as a function of the number of trials by which knowledge of results is delayed, *Percept. mot. Skills,* 15, 1962, 231-237.

which gave no indication of the direction of the error. In the Bilodeau and the Lavery and Suddon studies, the tasks were relatively simple. In essence, their *S*s had to learn to exert a given pressure (either by pulling or pushing) and the error could be made only in two directions, overestimation or underestimation.

The following experiment was undertaken with two objectives: (a) to determine how well *S*s trained consistently under 'trial-delay' will compare, in both acquisition and retention, with those trained under immediate knowledge on a tossing skill; and (b) to measure the effect on the learning progress of both groups of giving the *Ss* specific knowledge of results.

*Subjects and apparatus.* The task was to throw magnetized pellets at a target of concentric circles painted on a steel sheet 80 in. square. Although some pellets slid or bounced, each score was allotted on the basis of the point of impact on the target. The target lay flat on a table 30 in. high. It was identical in size and configuration to the one used by Lorge and Thorndike. The bull's-eye was 8 in. in diameter, the first circle was 16 in. in diameter, the second circle 24 in., and the third circle 32 in. The bull's-eye was scored +8, the next ring +4, the next 0, the next −4, and the remaining area within the 80-in. square was scored −8. This method of scoring was used by Lorge and Thorndike.

*S* stood facing away from the target with the tips of his shoes in line with a marker on the floor 60 in. from the center of the target. *S* tossed pellets over his shoulder at the target, and was instructed to toss each pellet as close as possible to the center of the target. Special care was taken to insure that the *S*s understood the method of scoring. They were reminded each day that the object of the game was to make as high a score as possible. Twenty men served in this study, 16 of them ranging in age from 30 to 41 yr., and 4, from 50 to 60 yr.

*Training conditions.* The *S*s were divided at random into two groups of 10 each. The *S*s in one group were informed of the result immediately after each throw. This procedure was called 'zero-delay' ($D_0$). Those of the other group were informed of the success of each trial only after the following throw had been made. This procedure was designated as one-trial delay ($D_1$).

Each *S* completed a session of 80 throws on each of 10 different days. With both procedures the series comprised a group of three training sessions (*TR*-Days 1, 2, and 3) followed by a testing or retention session (*TS*-Day 1), a second group of three training sessions (*TR*-Days 4, 5, and 6) followed by a second testing session (*TS*-Day 2), and two further testing sessions considerably later (*TS*-Days 3 and 4). In the first three training sessions *S* was given knowledge of results in the form of the score only. In the second group of three training sessions, which began two days after the first testing session, *S* was given both his score and the clock position at which the throw had fallen. No knowledge of results was given in any of the four testing (*TS*) sessions. The first group of training sessions started on a Wednesday for three consecutive days; the second group started a week later, on the following Wednesday, and was also continued for three consecutive days. The first two testing sessions were held on the third day (Monday) following the corresponding training sessions, and the third and fourth testing sessions were held 2 and 4 mo. respectively after the second testing session.

*Results.* The method of scoring and analyzing the data used by Lorge and Thorndike was applied to the results of the first three training sessions to obtain Table I. The values represent the gains and losses which resulted from 40 practice throws. The first column records the difference between the average score of the first four throws and the average score of the last four throws, *i.e.* Throws 1, 2, 3, 4 vs. Throws 37, 38, 39, and 40; values in succeeding columns are differences between average score of the last four trials and the average of the last four of the preceding 40 throws, *e.g.* average of Throws 77 to 80 less average of Throws 37 to 40, and so on. The over-all gain, *i.e.* the difference between the final four throws (at the end of *TR*-Day 3) and the first four throws (at the beginning of *TR*-Day 1) is also shown in Table I.

TABLE I

AVERAGE GAIN PER THROW RESULTING FROM PRECEDING 40 THROWS THROUGHOUT FIRST THREE *TR*-DAYS

|  | *S* | *TR*-Day 1 |  | *TR*-Day 2 |  | *TR*-Day 3 |  | Over-all gain |
|---|---|---|---|---|---|---|---|---|
|  |  | 40 | 80 | 120 | 160 | 200 | 240 |  |
| $D_0$ | 1 | 14 | −4 | −9 | 14 | −10 | 5 | 10 |
|  | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
|  | 3 | 10 | −4 | −7 | 12 | −1 | −6 | 4 |
|  | 4 | 9 | −2 | 2 | −3 | 3 | −2 | 7 |
|  | 5 | −3 | 2 | 0 | −2 | −6 | 6 | −3 |
|  | 6 | 9 | −3 | −3 | 0 | 4 | 0 | 7 |
|  | 7 | −2 | 3 | 0 | 5 | −2 | −2 | 2 |
|  | 8 | 7 | −7 | 3 | 2 | −4 | 1 | 2 |
|  | 9 | 2 | −3 | 6 | −4 | 2 | 0 | 3 |
|  | 10 | 7 | −7 | −3 | −1 | −2 | 7 | 1 |
|  | Mean gain | 5.3 | −2.5 | −1.1 | 2.3 | −1.6 | 0.9 | 3.3 |
| $D_1$ | 11 | 2 | −7 | 7 | −4 | 1 | 3 | 2 |
|  | 12 | −3 | 3 | −1 | 8 | −3 | 0 | 4 |
|  | 13 | −2 | 2 | −2 | −1 | 6 | 2 | 5 |
|  | 14 | 8 | −4 | −1 | 5 | −3 | 6 | 11 |
|  | 15 | 0 | 4 | −1 | −3 | 5 | −3 | 2 |
|  | 16 | 3 | −3 | 5 | 3 | 2 | 1 | 11 |
|  | 17 | −2 | 6 | 1 | −3 | −5 | 7 | 4 |
|  | 18 | −1 | 3 | 1 | −4 | 4 | 6 | 9 |
|  | 19 | 3 | 1 | −7 | 0 | 2 | 1 | 0 |
|  | 20 | −6 | 10 | 0 | −1 | 1 | −2 | 2 |
|  | Mean gain | 0.2 | 1.5 | 0.2 | 0.0 | 1.0 | 2.1 | 5.0 |

The shortcomings of both the scoring method and the analysis used by Lorge and Thorndike are obvious. First, Group $D_1$ shows only slight gain after the first 40 trials, but while further gains after subsequent practice are also slight the improvement is consistent and the over-all gain is considerable. Secondly, Group $D_0$ shows a loss after the initial 40 trials of practice, which supports the contention that Lorge and Thorndike really tested 'trial delay' on *S*s who had already reached some level of training.

A better estimate of the performance of both groups, and one which lends itself to statistical analysis, is the total percentage of shots that fell within the +8 and +4 circles throughout the experiment. This is shown in Fig. 1, for successive blocks of 10 throws in each session.

Fig. 1 shows the difference in rate of improvement between the two conditions. The *S*s trained under immediate knowledge of results show improvement sooner, but as might have been predicted from previous results, the advantage eventually disappears from the training data.[3] Although one *S* in Group $D_1$ showed no over-all gain by the third training day, Group $D_0$ also had one *S* who did not improve and another who actually showed a slight loss in accuracy over the same period of training. Even if the data for these three *S*s are discarded, the performance of Group $D_1$ remains slightly but not significantly superior to that of Group $D_0$. The superiority on the first testing day is quite apparent, and confirms the finding reported earlier.[4]

The effect of providing more specific information in the second group of training sessions on *TR*-Days 4, 5, and 6, is also quite marked and almost immediate. In both training procedures, the *S*s profit from the additional information. The two groups differ markedly, however, on subsequent testing days (*TS*-Days 2, 3, and 4). Although the groups start at about the same level of performance, they begin to diverge after 10 throws and terminate at different levels.

An analysis of trends was computed for all four of the *TS*-Days. It showed a significant difference between the means of the two groups on *TS*-Day 1 ($F = 6.43$ with 1 and 15 *df*). Since two of the *S*s in Group $D_0$ did not improve during training, while only one in Group $D_1$ did not learn, it could be argued that this comparison is biased. If these three *S*s are eliminated from the analysis, an even higher *F*-ratio is obtained ($F = 8.15$ with 1 and 15 *df*). The differences between the group-trends on the second and third testing days show a significant linear component; on the fourth retention-day, a difference appears between groups in both the linear and quadratic components.

Similar analyses were carried out on the first, third, and sixth training days, but no differences between trends or means were found.

*Discussion.* The results of the first training day confirm the suspicion that Lorge and Thorndike would probably have found some improvement in performance with 'trial-delay' had they trained their *S*s consistently with this technique for more than 40 trials.

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[3] Lavery and Suddon, *op. cit.*, 234.

[4] *Ibid.*, 235.

The results of subsequent training in which the direction as well as the

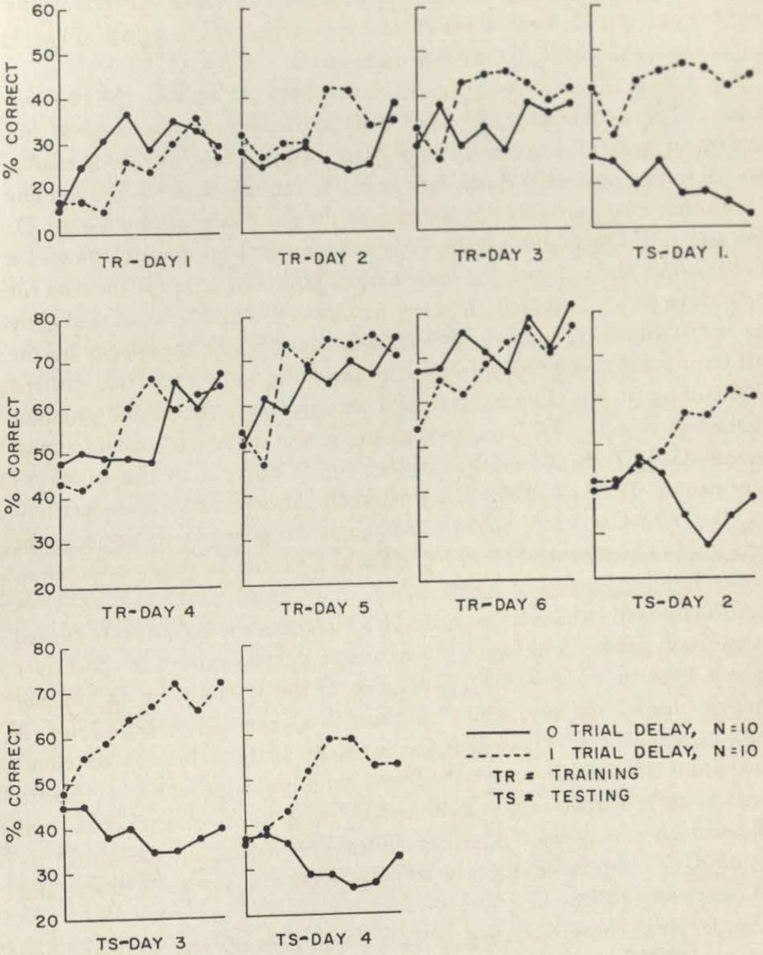

FIG. 1. TOTAL PERCENTAGE OF THROWS SCORING +8 OR +4
Every point represents a block of 10 trials per *S* or a total of 100 throws.

distance of each throw from the bull's-eye is given demonstrate that better accuracy is achieved both with and without 'trial-delay' when knowledge of results is more specific.

The most interesting finding of the present study, and one which was not predicted, is the effect of more specific information on retention following training with 'trial-delay.' On each of the last three testing days (*TS*-Days 2, 3, and 4), the *S*s improved their accuracy from block to block as if they were receiving knowledge of results.

As stated previously, the superior accuracy in retention produced by 'trial-delay' has already been demonstrated. An explanation for this has been offered by Lavery.[5] Briefly, the hypothesis states that any condition of training which enhances the cues inherent in the task will favor retention. When knowledge of results is delayed by one or more intervening trials, *S*, to improve his performance, has to refer the information to a response which was executed two or more trials earlier in the sequence. This means that *S* has to find some way of comparing responses independently of the information supplied by *E*. Presumably *S* is able to do this, since his score improves. So far, no attempt has been made to identify the cues involved, but their existence is assumed on the basis of the learning which is achieved and which must be explained on the basis of some mediating effect between trials.

If we assume the existence of such cues and their role in training, then we can venture the explanation that these cues represent a distinct advantage in retention-tests in which knowledge of results is not given. The present data allow us to make a clearer distinction between the view outlined above and Annett's contingency-hypothesis. Annett states that performance during retention depends on the "relative informativeness" of intrinsic (cues inherent in the task) and extrinsic feedback of knowledge; performance is higher when this ratio is close to unity.[6] This would explain why retention is better following more specific knowledge of results under both conditions, *i.e.* one-trial and zero-trial delay. It does not, however, account for the difference in performance between the two conditions. Indeed, if more specific knowledge of results enhances retention under both conditions, we should conclude that under neither condition was the knowledge informative enough and, on the basis of the results of the first training day, that the discrepancy was larger for Group $D_0$. An increase in the informativeness of knowledge of results should therefore favor Group $D_0$ and produce a larger gain in the retention of this group. This might appear to be the case at the beginning of each of the last training days, but certainly not on subsequent trials. The over-all performance indicates that the additional information favors Group $D_1$. This suggests that, regardless of the relative informativeness of intrinsic and extrinsic feedback of knowledge, *S*s trained under the 'trial-delay' condition have gained a better knowledge of the cues.

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[5] Lavery, Retention of simple motor skills as a function of type of knowledge of results, *Canad. J. Psychol.*, 16, 1962, 308-310.

[6] John Annett, The role of knowledge of results in learning: A survey, US Naval Training Device Center, Port Washington, N.Y., *Tech. Report No. 342-3*, 1961, 47-48.

The hypothesis outlined above does not necessarily explain why *S*s of Group $D_1$ continue to improve during the later retention-sessions (*TS*-Days 2, 3, and 4) instead of maintaining a constant level of accuracy, as in the previous study.[7] It is suggested that an explanation may be found in the difficulty of the task. Indeed, it should be pointed out that even on the later training days (*TR*-Days 4, 5, and 6) and in spite of knowledge of results being given, *S*s in both groups required from 30 to 40 throws to reach the level of accuracy they had attained at the end of the previous session. In the first retention-session (*TS*-Day 1), no improvement was observed. It is possible that, as a result of the two degrees of knowledge of results, the *S*s in Group $D_1$ were able to use two different sets of cues. Under the first training condition, they were presumably content to concern themselves with a posture or motion which produced a better score. Were this posture maintained in retention, a consistently good score would result. When more specific knowledge of results was given in the later training (*TR*-Days 4, 5, and 6), the same *S*s could take advantage of more specific cues, *i.e.* cues which would not only guide them in their general attitude toward the task but would also allow them to correct the error of specific responses if the situation required. For instance, it is possible that following the first three training sessions (*TR*-Days 1, 2, and 3), the *S*s knew how to stand and what motion to use to get a good score, and that after further training with more specific knowledge (*TR*-Days 4, 5, and 6) they were able to use some auditory or kinesthetic cue which allowed them to correct their performance after each response.

*Summary*. The effect of a delay of one trial in knowledge of results on the acquisition and retention of a tossing skill has been measured. The results show that acquisition is slightly slower under this condition but retention is better. Furthermore, if the *S*s are given more specific knowledge of results during training, they subsequently perform in retention as if they were being given knowledge of results.

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[7] Lavery and Suddon, *op. cit.*, 235.

# S–R VS. R–S RECALL AND R-TERM VS. S-TERM RECALL FOLLOWING PAIRED-ASSOCIATE TRAINING

By SLATER E. NEWMAN, University of North Carolina
and CLIFTON W. GRAY, Norwich Hospital, Connecticut

This paper reports the results of three experiments. The first compared the number of correct S–R and R–S pairs following paired-associate training. All terms were easy to pronounce. In the second experiment, half the *S*s were exposed to the same lists as in the first; the other half were exposed to lists with hard-to-pronounce terms. Half of the *S*s in each condition were given an S–R test following paired-associate training, and the other half were given an R–S test. The third experiment replicated the second except that stimulus-term recall and response-term recall were substituted for the S–R and R–S tests. The predictions were: (1) that the number of correct S–R pairs will exceed the number of correct R–S pairs (Experiments I and II); (2) that the number of correct R-terms during R-term recall will exceed the number of correct S-terms during S-term recall (Experiment III); and (3) and (4) that each of these effects will be greater at low than at high levels of pronounceability (Experiments II and III).

Prediction 2 was based on a model of paired-associate learning which holds that *S* selects a mediating response to the response-term with stimulus-properties sufficient to lead to overt emission of the response-term (*e.g.* its pronunciation or spelling).[1] For the stimulus-term such selection is less likely. Since the stimulus-term must occur as an overt response during the R–S test and the response-term as an overt response during the S–R test, the number of S–R pairs will exceed the number of R–S pairs (Prediction 1). Predictions 3 and 4 were based on the assumption that the tendency for *S* to be responding to the entire stimulus-term at the end of paired-associate training is inversely related to difficulty in pronouncing the term; his tendency to be responding to the entire response-term at the end of training is not.

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* Received for publication May 31, 1963. This is a technical report (No. 6) prepared under Office of Naval Research contract Nonr 486(08). Harold Tragash collected the data on all three experiments and performed some of the analyses.

[1] S. E. Newman, A mediation model for paired-associate learning, *Office of Naval Research Tech. Rep. No.* 1, Contract Nonr 486(08), 1961.

Data supporting Prediction 1 have been provided by Ward, and Prediction 2, by Cunningham.[2] They used the same lists and the same procedures during paired-associate training. Asch and Ebenholtz have also provided data supporting Predictions 1 (Experiment V, Group 2) and 2 (Experiment 1) when S-term as well as R-term pronunciation was required during paired-associate training.[3] When practice with both terms of the pairs preceded paired-associate training and a free recall rather than an anticipation procedure was used (Experiment V, Group 1), no differences were observed in performance on the S–R and R–S tests.

There are no experiments which provide a direct test of Predictions 3 and 4.

## METHOD AND PROCEDURE

*Subjects*. In each experiment, the *Ss* were undergraduate men at the University of North Carolina. In Experiment I, *N* was 40, and in Experiments II and III, *N* was 80.[4] In each experiment a balanced Latin square was used to assign *Ss* to the experimental conditions.

*Materials*. Twelve trigrams of pronounceability value less than 3.50 were used to construct a six-pair list (List A).[5] List B was constructed by reversing the positions of the stimulus- and response-terms. No two terms began with the same letter. The pairs were TUD-VER, FID-ZUN, HOV-SUL, REL-JAD, LAR-BOT, and MOG-DIR. These EP (easy-to-pronounce) lists were used in all three experiments.

Two HP (hard-to-pronounce) lists, C and D, were used in Experiments II and III. They were constructed in the same way as A and B except that the pronounceability-values of each item exceeded 8.00. The pairs were HTF-VGJ, QZP-DGM, CKB-XFH, JPV-RZQ, ZJM-KBV, and GVS-MBK.

*Procedure: Experiments I and II*. A pairing-test procedure was used during training. There were 21 pairing-trials, each followed by a test-trial. On the pairing-trials each of the six pairs was presented. On the test-trials each of the stimulus-terms was presented by itself, and *S* was required to spell its appropriate response-term. A 2-sec. rate was used during both pairing- and test-trials. The intertrial interval was 4 sec. Three different pairing orders and three different test-orders were used to reduce the likelihood that serial position would serve as a cue.

After 21 paired-associate training trials, half of the *Ss* under each condition were given an S–R test (Test 22) and then an R–S test (Test 23). For the other half the order of these tests was reversed. A 2-sec. rate was used on each test.

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[2] S. W. Ward, Strength of S–R and R–S pairs as a function of the number of S–R trials, Unpublished M.S. thesis, North Carolina State College, 1960, 24-42; Joseph W. Cunningham, S-term and R-term recall as functions of S–R training, Unpublished M.S. thesis, North Carolina State College, 1961, 38-67.

[3] S. E. Asch and S. M. Ebenholtz, The principle of associative symmetry, *Proc. Amer. Phil. Soc.*, 106, 1962, 135-163.

[4] Data from 3 of the original 83 *Ss* in Experiment III were not used because of difficulty with equipment or procedural errors by *E*. The three *Ss* were in the groups that learned pairs with hard-to-pronounce terms.

[5] B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning*, 1960, 371-372.

*Procedure: Experiment III.* The procedure was the same as for Experiments I and II, except that following paired-associate training, R-term recall and S-term recall replaced the S–R and R–S tests. Half the *S*s had S-term recall and then R-term recall; for the other half the order was reversed. During S-term and R-term recall, *S*s were given 30 sec. to spell aloud all the terms they could remember from the appropriate category.

## RESULTS

*S–R training: all experiments.* The *S*s varied widely in performance during the 21 training trials when all were treated alike except for lists. The total number of correct responses cumulated over all 21 trials ranged from 1 to 115 with EP-lists and from 0 to 44 with HP-lists. Means are reported in Table I. Analyses of variance of these data showed no significant differences between groups which were later differentiated by Order of Recall, and no differences between lists of the same pronounceability ($p > 0.05$).

TABLE I

MEAN SCORES DURING TRAINING

| Exp. | Pronounce-ability | 1st Recall | Correct | Omis-sions | Substitu-tions | Inven-tions (3 letters) | Inven-tions (others) |
|---|---|---|---|---|---|---|---|
| I | Easy | S–R | 48.40 | 54.90 | 8.20 | 14.20 | .30 |
| | | R–S | 48.10 | 56.00 | 9.75 | 11.85 | .30 |
| II | Easy | S–R | 54.15 | 46.40 | 8.85 | 15.60 | 1.00 |
| | | R–S | 64.50 | 39.50 | 8.35 | 12.65 | 1.00 |
| | Hard | S–R | 12.35 | 78.10 | 6.95 | 27.40 | 1.20 |
| | | R–S | 10.55 | 88.15 | 7.05 | 19.10 | 1.15 |
| III | Easy | R | 63.75 | 42.80 | 9.90 | 8.90 | .65 |
| | | S | 53.65 | 40.90 | 18.00 | 12.40 | 1.05 |
| | Hard | R | 8.45 | 87.65 | 8.05 | 19.60 | 2.25 |
| | | S | 11.60 | 88.75 | 5.25 | 18.70 | 1.70 |

The kinds of errors made by each group during paired-associate training are also presented in Table I. In Experiment I, the most frequent type of error was omissions, followed by three-letter invented terms, substitutions, and invented terms of other than three letters. This order was consistent in all three experiments and in both EP- and HP-lists. Compared to the *S*s in the HP-groups, the EP-*S*s made more substitutions, fewer omissions, and fewer inventions. Twenty-eight *S*s repeated one or more invented responses at least once in Experiment I, 31 EP- and 33 HP-*S*s did so in Experiment II, and 33 EP- and 32 HP-*S*s did so in Experiment III. Forty-three per cent of the invented responses were repetitions in Experiment I, 39% for EP- and 23% for HP-*S*s in Experiment II, and 43% for EP- and 26% for HP-*S*s in Experiment III.

*S–R and R–S tests: Experiment I.* The means on S–R and R–S first-recall tests of Experiment 1 are given in Table II. Analysis of variance showed that the first-recall mean for the S–R condition was higher than for the R–S condition ($F = 6.23$; $df = 1, 36$; $p < 0.05$). Although the effects of list were also significant ($F = 4.68$; $df = 1, 36$; $p < 0.05$), the difference of 1.5 terms attributable to direction of recall was identical in the two lists. For that reason, and because the effects of list were not significant in the other experiments, data for both lists within a treatment were combined in Table II.

On the second test of recall (Trial 23), the direction of recall for each *S* was reversed. The within-*S* differences, S–R score minus R–S score, are summarized in Table II (last column). The hypothesis of a mean difference of zero was rejected separately in the group where S–R preceded R–S ($t = 3.06$; $df = 19$; $p < 0.01$, two-tailed) and in the group where it followed ($t = 4.79$; $df = 19$; $p < 0.01$). Analysis of variance of difference scores gave a nonsignificant *F* for test-sequence ($df = 1, 36$; $p > 0.05$).

TABLE II

MEAN CORRECT DURING 22ND AND 23RD TESTS

| Exp. | Pronounceability | 1st Recall | Mean | 2nd Recall | Mean | Diff.* |
|---|---|---|---|---|---|---|
| I | Easy | S–R | 3.15 | R–S | 1.90 | 1.25 |
| | | R–S | 1.65 | S–R | 3.35 | 1.70 |
| II | Easy | S–R | 3.30 | R–S | 2.50 | .80 |
| | | R–S | 2.55 | S–R | 4.15 | 1.60 |
| | Hard | S–R | .45 | R–S | .05 | .40 |
| | | R–S | .05 | S–R | .80 | .75 |
| III | Easy | R | 4.45 | S | 1.85 | 2.60 |
| | | S | 2.75 | R | 3.65 | .90 |
| | Hard | R | 1.60 | S | .20 | 1.40 |
| | | S | .10 | R | 1.45 | 1.35 |

* In Experiments I and II, S–R test minus R–S test; in Experiment III, R-term recall minus S-term recall.

Thirteen *S*s who had the S–R test first had higher S–R than R–S scores and two had higher R–S scores. Among those who had the R–S test first, 13 had higher S–R scores and none had higher R–S scores.

*S–R and R–S tests: Experiment II.* Analysis of variance of number correct in first recall in Experiment II gave an *F* of 70.18 for pronounceability. *F* was 3.14 for direction ($df = 1, 76$; $0.10 > p > 0.05$). Interaction was not significant ($F < 1$). Variance between pronounceability groups was heterogeneous, but the superior performance of EP- over HP-groups was so large, consistent, and predictable that it does not seem necessary to question its statistical significance. Variance between direction-of-recall groups was homogeneous.

The hypothesis that the mean intra-*S* difference, S–R minus R–S, was zero was separately rejected in each of the four cells of Experiment II shown in Table II. S–R was higher than R–S in each cell. (Smallest $t = 2.35$; $df = 19$, $p < 0.05$, two-tailed.)

The effect of sequence on the intra-*S* difference was in the same direction as in Experiment I (greater when R–S preceded S–R), but, unlike in Experiment I, it was significant. Analysis of variance gave an *F* for sequence of 4.55 ($df = 1, 76$; $p < 0.05$); for pronounceability, 5.38 (same *df* and *p*); and for interaction, less than 1. Thus, although S–R recall was more accurate than R–S recall in all conditions of the experiment, the degree of its superiority was affected by which came first and by how pronounceable the terms were.

Of those who had the S–R test first, 12 EP-*S*s and 7 HP-*S*s had higher S–R than R–S scores, whereas only 4 EP-*S*s and one HP-*S* had higher R–S scores. Of those who had the R–S test first, 13 EP-*S*s and 12 HP-*S*s had higher S–R than R–S scores, whereas only one EP-*S* and no HP-*S*s had higher R–S scores.

*S-term vs. R-term recall: Experiment III.* An analysis of variance of first-recall scores gave an *F* for pronounceability of 116.4 and of 39.5 for direction of recall ($df = 1, 72$; $p < 0.01$ for both). R-term recall exceeded S-term recall. All other *F*s for 'between' effects were less than 1. Since variance again appeared to be heterogeneous as a result of pronounceability, S-term recall was compared to R-term separately within EP- and HP-groups, giving $p < 0.01$ in both.

The hypothesis that the mean intra-*S* difference, R minus S, was zero was separately rejected in each of the four cells of Experiment III shown in Table II. R-term recall was higher. (Smallest $t = 2.72$; $df = 19$; $p < 0.05$, two-tailed). An analysis of variance of difference-scores gave $F = 8.69$ for test-sequence ($df = 1, 76$; $p < 0.01$), 1.59 for pronounceability (same *df*); and 7.78 for their interaction (same *df*, $p < 0.01$). The interaction resulted from the fact that R-term recall exceeded S-term recall by a greater amount when R-term recall came first than when S-term recall came first in the EP-group, whereas order had no effect on the superiority of R- over S-term recall in the HP-group. For *S*s who had R-term recall first, 9 EP-*S*s and 17 HP-*S*s had higher scores on R-term recall than on S-term recall; 1 EP-*S* and no HP-*S*s had a higher score on S-term recall. For those who had S-term recall first, 11 EP-*S*s and 14 HP-*S*s did better on R-term than on S-term recall, while 3 EP-*S*s and no HP-*S*s had higher scores on S-term recall.

*Aided vs. unaided recall.* A comparison of S–R vs. R-term recall or R–S vs. S-term recall is confounded with experimental differences. This confounding is reduced somewhat by an examination of relative instead of absolute differences. In Experiment I, the mean S–R score was about 0.8 of a standard deviation above the mean R–S score, where the standard deviation was based on the error-term from the appropriate analysis. In Experiment II, the corresponding means were about 0.4 *SD* apart in EP-groups and twice that much apart in HP-groups. In Experiment III, the R-term mean was more than one *SD* above the S-term mean in EP-groups and more than two *SD*s above in HP-groups. These data, while not conclusive, suggest that the superiority of S–R over R–S association may be less than the superiority of R-term over S-term recall.

## DISCUSSION

The results of these experiments show that the directional effect is surprisingly small under the conditions studied and requires a large *N* to be established. In Experiment II, it failed in inter-*S* comparisons to reach the 5% level of significance. It was a consistent effect, however; more S–R pairs than R–S pairs were recalled in each of the 12 cells of the first two experiments, both in inter-*S* and in intra-*S* comparisons. All intra-*S* comparisons showed highly significant effects of direction under both sequences and all conditions. Prediction 1, therefore, is supported. The data supporting Prediction 2 concerning R-term and S-term recall can be used to explain, at least partly, why more S–R than R–S pairs were learned: Paired-associate training fosters the selection by *S* of a mediating response to the response-term with stimulus-properties sufficient to lead to its overt emission (*e.g.* its pronunciation or spelling). For the stimulus-term, such selection is less likely, particularly where, as in these experiments, *S*s did not have to give the stimulus-terms during paired-associate training. It has still to be determined whether, as Asch and Ebenholtz predict (and as is derivable from the above explanation), performance on the R–S test will equal that on the S–R test when S-terms are as 'available' as R-terms.[6] This prediction is probably best evaluated by use of a recognitive rather than a recall-test. It would be desirable, also, if during paired-associate training, only pairing trials were used and pronunciation of both terms were required.

Predictions 3 and 4 specify a greater effect of direction in HP- than in EP-terms. Table II shows the opposite trend, which was not statistically significant. When, however, the differences in the means for first recall between forward and backward groups are expressed in terms of *SD* units the results are reversed: The first recall means were twice as far apart in *SD* units in the HP- as in the EP-groups in Experiments II and III. But learning was so uniformly poor in HP-groups that this result should probably not be taken in support of Predictions 3 and 4 until it is confirmed in groups which show more learning.

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[6] Asch and Ebenholtz, *op. cit.*, 135-163.

In a post-experimental inquiry for Experiments II and III, HP-*S*s reported most frequently responding to a *part* of the stimulus-term (83% and 91% of the responses on the post-experimental inquiry for Experiments II and III, respectively), but to the entire response-term (66% and 78% of the responses); whereas EP-*S*s reported most frequently responding to the entire stimulus-term (50% and 73%) and the entire response-term (60% and 76%). These data also suggest that Predictions 3 and 4 might have been supported had *S*s from the HP-groups learned the response-terms as well as *S*s from the EP-groups.

In each experiment many *S*s repeated invented responses. This replicates results of Newman, Cunningham, and Gray and may be explained, at least in part, by assuming that when *S* has heard himself respond, the probability is increased that he will respond in the same way again.[7]

## SUMMARY

The following predictions were tested in three experiments: (1) following paired-associate training, the number of correct S–R pairs will exceed the number of correct R–S pairs; and (2) the number of correct R-terms during R-term recall will exceed the number of correct S-terms during S-term recall. Each of these effects was predicted to be greater at low than at high levels of pronounceability (Predictions 3 and 4). The data supported Predictions 1 and 2 but not 3 and 4.

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[7] S. E. Newman, J. W. Cunningham and C. W. Gray, Asymmetry of R-term vs. S-term recall as an explanation for S–R vs. R–S asymmetry, *Office of Naval Research Tech. Rep. No. 5*, Contract Nonr 486(08), 1963; Newman, *op. cit.*, 20-21.

# THE EFFECT OF BODY TILT ON TACTUAL-KINESTHETIC PERCEPTION OF VERTICALITY

By MARTIN BAUERMEISTER, HEINZ WERNER, and SEYMOUR WAPNER,
Clark University

Since Aubert discovered that body tilt led to changes in the perception of the vertical, a number of studies have been performed concerning this phenomenon.[1] Most of these studies have been limited, however, to visual perception of verticality and little is known about perception of verticality in the tactual-kinesthetic modality.[2] The present study attempts a systematic investigation of the effect of body tilt on the tactual-kinesthetic perception of verticality, employing a wide range of body tilts and assessing apparent verticality by means of a tactual-kinesthetically adjusted object. A further factor presumed to affect apparent verticality was introduced, namely, the use of both hands versus one hand (right, left).

*Method: (1) Apparatus.* The *Ss* were tilted laterally in an iron framework which could be rotated 360°. Body and head of the *S* were fully supported by adjustable boards. A metal bar, 40 cm. long and 4 mm. wide, was used as an indicator of apparent verticality. The bar was completely balanced and could be rotated 360° around *S*'s sagittal axis. Its pivot coincided with that of the tilting frame to which the bar was attached.

*(2) Subjects.* Sixty *Ss*, 30 men and 30 women, were tested; their average age was 18 yrs. ± 2.13 yr., ranging from 16 to 26 yr.

*(3) Test conditions.* Each *S* was tested under 20 conditions of body tilt: 90°, 80°, 70°, 60°, 50°, 40°, 30°, 20°, 10°, and 0° to the left and right respectively. The nine positions of body tilt to the right were systematically randomizd. Each tilt was then followed by an equally large tilt to the left. Two upright positions were used. One of these 0° positions was measured after a tilt to the left side and is represented as '+ 0°', the other 0° position was measured after a tilt to the right

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[1] Hermann Aubert, Eine scheinbare bedeutende Drehung von Objekten bei Neigung des Kopfes nach rechts oder links, *Virchow's Arch. path. Anat. u. Physiol.*, 20, 1860, 381-393.

[2] Moriz Sachs and Josef Meller, Untersuchungen über die optische und haptische Lokalisation bei Neigungen um eine sagittale Achse, *Z. Psychol.*, *31*, 1903, 89-109; Seymour Wapner and Heinz Werner, Experiments on sensory-tonic field theory of perception: V. Effect of body status on the kinaesthetic perception of verticality, *J. exp. Psychol.*, 44, 1952, 126-131; J. H. McFarland, Seymour Wapner, and Heinz Werner, The relation between perceived location of objects and perceived location of one's own body, *Percept. mot. Skills*, 15, 1962, 331-341.

side and is represented as '—0°'. The two 0° positions were distributed randomly between the positions of tilt.

The *Ss* were blindfolded and had to adjust the metal bar to a position which appeared vertical. In each of the 20 body positions, every *S* made six adjustments. Two of these adjustments were made with both hands, two with the right hand, and two with the left hand. For the first of these two judgments the metal bar was started 40° to the right of objective vertical and for the second judgment, 40° to the left as viewed by *S*.

The *Ss* were divided into three groups of 20 *Ss* each. One group of *Ss* made the adjustments in the sequence: both hands, right hand, left hand; one group in the sequence: right hand, left hand, both hands; the remaining group in the sequence: left hand, both hands, right hand.

*(4) Measures.* Deviations of apparent from objective vertical were measured with respect to the plumbline. Clockwise (CW) deviations, as viewed by *S*, were designated as plus (+), counterclockwise (CCW) deviations as minus (—).

*(5) Treatment of data.* The raw data were prepared for statistical analysis by computing points of subjective equality. A point of subjective equality was determined as the midpoint between the positions in which the metal bar first appeared as vertical when it was started to the left and when it was started to the right of objective vertical. A treatment-by-subjects analysis was applied to test the over-all effects of body tilt on apparent verticality. The variance between the treatments (tilt) was further analyzed with respect to polynomial trend components (to the fifth degree) according to the trend analysis suggested by Grant to show the trend of deviations of apparent from objective vertical as a polynomial function of the degree of body tilt.[3]

*Results: (1) Effects of body tilt upon adjustments.* Increasing body tilt led to changing deviations of apparent from objective vertical, which appeared as a nonlinear function of the degree of tilt. The results are presented in Fig. 1.

With body tilt increasing to approximately 70°, there was a general tendency of increasing displacements of apparent from objective vertical opposite to the direction of tilt. With further increasing tilt, up to 90°, this tendency reversed so that there was a decreasing displacement of apparent from objective vertical opposite body tilt. This trend pertained to adjustments with both hands as well as with the right and left hand. The significance of the effects of body tilt and particularly the reversal of the displacement tendency was affirmed by the statistical analysis which showed significant ($p < 0.001$) effects of body tilt on apparent vertical for adjustments with both hands as well as with the right and left hand. The trend analysis of the total curves of deviations of apparent from objective vertical between 90° of body tilt to the left (CCW), through upright, to 90° of body tilt to the right (CW) showed significant ($p < 0.001$) cubic and linear polynomial trend components for adjustments with both hands, with the right hand, and with the left hand. The three sets of deviations, as presented in Fig. 1, may accordingly be characterized as fitting sloping (linear) S-shaped (cubic) curves.

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[3] D. A. Grant, Analysis of variance tests in the analysis and comparison of curves, *Psychol. Bull.*, 53, 1956, 141-153.

*(2) Difference due to hands used in making adjustments.* The general finding was (1) that adjustments with the left hand were located to the right of adjustments with both hands and (2) adjustments with the right hand were located to the left of adjustments with both hands. Right-hand adjustments and left-hand adjustments were nearly equidistant from adjustments with both hands. Consequently, the three curves presented in Fig. 1 are practically parallel. The significance of these findings was affirmed by the statistical analysis. The comparison of adjustments with both hands versus with the right hand, and with both hands versus with the left hand, showed significant ($p < 0.001$) 'between-hands' variances which indicate the significance of the distance between the curves. Neither the interaction term 'body tilt by hands' nor the polynomial trend components of this source of variation is, however, significant; this indicates that the deviations of apparent from objective vertical are polynomial func-

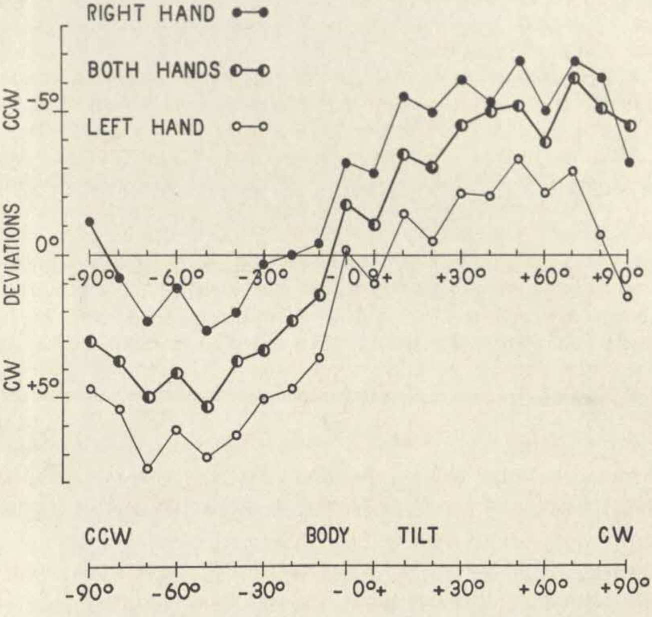

FIG. 1. DEVIATIONS OF APPARENT FROM OBJECTIVE VERTICAL AS A FUNCTION OF THE DEGREE OF BODY TILT

tions of the same order of the degree of body tilt whether the adjustments were made with both hands, the right hand or the left hand.

*Discussion.* The interpretation of the findings is based on an organismic theory of perception, in particular "sensory-tonic field-theory of perception" as developed by Werner and Wapner.[4] The first and fundamental postulate of this theory pertains to the field theoretical nature of perception. It assumes that a perceptual property corresponds to a particular relation between organismic state (psychophysiological) and stimuli issuing from an object (psychophysical). One of the basic relationships is that of stability-instability which relates to a homeostatic principle.

In the experimental situation employed, stability of organism-object relationship pertained to the spatial reference system of external space, in particular, to one of its main axes, the vertical. A stable relation between organism and object was reflected in the perceptual experience that the metal bar was vertical. An unstable relation was reflected in the experience of tilt with respect to verticality.

The relation of stability-instability entails organismic equilibrial states which are involved in the maintenance of upright posture and underlie the perceptual experience of directions defined with respect to the vertical dimension in space. These equilibrial states may be represented by an hypothetical equilibrial axis. Stability of organism-object relationship can now be defined in terms of the relation between the direction of a proximal stimulus and the direction of the dynamic equilibrial axis. Coincidence of direction of equilibrial axis and metal bar leads to the perceptual experience of verticality. Noncoincidence leads to the experience of tilt. These concepts are illustrated in Fig. 2.

The stability-instability relation will be considered with respect to the two factors involved in the present experimental situation: *(a)* asymmetric stimulation due to body tilt, and *(b)* symmetric (both hands) versus asymmetric (one hand) handling of the object.

*(a)* A stable relation in an equilibrial system has to be conceived of as a particular interaction between polar forces. With respect to asymmetric stimulation due to body tilt, these opposing forces may be defined as polar reaction-tendencies: one, toward ('yielding to,' 'turning to'), the other, away from ('counteracting') stimulation. Variation of asymmetric stimulation introduced by increasing degrees of body tilt is a determining factor in the interplay of these forces. There may be a dominance

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[4] Heinz Werner and Seymour Wapner, Sensory-tonic field theory of perception, *J. Personal.*, 18, 1949, 88-107; Toward a general theory of perception, *Psychol. Rev.*, 59, 1952, 324-338; Sensory-tonic field theory of perception: Basic concepts and experiments, *Revista di psicologica*, 50, 1956, 315-337; Seymour Wapner and Heinz Werner, *Perceptual Development*, 1957, pages 1-13.

of the yielding or of the counteracting reaction indicated by increasing shifts of the equilibrial axis (and consequently apparent vertical) toward or away from the direction of tilt.

Following Goldstein's notion of the interaction of yielding and counteracting in the establishment of postural equilibrium, it is postulated that the primary reaction of the organism is the turning toward stimulation, *i.e.* yielding.[5] If one assumed that the degree of yielding increased directly with the degree of stimulation due to body tilt, one would expect increasing displacement of apparent verticality into the

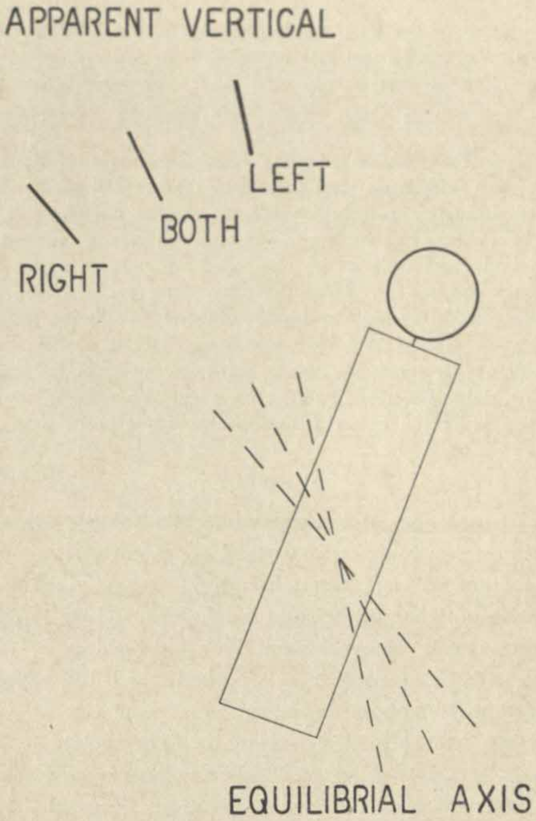

FIG. 2. SCHEMATIC REPRESENTATION OF THE RELATION BETWEEN EQUILIBRIAL AXIS AND TACTUAL-KINESTHETIC VERTICALITY FOR ADJUSTMENTS WITH BOTH HANDS, THE RIGHT HAND, AND THE LEFT HAND

direction of tilt. This would result in a relative instabilty of spatial organization. To maintain a stable spatial reference system the organism evokes forces counteracting the primary tendency of yielding. Within a certain range of tilt the counteracting tendency is reflected in the increasing displacement of apparent vertical opposite to the direction of tilt, which has to be regarded as an overcompensating mechanism due to dark-room conditions. That this tendency reverses for very large angles of tilt is interpreted as indicative of the lessening biological relevance of large tilts for our intercourse with the environment and accordingly the lesser importance of spatial constancy. This interpretation is supported by the known fact that stability conditions, such as spatial constancy, obtain more efficiently in biologically relevant situations.

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[5] Kurt Goldstein, *The Organism,* 1939, pages 439-441.

*(b)* To interpret the effects of symmetric versus asymmetric handling of the metal bar, *i.e.* right-hand adjustments were located to the left, and left-hand adjustments to the right, of adjustments with both hands, the equilibrial model will be employed again. These effects may be considered as due to a process of counterbalancing the asymmetry of body tonus due to the one sided manipulation of the object. This counterbalancing tendency is reflected in shifts of the equilibrial axis, and consequently apparent vertical, opposite to the side of asymmetrically increased body tonus. This interpretation is supported by well-known postural equilibration processes displayed under similar conditions of one sided increase of body tonus. Stretching out the left or right arm while walking on a rail brings about counterbalancing postural changes visible in the inclination of the body opposite to the side of the outstretched arm.

## SUMMARY

Sixty *S*s, 30 men and 30 women, while blindfolded, adjusted a metal bar to a position which appeared vertical (apparent vertical) under body tilts ranging from 90° left, through upright, to 90° right. Adjustments were made with both hands, the right hand alone, and the left hand alone. The following results were obtained: (1) Independent of whether one hand or two hands were used, increasing body tilt led to changing deviations of apparent from objective vertical which were a non-linear function of degree of tilt: with body tilt increasing to approximately 70°, there were increasing displacements of apparent from objective vertical opposite the direction of body tilt; with further increasing tilt, up to 90°, this tendency was reversed. (2) There were differences in effects of body tilt on apparent vertical depending on hands used: Adjustments with the left hand were located to the right of adjustments using both hands, and adjustments using the right hand were located to the left of adjustments with both hands. The results were interpreted in terms of an organismic theory of perception.

# INTERMANUAL TRANSFER OF ADAPTATION TO PRISMS

By Charles R. Hamilton, California Institute of Technology

Inversion, deflection, and other alterations of the visual field, produced by optical or surgical means, cause immediate errors of visually guided movement and localization. Although such errors may persist indefinitely in lower vertebrates, the higher mammals show considerable recovery of normal visuomotor function after sufficient practice.[1] For study of this adaptation, deflection of the visual field by wedge-prisms offers advantages in the ease and rapidity with which compensation is established. We have found, for reaching movements, that adaptation to wedge-prisms acquired through one eye in 'split-brain' monkeys shows immediate and complete interocular transfer, in marked contrast to the lack of transfer of pattern-discriminations learned monocularly.[2] If either 'split-brain' or normal monkeys adapt by reaching to a target with one arm only, there is, however, no transfer from the practiced to the unpracticed arm.[3] This was contrary to expectations based on Helmholtz's report that human *Ss* show such transfer.[4]

The question of intermanual transfer is important when considering at what stage in the visuomotor coördinating mechanism the changes involved in adaptation are introduced. Lack of transfer would preclude changes in parts of the system that control reaching by either arm, as, for example, Helmholtz's suggestion of altered "judgment of the direction of the eyes,"[5] or, more generally, altered judgment of the position of any part of the body necessary for visual localization and common to reaching with either arm. If transfer were to occur, no definite conclusion could be drawn about which parts of the system are altered. Confirmation in people of the results on intermanual transfer obtained with monkeys appeared

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[1] For a review of the earlier work, see K. U. Smith and W. M. Smith, *Motion and Perception*, 1962, 59-104.

[2] Joseph Bossom and C. R. Hamilton, Interocular transfer of prism-altered coordinations in split-brain monkeys, *J. comp. physiol. Psychol.*, 56, 1963, 769-774.

[3] Hamilton, Studies in adaptation to deflection of the visual field in split-brain monkeys and man, Doctoral dissertation, California Institute of Technology, 1964, 18-35.

[4] Hermann von Helmholtz, *Physiological Optics*, 3, 1962, 246-247.

[5] *Ibid.*

necessary before conducting further experiments seeking the locus of adaptation. The experiments reported in this paper investigate the extent to which adaptation of visually directed reaching transfers intermanually in people.

## METHOD

*Apparatus and general procedure.* The apparatus and techniques that we have used for producing and measuring adaptation of the coördination between eye and hand are similar to those described by Held and Gottlieb.[6] *S* looked through a binocular eye-piece, which held laterally oriented 20-diopter wedge-prisms, into an illuminated box. The eye-piece also served to hold *S*'s head in a relatively fixed position. If restricted movement of head and trunk was not desired, the front panel containing the eye-piece was removed. Then either the prisms were mounted in goggles and worn by *S* or a very large 20-diopter prism through which *S* looked was mounted in the box. Goggles without prisms were used to restrict the size of the visual field when appropriate.

*S* sat before the box and adapted by watching one arm through the prisms while moving it back and forth for 15 min., at a rate of about 25 times per min. *E* observed *S*, and, if necessary, made corrective suggestions to insure that each *S* obtained similar exposure.

When prisms are removed after adaptation, an error in reaching (the after-effect), which is in the direction opposite to the error initially induced by the prism, is observed. Recent work has used as a measure of adaptation the increase in magnitude of this error, rather than the decrease in error observed with the prisms in place, to avoid possible uncontrolled cues that might aid correct localization.[7] *S* was tested without the prisms before and after adaptation under conditions of no visual feedback. A mirror-arrangement permitted *S* to mark the apparent positions of three dots in the form of a triangle 4 in. on a side, at the same time preventing *S* from seeing his arm. Each dot was marked five times with each hand; the arm was removed from the box after each trial. The positions of the 15 marks were averaged to give a measure of reaching accuracy. The magnitude of the after-effect for each hand was computed by taking the difference between the average pre- and post-adaptation measurements for that hand. The results were measured and reported in units of 0.1 in.; 1 in. equalled approximately 3 degrees.

Unless otherwise noted, each experiment was designed to cancel possible differences due to adaptation by the right or left hand or to orientation of the prism to the left or right. Whenever the *S*s adapted twice during one experiment, the choice of the second condition of adaptation for the tests was permuted among *S*s to cancel any sequential effects of adaptation. The *S*s were otherwise arbitrarily assigned to different conditions of adaptation within each design.

*Transfer with restricted movement.* Sixteen *S*s adapted with one hand to prisms fixed in the eye-piece. After-effects were determined with each hand. Each *S* adapted twice under these conditions, which restricted movement of head and trunk.

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[6] Richard Held and Norman Gottlieb, Technique for studying adaptation to disarranged hand-eye coordination, *Percept. mot. Skills*, 8, 1958, 83-86.

[7] Richard Held and Joseph Bossom, Neonatal deprivation and adult rearrangement, *J. comp. physiol. Psychol.*, 54, 1961, 33-37.

The time-course of adaptation was sequentially determined for the two hands in 8 *Ss*. First one hand adapted for 10 min.; this was followed immediately by similar adaptation of the other hand. Measurements were taken for both hands before and after each *S* adapted. In addition, adaptation was interrupted after ¼, ½, 1, and 5 min. of practice and the after-effects for that hand determined. Head-restraint was employed for both testing and adaptation.

*Transfer with non-restricted movement.* Twelve *Ss* adapted one hand with the prisms placed in goggles and after-effects were determined as before. In addition, the changes in accuracy of reaching with the prisms in place were checked for each hand. This was also done without visual feedback; these readings were taken immediately after putting the prisms on and just before removing them. In this case the amount of adaptation was calculated from the difference between the initial and final errors as measured with the prisms in place.

An additional 8 *Ss* adapted once with prisms in goggles and once with the large fixed prism. After-effects were again checked with each hand. Four *Ss* adapted under one condition first, and four under the other. The design was balanced as before except for the use of base-left prisms only.

TABLE I

AFTER-EFFECTS WITH RESTRICTED MOVEMENT
(Mean in 0.1 in.)

| | *N* | Mean | *SD* |
|---|---|---|---|
| Exposed Arm | 32 | 9.8 | 7.7 |
| Unexposed Arm | 32 | 0.4 | 5.7 |

## RESULTS AND DISCUSSION

*Restricted movement.* The intermanual transfer results obtained when movements of head and trunk were nearly absent are presented in Table I. The mean of the after-effects is greater than zero for the exposed hand ($p < 0.005$) but not for the unexposed hand ($p > 0.4$). The mean of the exposed hand is significantly larger than the mean of the unexposed hand ($p < 0.005$). There was no correlation between the size of the after-effects of *S*'s exposed and unexposed hand. These results have recently been independently confirmed in three other laboratories.[8]

Fig. 1 presents the data from 8 *Ss* averaged point by point, for the time-course of adaptation determined for each hand. These data further emphasize the independence of the two arms during adaptation when movements of head and body are restricted.

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[8] C. S. Harris, Adaptation to displaced vision: Visual, motor or proprioceptive change?, *Science*, 140, 1963, 812-813; Haratune Mikaelin, Failure of a bilateral transfer in modified eye-hand coördination (paper read at Eastern Psychological Association, 1963; H. B. Cohen, personal communication.

Both these experiments show that the compensation for errors in visually guided hand-localizations produced by looking through wedge-prisms affected only the hand that was actually practiced. The hand that was not observed through the prisms showed no after-effects, nor were savings evident when the unexposed hand subsequently adapted.

The suggestion by Helmholtz that adaptation to prisms results from an altered judgment of the direction of the eyes is excluded for this case by these results, since that hypothesis predicts intermanual transfer.[9] Similarly, an adaptive change in the judgment of the position of any part of the body whose direction relative to the visual target is critical for visual localization and hence for accurate reaching by either hand is apparently ruled out. Only the mechanisms concerned with determining position or movement of the arm seem to remain as loci for the changes that are introduced. Harris has recently shown that, following adaptation to prisms, the after-effects are also present when *S* reaches to an auditory target or to the subjective center, which shows that the changes are not strictly visual or visuomotor. He also presents data to show that the change which occurs during adaptation is in the perceived position of the exposed hand rather than in its motor control.[10] The presence of interocular transfer of similar after-effects in monkeys with surgical separation of the optic chiasm and cerebral hemispheres is also more easily interpreted as a change in the mechanisms involved with the act of reaching rather than as a more visually centered adaptation.[11]

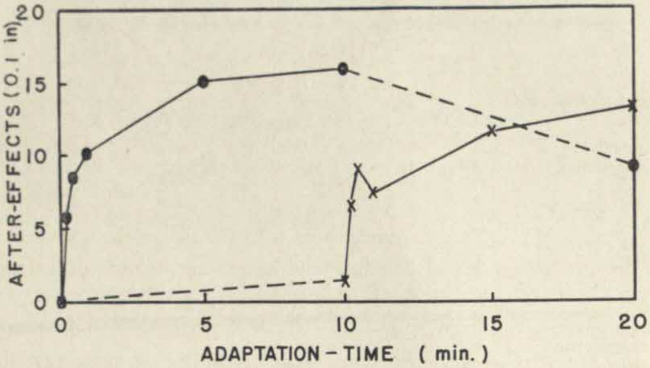

FIG. 1. AVERAGE ADAPTATION-CURVES FOR 8 *S*s
The dots indicate the hand first adapted and the 'x's the hand adapted secondly. The broken lines represent the period during which the hand not being adapted rested on *S*'s lap.

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[9] Helmholtz, *op. cit.,* 246-247.
[10] Harris, *op. cit.,* 812-813; personal communication.
[11] Hamilton, *op. cit.,* 36, 67.

*Non-restricted movement.* The results obtained from the 12 *Ss* who adapted with the prisms placed in goggles are presented in Table II. There were significant after-effects in the unexposed as well as the exposed hands (both, $p < 0.005$). The mean after-effect of the exposed hand, however, is significantly greater than that of the unexposed hand ($p < 0.05$). In this experiment adaptation was also determined from measurements of reaching accuracy made with the prisms in place. When measured in this way, adaptation in both hands was still significant ($p < 0.005$), and the exposed hand was again significantly more adapted than the unexposed hand ($p < 0.005$). The magnitude of adaptation of the exposed hand, when determined as an after-effect, did not differ significantly from the adaptation determined with the prisms in place ($p > 0.1$).

TABLE II

INTERMANUAL TRANSFER WITH NON-RESTRICTED MOVEMENT

(Mean in 0.1 in.)

| Effect measured | *N* | Exposed arm | | Unexposed arm | |
|---|---|---|---|---|---|
| | | Mean | *SD* | Mean | *SD* |
| After-effect | 12 | 9.3 | 6.9 | 4.2 | 3.7 |
| Adapted reaching | 12 | 13.8 | 8.6 | 4.1 | 5.4 |

Apparently the additional freedom of movement facilitated transfer of adaptation to the unexposed hand. This could be at least partially responsible for Helmholtz's report of transfer.[12] Testing with the prisms in place, as did Helmholtz, did not produce larger transfer than the tests measuring after-effects. Transfer might be induced by the extra cues provided by the swimming effects in the visual field that occur when *S* moves his head while wearing prisms. This possibility was tested by comparing the amount of transfer occurring with the prisms in goggles to that obtained using the large fixed prism that permitted free movement of head and body. The results in Table III show significant after-effects in all cases. The after-effects are not significantly different for the two conditions; certainly the transfer is not increased when the prisms are worn in goggles. Transfer therefore appears to be associated directly with the increased stimulation due to movement and kinesthetic feedback. Whether this generalization of adaptation reflects an over-all change in egocentric localization or whether it indicates altered interpretations of only the positions of the body-parts that actually move during adaptation cannot be decided from these data. The latter suggestion would, however, more easily explain why only partial transfer is found, since part of the adaptation would still be attributed to changes in the sense of position of the adapted arm, with the rest of the adaptation (available to either arm) resulting from changes in the sense of position of the neck-implication.

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[12] Helmholtz, *op. cit.*, 246-247.

The fact that the judgment of the position of the eyes was not altered, as indicated by the lack of intermanual transfer of after-effects in the restricted condition, correlates well with the evidence for the lack of proprioceptive position-sense for the eyes.[13] The parts of the body for which sense of position appears to have been changed do have proprioceptive joint receptors. If corollary discharges of motor-messages to relevant parts of the body rather than proprioception were involved in the adaptive changes, as suggested by Held,[14] it would seem that the position of the eyes, for which there is good evidence that corollary motor-discharges exist,[15] would be at least as adaptable as other parts of the body for which there is no evidence of these discharges. It might be suggested that the adaptive changes affect only those members of the body whose position-sense is determined by information from joint receptors.

TABLE III
INFLUENCE OF PRISM ON AFTER-EFFECTS
(Mean in 0.1 in.)

| Type of prism | *N* | Exposed Arm | | Unexposed arm | |
|---|---|---|---|---|---|
| | | Mean | *SD* | Mean | *SD* |
| Goggle-mounted | 8 | 10.3 | 5.3 | 3.8 | 4.6 |
| Box-mounted | 8 | 13.0 | 5.9 | 7.5 | 4.6 |

## SUMMARY

The *S*s moved one arm back and forth for 15 min. while viewing the movement through wedge-prisms. Adaptation of eye-hand localization to the lateral deflection of the visual field was found not to transfer to the unpracticed hand as long as movements of head and torso were restricted. Partial transfer occurred when the *S*s were permitted movements of head and trunk while adapting.

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[13] G. S. Brindley and P. A. Merton, Absence of position sense in the eye, *J. Physiol.*, 153, 1954, 127-130.

[14] Held, Exposure-history as a factor in maintaining stability of perception and coordination, *J. nerv. ment. Dis.*, 132, 1961, 26-32.

[15] E. von Holst, Relations between the central nervous system and the peripheral organs, *Brit. J. anim. Behav.*, 2, 1954, 89-94.

# LANGUAGE-HABITS, S–R COMPATIBILITY, AND VERBAL LEARNING

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By A. D. BADDELEY, Medical Research Council, Applied Psychology Research Unit, Cambridge, England

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The concept of S–R compatibility has been used mainly in the area of sensory-motor skills, where it was developed to explain why certain S–R pairs, said to conform to a population-stereotype, were learned more easily than others.[1] The following experiments test the hypothesis that language-habits will function as a population-stereotype giving rise to effects of S–R compatibility in both the paired-associate and serial learning of nonsense-syllables.

A pair of nonsense-syllables is regarded as compatible if the syllables, when fitted together, give a combination of letters that occurs frequently in the language. S–R compatibility is assessed on the basis of the digram-frequency count of printed English, by Baddeley, Conrad, and Thomson, using a method based on Shannon's guessing game.[2] The method assumes a statistical subject who is given the last letter of the stimulus and attempts to 'guess' the first letter of the response on the basis of the relative frequency of digrams in printed English. The number of 'guesses' required is the measure of compatibility; thus, the compatibility-score for the pair QEM POG is six 'guesses,' because P is the sixth most likely letter following M in English. If the pair is reversed to POG QEM, its compatibility-score becomes 22 guesses, as there are 22 letters more likely than Q, following G. The pair POG QEM may thus be regarded as less compatible than the pair QEM POG, and should thus be harder to learn.

## EXPERIMENT I: PAIRED-ASSOCIATE LEARNING

*Method.* Two groups of *Ss* learned 8 pairs of syllables, so selected as to be compatible in one order (QEM POG), but relatively incompatible in the reverse order (POG QEM). Each group learned one of two lists in which half the pairs occurred in the compatible order, while the other half were in the reverse order. Thus, in List A, Pairs 1 to 4 were compatible and Pairs 5 to 8 incompatible, while in List B,

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* Received for publication July 22, 1963.

[1] P. M. Fitts and C. M. Seeger, S–R compatibility: Spatial characteristics of stimulus and response codes, *J. exp. Psychol.*, 46, 1953, 199-210.

[2] A. D. Baddeley, R. Conrad and W. E. Thomson, Letter structure of the English language, *Nature*, 186, 1960, 414-416; C. E. Shannon, Prediction and entropy of printed English, *Bell. Syst. Tech. J.*, 30, 1951, 50-64.

the order of each pair was reversed, making Pairs 1 to 4 incompatible and Pairs 5 to 8 compatible. It was predicted that the *S*s would learn the compatible pairs more easily.

*Material.* Eight pairs of syllables were selected from Glaze's list, two pairs each of 0, 6.67, 13.33 and 20% associative value.[3] Each pair was compatible in one order, but less so in the reverse, the relative mean compatibility-scores being 10.8 and 19.1 'guesses' respectively.

Two lists, A and B, were made up as described and typed on 35-mm. recording paper. This was made into two endless belts, which were moved, as in a memory-drum, past a window in a black screen. Both belts contained 7 repetitions of the list, and in each repetition the pairs occurred in a different random order.

TABLE I

EASE OF LEARNING PAIRS OF SYLLABLES AS A FUNCTION OF S–R COMPATIBILITY

| Pair Number | Compatibility |  |  |  |  |  |
|---|---|---|---|---|---|---|
|  | High |  | Score* | Low |  | Score* |
| 1 | Zil | Tov | 7.93 | Tov | Zil | 7.60 |
| 2 | Yuf | Laj | 5.80 | Laj | Yuf | 4.93 |
| 3 | Zow | Nux | 9.33 | Nux | Zow | 8.73 |
| 4 | Qem | Pog | 13.53 | Pog | Qem | 12.07 |
| 5 | Jul | Paf | 7.07 | Paf | Jul | 8.67 |
| 6 | Qod | Rix | 11.80 | Rix | Qod | 8.93 |
| 7 | Kig | Leb | 9.60 | Leb | Kig | 6.73 |
| 8 | Vac | Tef | 8.13 | Tef | Vac | 5.47 |
| Mean |  |  | 9.15 |  |  | 7.89 |

* Mean correct *R*s per *S* (Max. = 20)

*Procedure.* The *S*s, 30 enlisted men, were divided into two groups of 15 each, one of which learned List A, the other List B. Each *S* was tested individually at a 3-sec. rate of presentation. *S* attempted to anticipate the response but did not pronounce the stimulus. Trials were separated by a break of about 15 sec. during which *S* was told how many correct responses he had made. The *S*s practiced to a criterion of three successive errorless trials within a limit of 20 presentations. They were then questioned on their methods of learning. If meaningful associations were not mentioned, they were asked if any of the items had suggested words, and an example was given by *E*.

*Results.* Table I shows the mean number of correct responses for each pair in both the compatible and incompatible orders. The design employed involves two random variables, *S*s and syllables. To demonstrate a definite influence of compatibility, it is therefore necessary to show that any effect is due neither to a few atypical *S*s nor to one or two unusual pairs of syllables.

It can be seen from Table I that seven of the eight pairs of syllables are learned more readily in the compatible order. A Wilcoxon matched-pairs signed-ranks test suggested that this difference is significant ($p <$ 0.05, 1-tail).

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[3] J. A. Glaze, The association value of non-sense syllables, *J. genet. Psychol.* 35, 1928, 255-267.

An analysis of compatibility was then made across the *S*s as follows. Each *S* was given a difference-score based on his total correct response on Pairs 1 to 4, minus his total on Pairs 5 to 8. As predicted, the *S*s learning List A, in which Pairs 1 to 4 were compatible, had higher scores (median + 1.0) than those learning List B, in which Pairs 5 to 8 were compatible (median, − 13.0). A Mann-Whitney test confirmed the predicted difference ($p < 0.01$, 1-tail).

Within the limited range sampled, there was no systematic relationship between associative value and ease of learning. This conclusion is supported by the *S*s' reports. Only 6 of the 30 *S*s gave meaningful associations; these *S*s reported a total of 10 associations, 7 of which were names or pseudonames *e.g.* Zil Tov—a Russian name. All 10 associations concerned both the stimulus and response of the pair concerned.

## EXPERIMENT II: SERIAL LEARNING

*Method.* Experiment II examined the hypothesis that S–R compatibility would also influence the serial learning of nonsense-syllables. A series of 8-item lists was prepared in which inter-syllable compatibility was high, and the learning of such lists was compared with the learning of the same items in the reverse order. All *S*s learned two lists of the same associative value, one a compatible sequence and the other a reversed list, in counterbalanced order.

*Material.* Ten sequences of eight syllables were prepared, two each of 40, 50, 60, 70 and 80% Glaze associative value. From each sequence, two lists were made, one compatible, *i.e.* ZAN-DIB-LUD-REH-TEK-NAL-DAC-TOZ, the other a control consisting of the same items in the opposite order, *i.e.* TOZ-DAC . . . ZAN. A compatibility-score was computed by treating the list as seven successive pairs and using the guessing model described previously. Compatible lists had a mean score of 7.97 'guesses' and control lists a mean score of 14.44 'guesses' per pair.

*Procedure.* The *S*s, 20 enlisted men were divided into five equal groups, one for each level of associative value. At each level there were two lists, A and B, and two degrees of compatibility, high (HiC) and low (LoC). Each *S* was tested twice on successive days, once on each list and once at each level of compatibility. The four *S*s in each group would, therefore, learn the lists as follows, $S_1$, A-HiC–B-LoC; $S_2$, A-LoC–B-HiC; $S_3$, B-HiC–A-LoC; $S_4$, B-LoC–A-HiC.

The lists were typed on 35-mm. recording paper and presented, as in Experiment I, at a 3-sec. rate with a 15-sec. intertrial interval. The *S*s were tested for 20 trials on each day, using the method of anticipation. After the second list they were questioned about strategies of learning, as in Experiment I.

*Results.* Table II shows the percentage of correct responses as a function of compatibility and associative-value. Compatibility is clearly effective at each of the five levels of meaningfulness.

An 'improvement'-score was computed for each *S* by subtracting his Day 1 score from his Day 2 score. If compatibility is affecting performance, this score should be greater for the *S*s who learned the lists in the order Lo C—Hi C, than for those who had the reverse order, in which the effects of compatibility and of practice would tend to counteract each other. A comparison of the two groups by the Mann-Whitney test showed that this was the case ($p < 0.001$, 1-tail), again indicating a clear effect of compatibility.

*Associative value.* Table II suggests a tendency for the more meaningful lists to be learned more easily. This trend did not, however, prove significant by Jonckheere's test,[4] which tests the hypothesis that the observations from a number of groups occur in a predicted order ($z = 1.23$, $p > 0.1$). When questioned about their methods of learning, 8 of the 20 *S*s reported associations. They gave a total of 23 associations, which, unlike those reported in paired-associate learning, were all concerned with single syllables rather than pairs of sequences.

TABLE II

THE INFLUENCE OF S–R COMPATIBILITY AND ASSOCIATIVE VALUE ON SERIAL LEARNING

(Entries are mean correct *R*s per *S* per list)*

| Associative value (%) | Compatibility | |
|---|---|---|
| | High | Low |
| 40 | 98.50 | 92.25 |
| 50 | 75.75 | 56.50 |
| 60 | 91.75 | 83.25 |
| 70 | 116.75 | 96.50 |
| 80 | 109.25 | 89.75 |
| Mean | 98.40 | 83.65 |

* Maximum possible = 160.

Serial-position curves were plotted for compatible and incompatible lists at each of the five levels of meaningfulness. They did not differ significantly in shape.

## DISCUSSION

These results show that clear effects of S–R compatibility can be produced in verbal learning by use of a simple guessing model based on the digram-structure of printed English. It seems highly unlikely, however, that this will ultimately prove to be the most adequate measure of compatibility. An improved measure might take into account sequences of more than two letters and use more information about letter-structure than simple rank-frequency. An analysis in terms of phoneme- rather than letter-sequences might also prove fruitful. The guessing model, however, does have the advantage of simplicity, and, as a first approximation, is relatively successful.

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[4] A. R. Jonckheere, A distribution-free K-sample test against ordered alternatives, *Biometrika*, 41, 1954, 133-145.

The existence of effects of compatibility in verbal learning is of both practical and theoretical interest. Studies of verbal learning give results showing considerable apparently non-systematic variability.[5] It seems likely that some of this 'noise' has in the past been due to the failure to control S–R compatibility.

The existence of effects of S–R compatibility clearly has implications for a theory of verbal learning. It suggests that the effects of familiarity and frequency which have been shown to be important within-syllable factors,[6] are also important between successive syllables. Underwood and Schulz have distinguished two processes in verbal learning, a response-learning phase and an associative or hook-up phase.[7] They suggest that frequency is important for response-learning, but that meaningfulness is crucial for the associative phase. Their evidence for this latter position is rather sketchy, being based on introspective reports from a single experiment, a study which differs from most of the experiments they cite in that the stimuli are CCC trigrams, and the responses common three-letter words. While it seems likely that the *Ss* learn lists involving *real words* in terms of their meanings,[8] the results of the present experiments suggest that Underwood and Schulz are wrong in assuming that the associative stage of *nonsense-syllable* learning is similarly dependent on meaningful associations rather than effects of frequency. These results show first, that frequency does influence the associative phase, and secondly that conscious meaningful associations appear to play no part in the learning behavior of most *Ss*, and are of relatively small importance for the few *Ss* who do use them. As such, the results give very little support for the model of verbal learning proposed by Miller, Galanter, and Pribram,[9] which suggests that such learning involves the linking together of meaningful associations to form a coherent sequence. Such a strategy may well be the best approach for a highly intelligent *S* with no time pressure, but it does not appear to be a feasible strategy for *S*s of average intelligence working at a fairly rapid, paced rate. It is interesting to note, however, that the few associations that do occur are characteristically different in the two experiments. All those found in paired-associate learning involve both elements of the pair concerned, suggesting that *S* learns the pair as a single disyllabic 'word,' whereas the associations produced in serial learning involve single items, suggesting that *S* treats a serial list as a sequence of separate syllables.

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[5] A. D. Baddeley, Some psychological aspects of the coding of information, Unpublished Doctoral dissertation, Cambridge University, 1963.

[6] B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning*, 1960, 271-306;

[7] Underwood and Schulz, *op. cit.*, 271-306.

[8] William Epstein, Irvin Rock, and C. B. Zuckerman, Meaning and familiarity in associative learning, *Psychol. Monogr.* 74, 1960, (No. 491), 1-22.

[9] G. A. Miller, Eugene Galanter, and K. H. Pribram, *Plans and the Structure of Behavior*. 1960, 126-130.

These results have shown that effects of S–R compatibility occur in verbal learning, just as they do in motor-skills. It might perhaps prove fruitful to treat nonsense-syllable learning as a motor-skill involving transfer from *S*'s vastly overlearned verbal habits. Compatible material, being structurally more similar to the language on which *S*'s verbal habits are based allows more positive transfer and thus leads to faster learning.

## SUMMARY

It was predicted that pairs of nonsense-syllables which fit together to give a letter-combination occurring frequently in English (compatible pairs) would be learned more rapidly than pairs which combine to give a less probable letter-sequence (incompatible pairs). In Experiment I, eight pairs of syllables were so selected that in one order they formd a compatible pair (*e.g.* QEM POG), but when reversed became relatively incompatible (POG QEM, MP being more frequent in English than GQ). Experiment II was concerned with serial learning; it used ten 8-item lists, each of which formed a compatible sequence of syllables in one order, but a relatively incompatible sequence when reversed. In both experiments, the compatible material was learned significantly more rapidly than the incompatible. Meaningful associations did not appear to be an important factor in either study. It is suggested that nonsense-syllable learning may be regarded as the acquisition of a motor-skill in which *S* transfers from his established language-habits to a new sequence of verbal responses. A compatible sequence, being similar to the language in sequential structure allows more positive transfer and is thus learned more readily.

# SIZE-CONSTANCY AND CRITICAL FLICKER-FREQUENCY

By IRWIN M. SPIGEL, Temple University

Investigations of size-constancy have almost invariably employed direct judgments or estimates by *O*s of size or distance as dependent variables.[1] It was the purpose of the current experiment to determine whether size-constancy was demonstrable in a situation which required no such subjective estimation *per se*. The threshold of fusion, already shown to be a function of the visual angle subtended by the intermittent luminance,[2] was employed. The primary question was whether the critical flicker-frequency (*CFF*) was also subject to the influence of apparent size under conditions productive of constancy. More specifically, would a size-constancy effect offset an expected decrement in the *CFF* which would normally follow a reduction of the retinal size of the stimulus?

## METHOD

*Apparatus*. The flickering luminance was produced by the electronic switching-unit manufactured by the Lafayette Instrument Company for its Model 1202A Fusion Apparatus. The square-wave output, variable from 0-100 v. DC at 65 mills, was fed to an NE 40 lamp. The viewing assembly consisted simply of an aluminum-mount, 5 in. in length. An upright front-panel 4⅜ in. by 5 in. contained a circular aperture of 2 in. The lamp was mounted to the rear panel. The front-panel was so flanged as to support a 4 x 5-in. sheet of ground glass and interchangeable aluminum-masks of the same dimensions. Two such masks, painted black, were employed. One contained a circular aperture 1 in. in diameter, and the other an aperture ½ in. in diameter. The entire assembly could be moved to various distances from *O* along a black-top table.

*Observers*. Eighteen women students in a general nursing program volunteered as *O*s. They were naïve as to the purpose of the experiment.

*Procedure*. The *O*s were assigned randomly to one of six groups, representing order of presentation of the flickering stimulus under the three conditions to be experienced by all the *O*s. In Condition A, the mask containing the 1-in. diameter aperture was employed at a distance of 40 in. from *O*, allowing the flickering stimulus to subtend a visual angle of 1°26′. In Condition B, the 1-in. diameter mask was also employed, but the viewing assembly positioned at a distance of 80 in. from *O*, for an angular subtense of 43′ at the eye. In Condition C, the mask

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* Received for publication May 17, 1962.

[1] See William Epstein, John Park, and Albert Casey, The current status of the size-distance hypotheses, *Psychol. Bull.*, 58, 1961, 491-514.

[2] Ragnar Granit and P. Harper, Comparative studies on the peripheral and central retina: II. Synaptic reactions in the eye, *Amer. J. Physiol.*, 95, 1930, 211-228.

containing the ½-in. aperture was used with the viewing assembly at 40 in. distance from *O*, so that the angular subtense of the light-source at the eye was the same as in Condition B, *i.e.* 43′. The stimulus was viewed through a slot cut in a screen at *O*'s position. The luminance of the flickering source was kept constant at 9.05 mL. in each condition by adjustment of a pre-calibrated intensity-control on the electronic switching-unit. The experiment was conducted in dim ambient illumination. The *O*s were presented with all three conditions during their single appearance at the laboratory. Thus, the six possible combinations of the order of the three conditions were represented by the measurements of three *O*s in each. The changes in the size and position of the masks were made in full view of the *O*s. Six determinations of *CFF* were made, three ascending and three descending, but alternately presented. Only the last four determinations were employed in the computation of a mean *CFF* for each *O* in each condition. *E* sat behind a screen which also kept the control-panel from view.

## RESULTS

The highest mean threshold of fusion of 31.3 cps. was obtained for determinations made with the 1-in.-diameter stimulus at 40 in. The next highest mean *CFF* of 30.4 cps. was obtained with the stimulus of the same size at 80 in. The lowest mean *CFF* of 27.6 cps. was computed for determinations with the ½-in. stimulus at 40 in. Mean thresholds obtained for all the *O*s under the separate conditions were pooled for a simple analysis of variance, yielding an *F*-ratio of 23.6 ($df = 2/51$, $p < 0.001$). The difference between *CFF*s with the 1-in. and the ½-in. stimuli at 40 in. was statistically significant, as was the difference between thresholds with the 1-in. stimulus at 80 in. and the ½-in. diameter at 40 in. The difference between *CFF*s with the two 1-in. stimuli just failed to reach statistical significance.

## DISCUSSION AND CONCLUSIONS

The data support the influence of a size-constancy effect on the *CFF* under the imposed experimental conditions. With brightness constant, the thresholds of fusion obtained with the 1-in. stimulus at 80 in. were only slightly lower than those obtained with the stimulus of the same size at 40 in. If retinal size were the predominant determinant of the *CFF*, the former thresholds should have approximated those obtained with the ½-in. diameter stimulus at 40 in., subtending the same visual angle. The difference between them, however, was highly significant. In short, a factor of constancy did appear to offset an expected reduction of the *CFF* with reduced retinal size.

While no attempt was made to evaluate cues or mechanisms underlying constancy in the current experiment, the data are consistent with Ohwaki's determinations that with identical visual angles, nearer objects yield smaller retinal sizes.[3] Such a reduction might account for the reduced threshold of fusion in the condition using the ½-in diameter at a distance of 40 in. as compared with the *CFF*s obtained with the 1-in. stimulus at 80 in., subtending the same visual angle. Ohwaki attributes the discrepant retinal sizes in part to the dependency of accommodation on perceived rather than physical distance.

The purpose of the experiment to explore a more direct manifestation of constancy on a sensory threshold was achieved. As such, the findings seem to bear on the question raised by King and Gruber as to the changes undergone by sensory inputs when mapped in psychological spaces.[4] Even though apparent size was not the direct object of experimental inquiry in the current study, the psychological mapping of this dimension was shown to influence a more fundamental perceptual process. It would seem that perceptual theory, in dealing with the constancies, must be able to provide for such ancillary phenomena, or at least offer no theoretical restriction to their occurrence.

## SUMMARY

An experiment was performed to examine the influence of size-constancy in a situation which did not require estimates of size or distance by *O*s. The specific question was whether a size-constancy effect would offset an expected decrement in the critical flicker-frequency which would normally follow a reduction of the retinal size of the stimulus. *CFF*s were obtained from 18 *O*s under three size-distance conditions: a 1-in.-diameter stimulus at 40 in., a 1-in. stimulus at 80 in., and a ½-in. diameter at 40 in. The stimuli under the two latter arrangements subtended the same visual angle. Brightness was held constant. The six possible orders of presentation of the three conditions were represented by the thresholds of fusion for three *O*s in each. The results reflected the effects of constancy, with the *CFF*s obtained with the 1-in. light-source at 80 in. significantly closer to those obtained with the 1-in. stimulus at 40 in. rather than to the thresholds obtained under the ½-in. diameter at 80 in. which subtended the same visual angle. In short, the expected decrement in the *CFF* with reduced retinal size was considerably offset by what appeared to be the influence of size-constancy in the experimental situation.

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[3] Sonoko Ohwaki, On the factors determining accommodation: Research on size constancy phenomena, *Tohoku psychol. Folia*, 14, 1955, 147-158.

[4] W. L. King and H. E. Gruber, Moon illusion and Emmert's law, *Science*, 135, 1962, 1125-1126.

# VERBAL CONDITIONING AND AWARENESS IN A SENTENCE-CONSTRUCTION TASK

By RICHARD I. LANYON, State University of Iowa

In studies of verbal conditioning, there has recently been considerable discussion of the importance of *S*'s awareness, *i.e.* whether *S* can verbalize why he has been reinforced. Much of this discussion has been concerned with the question of whether the *S*s show any conditioning if they do not know why they are being reinforced, *i.e.* whether 'unaware' condition. In many of the relevant studies, one or more factors were inadequately controlled, so that it is difficult to interpret the results. The purpose of this study was to gain more adequate control over these factors, and it was expected that, under these more closely controlled conditions, the *S*s who could not verbalize the reason for reinforcement would show no increment in the reinforced response.

Five specific factors were considered. First, many of the questioning procedures which were aimed at discovering whether the *S*s were aware have been inadequate. Thus, studies by Hills and others have shown that the *S*s were more likely to admit awareness on a written questionary than if questioned orally.[1] Secondly, some *E*s have questioned *S*s about their awareness after running extinction-trials, which might have confused them about the reason for reinforcement. Third, some studies have failed to include a comparison-group of *S*s who have not been reinforced. That such a comparison-group is necessary was shown by Daily, who found a highly significant increment in the frequency of 'I' and 'we' pronouns *without* reinforcement.[2] Fourth is the problem known as 'correlated hypotheses,' *i.e.* that *S* might learn to respond to some cue correlated with the reinforced stimulus rather than to the stimulus itself. It was believed that the chance of the occurrence of 'correlated hypotheses' would be less if a sentence-completion task was used rather than a free-responding situation. The fifth difficulty has to do with the emotional effect of the stimulus on the *S*s. It has been suggested that emotional reactions to the stimuli have been largely

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[*] Received for publication November 18, 1962. My thanks are due Professor Leonard Goodstein for his assistance on this study.

[1] Barbara Hills, A study of acquisition, extinction, and 'awareness' in verbal conditioning, Unpublished Master's thesis, State University of Iowa, 1959, 39.

[2] J. M. Daily, Verbal conditioning without awareness, Unpublished Doctoral dissertation, State University of Iowa, 1953, 38.

responsible for inconsistencies in some of the previous findings, hence it is considered important to find a verbal conditioning task which is 'neutral' but still meaningful to the *Ss*.[3] Recent papers by Spielberger and Dulany, which deal at length with the problem of awareness, have adhered to Taffel's original task, where 'I' and 'we' pronouns are selectively reinforced, in spite of the possible emotional bias thus introduced.[4]

In summary, this study attempts to overcome previous difficulties in studies of verbal conditioning investigating awareness by using more adequate procedures for questioning the *S*s about their awareness of the *E*'s intention, adequate control groups, and a well-structured emotionally neutral task. It was expected that under these conditions the *S*s who were not aware of the contingency of the reinforcement upon a particular subclass of responses would show no increase in the reinforced subclass of responses.

## METHOD

The task selected for this study involved the use of personal names. Lists were made of 25 names of men beginning with the letter L, 25 names of men beginning with M, 25 names of women beginning with L, and 25 names of women beginning with M. A hundred stimulus-cards were prepared with a verb in the past tense in the middle, and four of these names across the bottom. Each card contained a different set of four names: an L-male name, an M-male name, an L-female name, and an M-female name, in counterbalanced order. The *S*s were 85 women, college-students. Each *S* was shown the cards one at a time and told to make up a sentence beginning with any one of the four names, and including the verb. After the first 20 trials, the *S*s were divided into five groups—four to be reinforced, and the fifth a control group. If, during the first 20 trials, *S* used more than 14 or fewer than 6 names in the particular subclass to be reinforced (for the groups to be reinforced) or in any of the subclasses (for control *S*s), the experiment was terminated. Seven additional *S*s were eliminated on this basis. During Trials 21—100, *E* said 'Good' to the four reinforced groups whenever they chose L-names (for Task I), M-names (for Task II), male names (for Task III), and female names (for Task IV). The control group received no reinforcement. All the *S*s were then given a written questionary, asking whether they had noticed anything in particular the *E* had done during the experiment, whether they thought it had affected their performance, what they thought the experiment was about, which names they had used more frequently, and any reason for doing so.

## RESULTS

*(1)* Twenty-five per cent of all reinforced *S*s were classified as 'aware,' and the proportion of 'aware' *S*s was found not to differ among the four

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[3] F. H. Kanfer and J. F. McBrearty, Verbal conditioning: Discrimination and awareness, *J. Psychol.*, 52, 1961, 115-124; Daily, *op. cit.*, 39.

[4] See C. W. Eriksen (Ed.), *Behavior and Awareness: A Symposium of Research and Interpretation*, 1962, 73-101, 102-129.

reinforced groups (Chi-square = 1.7; 3 *df*). There were no significant differences in performance among the four tasks ($F = 1.56$). Therefore, the experimental *S*s are considered as a single group, and divided on the basis of awareness.

*(2)* An analysis of variance comparing the performance of the 'unaware' *S*s, the 'aware' *S*s, and the control group, showed that the proportion of correct responses differed among these groups (F = 21.8; 2/81 *df*, $p < 0.001$). Further comparisons showed that the 'unaware' *S*s did not differ from the control group ($t = 0.94$), and that the experimental *S*s as a whole gave more correct responses than the control group ($t = 2.5$; 82 *df*, $p < 0.02$).

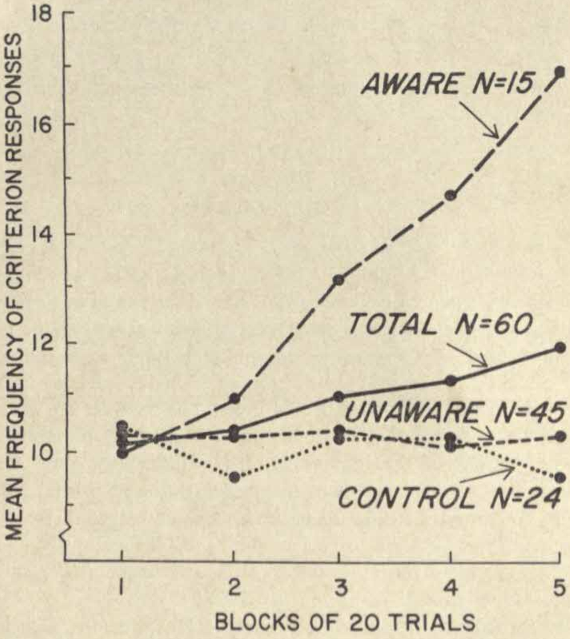

FIG. 1. MEAN FREQUENCY OF CRITERION-RESPONSES

*(3)* The protocols of each 'unaware' *S* were examined for evidence of learning, using the criterion of a significant (5% level) increase in reinforced responses between the first 20 and last 20 trials. Such an increment was shown by 2 of the 45 'unaware' *S*s, approximately what would be expected by chance. Thus, it would appear that the 'unaware' *S*s did not learn. On the other hand, examination of the protocols of each 'aware' *S* for evidence of learning disclosed a significant increment for 13 of the total of 15 'aware' *S*s.

## DISCUSSION

The results show that the increment in correct responses shown by the experimental group as a whole was due entirely to those *S*s classified as 'aware.' The expectation that 'unaware' *S*s would show no conditioning is thus confirmed.

The fact that the percentage of aware *S*s on this task is considerably less than the percentages reported recently in similar experiments using the pronoun-task (reinforcing 'I' and 'we') was to be expected if it is assumed that a more abstract concept like 'L-ness' or 'male-ness' is more difficult to learn than 'I' or 'we.' Related to this is the question of whether aware *S*s are more intelligent as a group than unaware *S*s. If this is true, it may be necessary to use two control groups, one each for the aware and unaware *S*s, matched for intelligence, to prevent this variable from introducing a bias into the comparisons.

The status of the concept of 'awareness' and its relationship to learning and performance continues to present a problem. Kimble suggests that it is really the awareness of a concept which is learned, implying that what happens after this point in the experiment is largely irrelevant, since the *S*, having learned the concept, applies it only if his motivation for doing so is high enough.[5] Dulany has also distinguished between what the *S* thinks he is supposed to do (behavioral hypothesis) and what he actually instructs himself to do (behavioral intention).[6] This problem might be reduced and a major source of variance removed from future experiments by increasing *S*'s motivation sufficiently so that he will in fact do what he considers to be required of him. In the limiting case, *S*'s verbal behaviors (behavioral hypothesis and behavioral intention) would then correlate perfectly with each other and with his actual performance, and all might be regarded as measures of learning.

## SUMMARY

This study attempted to overcome previous difficulties encountered in the investigation of 'awareness' in verbal conditioning by using a sentence-completion task involving neutral stimuli (personal names, both male and female, beginning with either 'L' or 'M'), adequate control groups, and careful questioning about awareness. As predicted, learning without awareness did not occur.

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[5] G. A. Kimble, Classical conditioning and the problem of awareness, in C. W. Eriksen (Ed.), *op. cit.*, 27-45.

[6] D. E. Dulany, Jr., The place of hypotheses and intentions: An analysis of verbal control in verbal conditioning, in C. W. Eriksen (Ed.), *op. cit.*, 102-129.

# MONOCULAR AND BINOCULAR COMPARISON OF APPARENT SIZE

By Robert Fried, Rutgers University

As reported in previous studies,[1] there is a notable shift in apparent size between alternate monocular and binocular perception of objects. In addition, qualitative differences in brightness, chroma, and surface appearance have been reported in uniocular and binocular observation. There are, however, no data available on the quantitative aspects of this difference.

Binocular perception has been traditionally assumed to result from the simple superimposition of the monocular images, within the context of corresponding points.[2] This contention is supported by the fact, among others, that pressure on an eye, such as may be simply produced by pressing on the lower eye-lid, results in a double image. Those differences observed between monocular and binocular perception of objects were attributed to either aniseikonia or to artifacts attending ciliary muscle-involvement when alternately opening and closing an eye.

The purpose of this study is to demonstrate that there is a quantifiable difference between monocular and binocular perception of apparent length and that this difference is not artifactual to the experimental conditions under which it is obtained.

## METHOD AND PROCEDURE

*(1) Apparatus.* The apparatus, shown schematically in Fig. 1, permitted the presentation of a binocular stimulus and identical stimuli to the right or left eye. It was enclosed in a lightproof plywood case. The binocular stimulus-carriage, $S_1$ was adjusted to a distance 39.00 cm. from the front of the reference-point (*RP*) on the front of the apparatus-housing. The monocular stimulus-carriages, $S_2$ and $S_3$, were so adjusted that the midpoint in their range of displacement (5 cm.) also corresponds to 39.00 cm. from the *RP*, but through the beam splitting cubes ($C_1$ and $C_2$). The change in distance of the monocular comparison-stimuli from the point nearest the *S* to the point furthest from him effected a corresponding change in retinal

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* Accepted for publication January 16, 1964. This study was conducted at the Psychopharmacology Laboratory, Rutgers University. Special thanks are due Dr. Morgan Upton.

[1] M. J. Zigler and Kathryn Ward, Qualitative differences between binocular and uniocular impressions, this Journal, 40, 1928, 468; Ernest Thelin and E. R. Altman, Identification of monocular functions, *J. exp. Psychol.*, 12, 1929, 87.

[2] E. G. Boring, *Sensation and Perception in the History of Experimental Psychology*, 1942, 226.

image of 0.14 mm. On the basis of pre-established data, this was normally discriminable.[3]

The stimulus-object was an illuminated line 2 cm. long and 3 mm. wide. The intensity of the light was kept relatively low to avoid irradiational effects.

The size of the stimulus-objects, which were compared monocularly, was adjusted by means of a knob protruding from the front of the apparatus. A motor-driven mechanical program presented the sequence of standard- and comparison-stimuli automatically by alternately turning the appropriate lights on and off.

A headrest was mounted over apertures on the front of the apparatus. A rod, permanently attached to the stimulus-carriages and traveling along a metric scale, indicated change in distance of the comparison-stimuli.

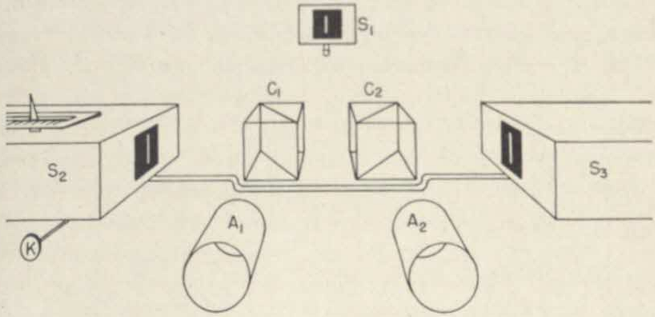

FIG. 1. SCHEMATIC DESIGN OF THE APPARATUS
*O* looks through apertures ($A_1$ and $A_2$), perceives monocular stimulus $S_2$ or $S_3$ or binocular stimulus $S_1$ through optical cubes $C_1$ and $C_2$.

*(2) Observers.* Eight naïve *O*s, whose vision was normal without correction, were used in this study.

*(3) Procedure.* The experimental sessions were carried out in a darkened room with the only source of illumination a red light dimmed to the point where it was just sufficient to read the displacement-scale. The *O*s served one at a time. After being dark-adapted for 20 min., *O* was seated before the apparatus and told so to adjust the stimulus-object that it appeared to be the same size as it went on and off. He was also instructed not to close his eyes, but to step back from the apparatus should his eyes become fatigued.

The method of average error was employed. Trials were balanced for position and movement. Twenty trials were obtained for each position and movement-condition from each *O*.

## RESULTS

The analysis of the data is based on the contention that under conditions of restricted environmental cues, visual size-constancy disappears.[4] In the present study, apparent size may be assumed to be a simple function of retinal size. The judgments of equality of comparison of monocular and standard binocular stimuli were obtained in terms of distance-comparisons.

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[3] W. H. Howell, *A Textbook of Physiology,* 1913, 338.

[4] R. M. Woodworth and Harold Schlosberg, *Experimental Psychology,* 1956, 480.

Upper and lower thresholds were calculated for each eye, and the data were combined to obtain the extent of the disparity between the binocular and monocular stimuli. These data are summarized in Table I.

The upper threshold for the right eye was found to be at 38.139 cm.; and the lower threshold at 38.020 cm. The point of subjective equality (*PSE*) was at 38.080 cm. The difference between the *PSE* and the actual distance of the standard stimulus was 0.920 cm. The *O*s so adjusted the comparison-stimulus to the standard stimulus that the two appeared equal when there was a difference of 0.920 cm. The upper threshold for the left eye was found to be at 38.373 cm. and the lower threshold at 38.071 cm. The *PSE* was at 38.224 cm. The difference between the distance of the standard and the left monocular stimulus was 0.776 cm.

TABLE I

MONOCULAR THRESHOLDS

| Condition | $M$ | $SE_m$ |
|---|---|---|
| Right eye: | | |
| up | 38.020 | .068 |
| down | 38.139 | .072 |
| up and down | 38.080 | .047 |
| Left eye: | | |
| up | 38.071 | .076 |
| down | 38.373 | .081 |
| up and down | 38.224 | .056 |

TABLE II

MONOCULAR RETINAL IMAGES

| Stimulus | Retinal size | Visual angle |
|---|---|---|
| Binocular | 0.795 mm. | 2° 56′ |
| Monocular—right | 0.814 mm. | 3° 00′ |
| Monocular—left | 0.812 mm. | 2° 59′ |

The size-judgments, obtained in distance-units, were converted to retinal size. For an examination of the dioptrics, see Table II.

The data were subjected to analyses of variance and a significant *F* for interaction indicates a difference between the judgments of the right and left eye. This significant difference may be attributed to hemilateral dominance or *eyedness* rather than to procedural effects or to apparatus-artifact.

*Discussion.* The quantitative disparity in the comparison of monocular stimuli to a binocular stimulus may be interpreted to support the contention that binocular perception of linear extents is by no means a function of simple superimposition of the uniocular percepts.

The data indicate that for a monocular and a binocular stimulus to be perceived as equal in length, the monocular stimulus must be made larger than the binocular. This shows that the monocular percept is phenomenologically smaller than the binocular percept.

It is interesting to note that the *PSE*s for the right and left eye are somewhat different. The judgments of the left eye, while more nearly like the binocular percept, are considerably more variable than those in the right eye. One might be tempted to suspect that the percepts of the dominant eye ought to be most like the binocular percepts. Data in the present study contradict this conclusion.

While it was demonstrated that simple superimposition of monocular percepts is not applicable to the explanation of the unity of binocular perception, one cannot derive any simple function to demonstrate the relationship. The manner by which a unitary percept is obtained is certainly not additive.

The only plausible explanation of the phenomenal difference between monocular and binocular percepts resides in a somewhat gross analysis of the relationship of the retinal area. Under most conditions, when environmental cues are negligible, apparent size must be a function of the ratio of the retinal area stimulated to over-all retinal area. Under most conditions, binocular vision is partly monocular. Retinal fields never completely overlap. When only one eye is stimulated, the ratio between stimulated and total retinal area is somewhat increased, thus the object, now occupying a smaller area of the total field, appears smaller.

## SUMMARY

Eight *O*s compared the apparent length of a line-segment presented binocularly to an identical stimulus presented monocularly. A technique was developed whereby the nature of the comparison was not revealed to the *O*s.

The method of average error was employed and thresholds were obtained for the right and left eye.

Results indicate that a monocular stimulus must be made larger than a binocular stimulus at the point where the two are judged to be equal.

Unilateral effects were also found and, in conjunction with the thresholds, indicate that the binocular size is not a function of the simple combination of the monocular percepts; nor is it an overlap *per se*.

Statistical significance, at the 5% level, indicates that monocular apparent size is different from binocular apparent size.

# THE JUDGMENT OF LINEAR PROPORTIONS

By HARVEY NASH, Illinois State Psychiatric Institute

Anchoring effects in judgment have been reported by various investigators. This paper, which reports biases in the judgment of linear proportions, provides a further example of such anchoring effects.

*Method.* Strips of paper, 1 × 11 in. in size, were presented to 32 *S*s (young adult volunteers—16 men and 16 women). A line 8.33 in. long, punctuated by 0.08-in. cross-bars at the ends, was placed in the center of each strip (see Fig. 1). The strips were held on a horiontal working surface, their long dimension in a sagittal plane. The *S*s were asked to assume that the line represented percentages ranging from 0%, at the 'bottom' (the cross-bar nearest *S*), to 100%, at the 'top' (the more distant cross-bar).

*E* announced nine deciles, from 10 to 90%. Deciles were presented in five trials, in a different random order on each trial. The *S*s were asked to estimate the position of each decile by marking an 'x' on the line (Fig. 1). They were encouraged to respond rapidly, without deliberation, and were given a fresh strip for every response. They had, therefore, no opportunity to review previous responses. Their responses were scored by translating them into numerical values ranging from zero to 100.

*Results.* The five responses produced by *S* for each decile were averaged. Means for the 32 averaged values of each decile are presented in Table I. They are expressed as discrepancies from the true values; a positive discrepancy indicates that *S*s placed their 'x's' 'above' the correct position; a negative discrepancy, that they placed 'x' below the correct position. Thus, the discrepancy 0.58 for the eighth decile represents a response with a numerical value of 80.58%.

Judgments were significantly distorted for three deciles ($p = 0.005$ at the third decile and $p = 0.001$ at the fourth and seventh deciles). Mean discrepancies between judged and true values, plotted in Fig. 1, approximate the form of a sinusoid having a wave-length slightly shorter than the distance between the first and ninth deciles. The *SD*s for the 32 averaged values of each decile, also plotted in Fig. 1, resemble a rectified sinusoid. A Hartley test reveals a significant variation ($p = 0.005$) among the nine *SD*s.[1]

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* Received for publication June 25, 1963. The author is grateful to Professors Norman H. Anderson, W. J. Brogden, and K. A. Brownlee for their many helpful suggestions.

[1] H. O. Hartley, The maximum F-ratio as a short-cut test for heterogeneity of variance, *Biometrika,* 37, 1950, 308-312.

An analysis of variance indicates that distortions varied significantly neither with trial nor with sex of *S*. The trials × sexes and deciles × trials × sexes interactions were likewise non-significant.

The deciles × trials interaction was significant at the 0.005 level. This perhaps exaggerates the true level of significance since the homogeneity of variance assumption (see the Hartley test-results) was not satisfied. Distortion was approximately sinusoidal in the later trials, as in the earlier

TABLE I

JUDGMENT AS A FUNCTION OF TRIAL AND POSITION

(Discrepancies (in %) from true values; average of 32 *Ss*.)

| Per-centile | Trial | | | | | Total | Mean |
|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | | |
| 90 | −0.03 | −0.63 | −0.22 | −0.44 | 0.72 | −0.60 | −0.12 |
| 80 | 0.81 | 1.44 | 0.91 | −1.53 | 1.28 | 2.91 | 0.58 |
| 70 | 3.06 | 3.44 | 3.06 | 0.41 | 1.56 | 11.53 | 2.31 |
| 60 | 1.72 | 0.69 | 0.34 | −0.38 | −0.13 | 2.24 | 0.45 |
| 50 | 0.47 | 0.34 | −0.50 | −0.50 | 0.84 | 0.65 | 0.13 |
| 40 | −3.09 | −0.53 | −2.63 | −1.94 | −2.47 | −10.66 | −2.13 |
| 30 | −3.22 | −3.16 | −3.66 | −1.16 | −0.69 | −11.89 | −2.38 |
| 20 | −2.16 | −1.66 | −0.88 | 0.31 | −0.16 | −4.55 | −0.91 |
| 10 | −0.03 | −0.13 | 0.34 | 0.00 | 1.34 | 1.52 | 0.30 |
| Total | −2.47 | −0.20 | −3.24 | −5.23 | 2.29 | −8.85 | — |
| Mean | −0.27 | −0.02 | −0.36 | −0.58 | 0.25 | — | −0.20 |

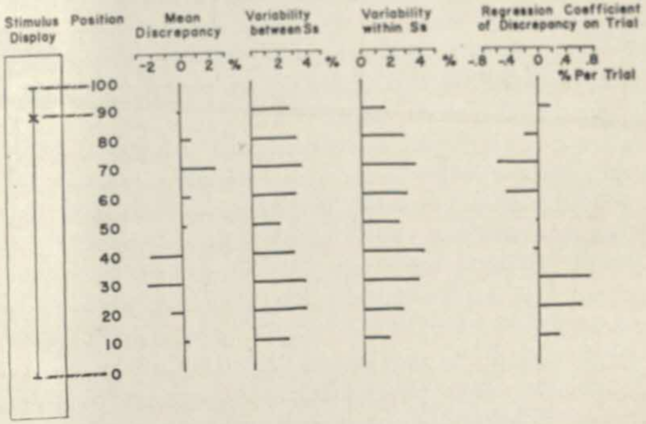

FIG. 1. JUDGMENT OF DECILE POSITIONS

The stimulus-display includes a sample 'x' at the ninetieth percentile. Among-*S* variability is indicated by the *SD* of the averaged values of the 32 *Ss*; within-*S* variability, by the mean of the 32 *SDs* computed from each *S*'s five trials. Regression coefficients ($b_{yx}$) of mean discrepancy on trial are based on the cell entries of Table I.

trials, but accuracy improved steadily with experience, even though the *S*s had no knowledge of results. Accuracy tended to improve least for those deciles (*e.g.* the first, fifth, and ninth) at which the *S*s were initially most accurate—note the somewhat smaller regression, at these deciles, of discrepancy on trial (Fig. 1). That judgments varied least, from trial to trial, for the first, fifth, and ninth deciles (Fig. 1) suggests that, at these deciles, the *S*s early attained and steadily maintained a proficiency that was only gradually achieved at intermediate deciles.

*Discussion.* Distortion varied from one region of the line to the next. Consider the factors whose joint operation accounts for this variation.

*Mid-region.* The accuracy of fifth decile responses is consistent with the fact that intervals on many perceptual continua are readily bisected.[2] Particularly relevant is Levett's finding that *S*s instructed to set an artificial slide rule at nine positions equally spaced between two scale divisions rarely made large errors for mid-position settings.[3] Also pertinent are the results of studies on visual interpolation which indicate that a point midway between two markers is readily perceived as bisecting the interval between the markers.[4]

There was much agreement among the responses at the fifth decile produced by a given *S*, as well as among those produced by different *S*s. Investigating the ability to set a variable line perpendicular to a fixed line, in the absence of visual cues of the horizontal and vertical, Onley and Volkmann noted that judgments were consistent not only when the reference line was horizontal or vertical, but also when it was in midquadrant (45°).[5] *S*'s judgments thus seem to be guided not only by objective landmarks, but also by subjectively determined landmarks (*e.g.* subjective midpoints or mid-axes) that *S* confidently feels he can locate and use for reference purposes.

*End-regions.* Variability was low in the vicinity of the end-anchors, a result consistent with previous findings, for a variety of continua, that anchoring stimuli bolster self-confidence, accelerate response, and reduce variations in response.[6] Near-end-anchor positions also showed little distortion, a finding that accords with Stevens and Galanter's observation that a continuum punctuated by end-anchors is discriminated best in the neighborhood of these landmarks.[7]

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[2] S. S. Stevens, Mathematics, measurement, and psychophysics, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 1-49.

[3] C. M. Levett, Jr., Errors of interpolation in instrument reading and setting, *J. appl. Psychol.*, 36, 1952, 49-52.

[4] Alphonse Chapanis and Michael Leyzorek, Accuracy of visual interpolation between scale markers as a function of the number assigned to the scale interval, *J. exp. Psychol.*, 40, 1950, 655-667; H. K. Miller, Jr., An exploratory study of linear interpolation, *J. appl. Psychol.*, 34, 1950, 367-370; Levett, *op. cit.*, 49-52.

[5] J. W. Onley and John Volkmann, The visual perception of perpendicularity, this JOURNAL, 71, 1958, 504-516.

Responses for a position in the end-regions were distributed asymmetrically, the tail of the distribution trailing away from the nearest end-anchor.[8] First decile responses, for example, ranged from 3 to 21%. Had the first decile been underestimated to the same extent that it was overestimated, the *S*s would have made the too obvious error of placing some responses at or below the zero-anchor. Critical capacity is thus heightened in the vicinity of an end-anchor, which constrains response for nearby positions, and consequently skews distributions. Such skewing probably accounts for the reversal of distortion at the first and ninth deciles.

*Intermediate regions.* Responses were least accurate and least consistent for positions intermediate between midposition and the two end-regions, indicating that discrimination worsens for positions removed from both the subjective mid-anchor and the objective end-anchors. The distortion of intermediate responses away from mid-position, toward the nearest end-anchor, is not attributable to the use of subjective points of reference (*e.g.* quartiles) other than the imagined mid-point. Nor are these distortions (which are unassociated with asymmetrical distributions in the midregion) analogous to the distortions of the end-regions.

The intermediate distortions may be explained by assuming that the mid-anchor, which is firmly secured when *S* bisects the line, drifts as his gaze is diverted from mid-position to the intermediate regions. *S*'s increased uncertainty about the location of the midpoint transforms the subjective midpoint into a subjective midregion that encroaches upon the intermediate regions, compressing them and displacing them toward the end-anchors. Though attractive, this hypothesis requires empirical confirmation.

## SUMMARY

Judging the position of nine deciles along a line representing percentages from zero to 100, 32 *S*s produced errors that varied with position in an approximately sinusoidal manner. Except in the vicinity of the line's end-

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[6] John Volkmann, Scales of judgment and their implications for social psychology, in J. H. Rohrer and Muzafer Sherif (eds.), *Social Psychology at the Crossroads*, 1951, 273-294.

[7] S. S. Stevens and E. H. Galanter, Ratio scales and category scales for a dozen perceptual continua, *J. exp. Psychol.*, 54, 1957, 377-411.

[8] The skewness for those positions included in the present study is insufficient to affect the validity of the assumptions underlying the analysis of variance.

anchors, judgments were distorted away from the mid-position, toward the nearer end-anchor. Responses were least distorted and least variable in the mid- and end-regions. Where distortion was large, it was attenuated by repeated response. The distortions reflect *S*'s reliance on the visible end-anchors as well as on an imaginary mid-point presumably projected upon the line by the *S*s.

# ISOLATION AND ORGANIZATIONAL EFFECTS IN SERIAL LEARNING

By GLORIA J. FISCHER, University of South Florida

When a syllable in the middle of a serial list is 'isolated,' *i.e.* made to contrast with the other syllables, it now seems clear that the isolated syllable is learned *sooner* than it otherwise would have been, but that there is no spread of effect to adjacent items; nor is there a facilitation in learning the list as a whole. Waugh has cited convincing evidence that in serial learning an individual starts at the beginning of a list and learns sequentially his average number of items per trial, until the entire list is learned.[1] Isolation, however, has been found to move up the order in which an item is ordinarily learned in a sequence. Thus, as Jensen points out, the Von Restorff-effect (isolation) could be due to an earlier ordering, and hence learning of the isolated item.[2] The earlier ordering might be explained as a function of the increased discriminability engendered by isolation, resulting in selective attention to the contrasting item.

A very different explanation of the Von Restorff-effect is based on an extension of the anchoring effects of stimuli on judgment to comparable effects on learning. A general anchor-effect ('natural' or 'supplied') on judgment is decreased variability, *i.e.* increased accuracy in the region of the anchor.[3] Wishner, Shipley, and Hurvich view the initial and terminal stimuli in a serial list as natural anchors about which the list is organized or learned.[4] If an isolated stimulus is similarly viewed as a supplied anchor, increased accuracy at and about its locus would be predicted. A serious difficulty with this approach is that the predicted spread of effect about the supplied anchor has not in fact been found. Also, learning as organization about natural end-anchors would lead to a prediction of symmetry, rather than the negative skew found in the curves of serial learning.

Yet the anchor approach generated a research finding consistent with it, and inconsistent with the alternative approach considered previously.[5] *S*s were instructed to learn, as if there were two lists, a list of nonsense-syllables in which

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* Received for publication July 18, 1963. The author's thanks are due Edward Warren and Bruce Bobys for their assistance in this study.

[1] N. C. Waugh, Length of series and the learning curve, this JOURNAL, 75, 1962, 177-192.

[2] A. R. Jensen, the Von Restorff effect with minimal response learning, *J. exp. Psychol.*, 64, 1962, 123-125.

[3] D. M. Johnson, *The Psychology of Thought and Judgment*, 1955, 362 f.

[4] Julius Wishner, T. E. Shipley, and M. S. Hurvich, The serial-position curve as a function of organization, this JOURNAL, 70, 1957, 258-262.

[5] *Ibid.*, 259 f.

the first half of the syllables were in black, capital letters; and the second half in red, lower case letters. These *S*s learned the list in fewer trials than control *S*s who were instructed to learn the same list of syllables, all of which were in black capital letters. The question is why a single isolated stimulus in the middle of a list seems merely to be learned sooner than it otherwise would have been learned, whereas similar isolation of all stimuli just past the middle of the list results also in such reordering, but in addition, facilities learning of the list as a whole. Since both stimulus-situations contain a supplied anchor, yet yield differential effects on learning, the anchor-approach is unsatisfactory.

A possible explanation is that the isolation-effect on learning *is* restricted to such syllables being learned sooner than they otherwise would have been, due to selective attention to them. That the facilitating effect on learning found by Wishner, Shipley, and Hurvich resulted, rather, from instructions to the *S*s to learn the syllables as if there were two lists.[6] The latter might be expected to facilitate learning, since it amounts to a simple coding of the material.[7] The purpose of the present study was to test this hypothesis by replicating conditions in the Wishner, Shipley, and Hurvich study that might be called: Control (learn one list: no isolation) and Experimental 1 (learn two lists: isolation),[8] but adding conditions. Experimental 2 (learn two lists: no isolation) and Experimental 3 (learn one list: isolation). Specific predictions to be tested were: (1) coding engendered by instructions to learn two lists will produce quicker serial learning of the syllable-list than no coding; and (2) isolation will not produce quicker serial learning than no-isolation, but will cause the middle (seventh and eighth) syllables to be learned sooner than without isolation.

*Method.* The method used by Wishner, Shipley, and Hurvich, including instructions to the *S*s, was followed with no known deviations.[9]

*Subjects.* The *S*s were 112 volunteers from elementary classes in psychology who were assigned randomly to four groups of 28 *S*s each.

*Materials.* A list of 14 nonsense-syllables of 33% association-value was constructed from Glaze's list, following the rules suggested by Hilgard.[10] Fourteen different orders (rotations) of the list were constructed.

*Procedure.* Using the serial anticipation-method and spelling the syllables, each *S* was required to learn the list to a criterion of one perfect reproduction. Inter-item and inter-trial intervals were 2 sec. for one-half the *S*s in each group and 4 sec. for the other half of the *S*s in each group. For each half, also, the 14 different orders of the list were assigned to the *S*s at random.

*Control (learn one list: no isolation).* Syllables were presented in black, capital letters, with instructions to learn to spell all the syllables in the 14-syllable list correctly. *Experimental 1 (learn two lists: isolation).* The first seven syllables were presented in black caps and the second seven in red, lower case letters, with instructions to learn the two lists of seven syllables. *Experimental 2 (learn two lists: no isolation)*. Like the control group, but with instructions to learn the two lists of seven syllables, both in black caps. *Experimental 3 (learn one list: isolation)*; Like Experimental 1, but with instructions to learn the 14-syllable list.

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[6] *Ibid.*, 261.

[7] G. A. Miller, The magical number seven, plus or minus two: Some limits on our capacity for processing information, *Psychol. Rev.*, 63, 1956, 81-97.

[8] Wishner, Shipley, and Hurvich, *op. cit.*, 258-262.

[9] *Ibid.*, Personal communication from Dr. Wishner.

[10] E. R. Hilgard, Methods and procedures in the study of learning, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 540 f.

*Results.* The statistical design of the experiment was a $2^3$ factorial with $n = 14$ observations per cell. An analysis of variance was performed on the number of trials to the criterion of one perfect serial reproduction of the list. Results of the analysis indicated that only isolation (I) and exposure-interval (I.T.I.) were statistically significant sources of variation ($F = 68.47$ and $8.13$ respectively, with 1 and 104 *df*). The magnitude and direction of the differences between levels of these main effects are shown in Table I. As can be seen from Table I, the 2-sec. I.T.I. requires approximately twice as many trials to learn the list serially as does the 4-sec. I.T.I. Since, however, the former I.T.I. is half the latter, this means that serial learning took the same amount of total time for the two conditions.

TABLE I

GROUP MEAN NUMBER OF TRIALS TO CRITERION IN SERIAL LEARNING OF THE 14 NONSENSE-SYLLABLE LIST

| Isolation | | Interval | |
|---|---|---|---|
| BLACK | BLACK-red | 2-sec. | 4-sec. |
| 32.45 | 25.93 | 38.64 | 19.73 |

As can also be seen from Table I, the BLACK-red condition of syllable-isolation does facilitate learning; but instructions to learn two lists do not. Both instructions to learn one or two lists (L and L × I) were insignificant sources of variation. Thus, the data flatly contradict the hypotheses tested by the present study. First, the facilitation in serial learning found by Wishner, Shipley, and Hurvich was not due to simple coding by virtue of instructions to learn two lists. Secondly, isolation *per se* did not result merely in the middle items' being learned sooner than they otherwise would have been. Rather, the data support Wishner, Shipley, and Hurvich's conclusion that an organizational factor enters to facilitate serial learning when the material to be learned is experimentally 'anchored.' The nature of this effect can be seen most clearly when the percentage of errors of the group-means is plotted for each syllable-position (see Fig. 1). BLACK-red isolation, indeed, appears to function like an anchor, since errors are clearly reduced at and about its locus. Instructions to learn two lists can be seen to have no additional influence on the serial-position curve; but instructions to learn two lists do have an influence when there is no isolation (BLACK-two lists). It is one like the more usual isolation-effect, viz. the two middle syllables are learned sooner than they are learned by the control group, but the list as a whole is not learned in fewer trials.

*Discussion.* Data from the present study would appear to have clarified the fact that visual division of sequential material facilitates serial learning. Yet other work clearly shows that isolation or visual emphasis of a single item in an otherwise homogeneous field does not facilitate serial learning, but merely results in the emphasized item's being learned sooner than it otherwise would have been. As indicated previously, the latter finding cannot be accounted for by the anchor-framework. In spite of the fact that simple coding was not an effect from instructions to learn material as if there were two lists rather than one, it is tempting to regard the organization that results from visual division of a list as a function of coding—perhaps due to the visual division, *per se.* Even if coding could be demonstrated, however, and the present study has not done so, the class of stimuli producing isolation or visual emphasis-effects, in contrast to the class of stimuli producing organizational effects, remains obscure.

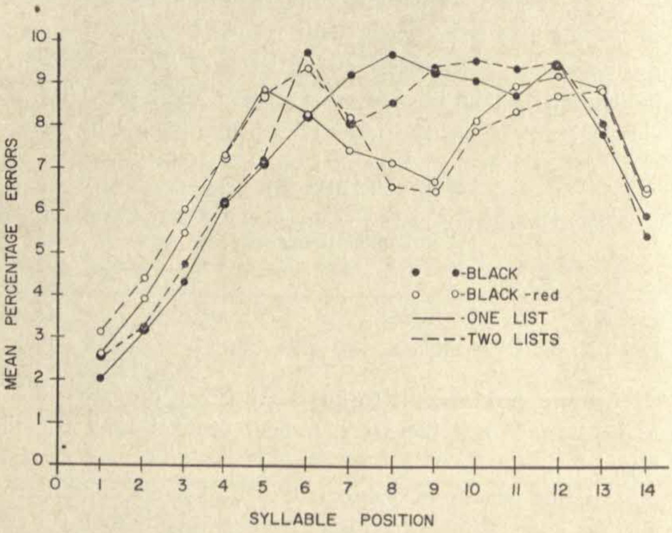

FIG. 1. MEAN PERCENTAGE OF ERRORS AT EACH SYLLABLE-POSITION FOR CONTROL AND EXPERIMENTAL GROUPS

## APPARATUS

### AN APPARATUS FOR THE MEASUREMENT OF TACTILE ACUITY

By Dolly Chan, University of Singapore

The apparatus newly devised for the measurement of tactile acuity, which is described here, uses a technique based upon the familiar "Landolt C-test" of visual acuity. Testing is limited to the pad of the finger tip, though a modification would enable other areas to be tested. The subject (*S*) is presented with a series of small metal rings, some complete (O-rings) and some slotted, (C-rings), the slots being of 4 widths, 1, 2, 3, and 4 mm. The rings are presented to *S* in a random order with the

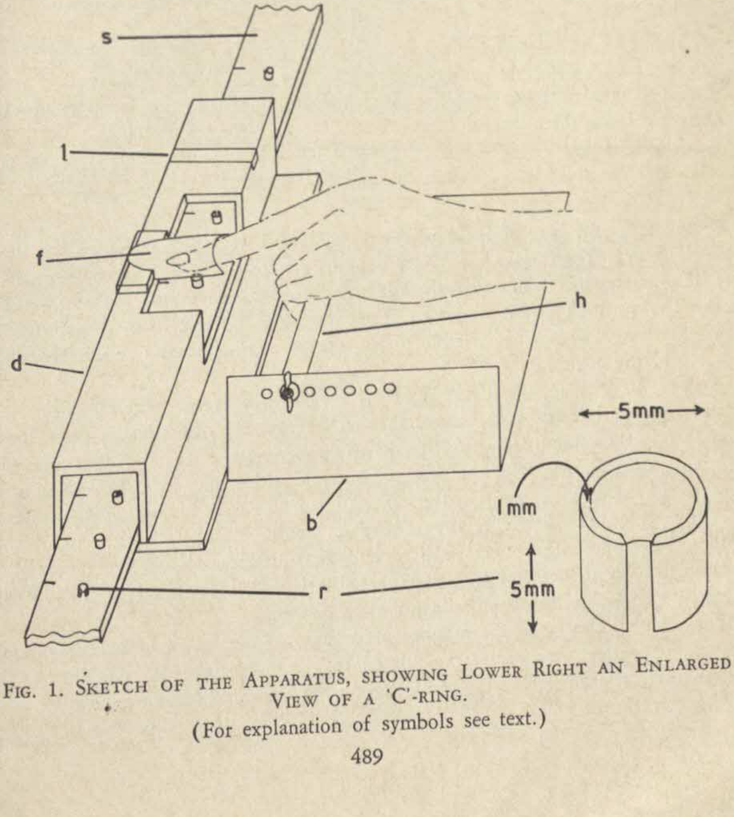

Fig. 1. Sketch of the Apparatus, showing Lower Right an Enlarged View of a 'C'-ring.

(For explanation of symbols see text.)

C-rings so oriented that the slots point 'N,' 'S,' 'E,' or 'W.' The complete test-series consists of 20 rings, 4 of which are O-rings and 4, C-rings of each width of slot. The arrangement of the apparatus effectively masks the test-ring from *S*'s view.

*Apparatus.* The apparatus (Fig. 1) consists of a light metal base plate (b), padded to support the lower arm, and fitted with a padded handgrip (h). At the far end of the plate is a hardwood box open at the center to permit the insertion of *S*'s left index finger. A curved lucite shield (f) is fitted above this opening to guide the finger. In the base of the box runs a slide (s) on which are mounted the 20 test-rings (r). These rings are 5-mm. lengths cut from copper tubing of 5-mm. external diameter and 1-mm. wall-thickness and are fastened to the slide with sealing wax. The position of the slide, *i.e.* the ring being offered to *S*, is indicated by reference-lines and numbers visible to the operator through a lucite window (l) in the top of the box.

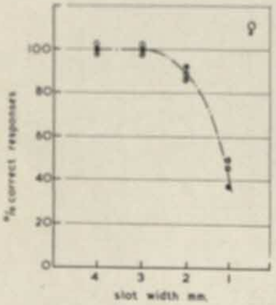

FIG. 2. CURVE OF BEST FIT
(Derived from three experiments.)

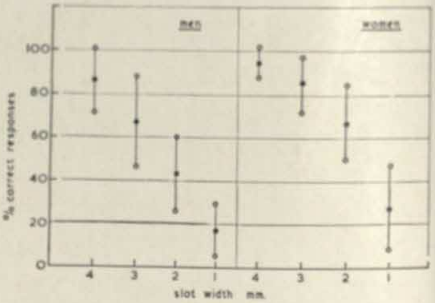

FIG. 3. DATA OBTAINED FROM 19 *S*s (10 MEN AND 9 WOMEN)
Every *S* tested three times. Dots represent mean values; circles, the standard deviations.

*Procedure.* The apparatus is placed on a table of convenient height and *S* is so seated that his left arm rests comfortably on the pad. He graps the handgrip loosely and this is then adjusted to position his finger over the test-ring. The lucite guide offers a position of rest for the finger. The slide is then moved to bring one of the test-rings beneath the finger. On the word "down," *S* moves his index finger to touch the ring. He immediately reports "north," "south," "east," "west," or "blank," which is recorded. The finger is lifted and the slide moved according to a random pattern to each of the rings. This is repeated 5 times, hence *S* makes in all 120 judgments, 24 on each of the 4 slot-widths and 24 on O-rings.

For each width of slot the number of correct judgments is counted and the percentage of correct responses is calculated to give a measure of reliability of discrimination. These percentages are plotted against the width of slot to obtain a curve of the type shown in Fig. 2. (The percentage-value for the O-rings is not plotted.)

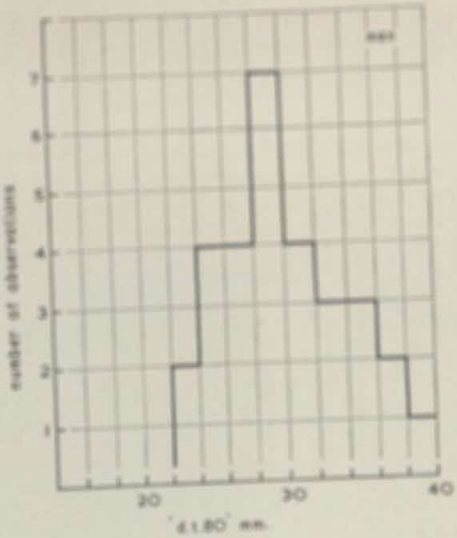

FIG. 4. HISTOGRAM OF VALUES FOR DISCRIMINATIVE THRESHOLD AT 80% RELIABILITY (dt 80)
(Based on 30 experiments on 10 young men.)

Fig. 3 illustrates the data obtained from 10 men and 9 women: every S being tested three times. The solid dots represents the mean values and the open circles the extent of the standard deviations.

For use in the study on environmental effects, a single 'acuity' value is derived for an experiment by sectioning the curve at the 80% correct level to give a "discriminative threshold" (dt 80). A histogram of dt-values obtained in a pilot series is shown in Fig. 4.

# AN APPARATUS FOR MEASURING KINESTHETIC JUDGMENTS

The instrument for measuring kinesthetic judgments described here was patterned somewhat after one constructed by Rodger and McEwen from a woodworker's vise.[1] Its principal advantage is its ease of operation.

Fig. 1 shows the standard stimulus-object at 'A'; the comparator at 'B'; and the foot-pedal that controls the reversible motor which operates the comparator at 'C'. The stimulus-object 'A,' which is grasped between thumb and finger by *O,* is 3 in. deep, 8 in. long, and is capable of being adjusted to any desired width between 1 to 4 in. It may be equipped with internal springs (or not), which govern the tension between the parallel sides. A set of contacts and a battery-bell circuit insure that tension is maintained at a given width as the bell rings when tension is relaxed. The comparator 'B' is 8 in. long and 6 in. wide and the upper parallel sides can be varied between 2–4½ in. apart when activated by foot-pedal 'C' by means of a worm gear at a rate of 0.05 in. per sec. The separation in width, measured by two micrometers, mounted on the comparator, is accurate to within 0.001 in. The foot-pedal, hence the adjustment of the comparator, may be operated by *O* or *E,* as conditions dictate.

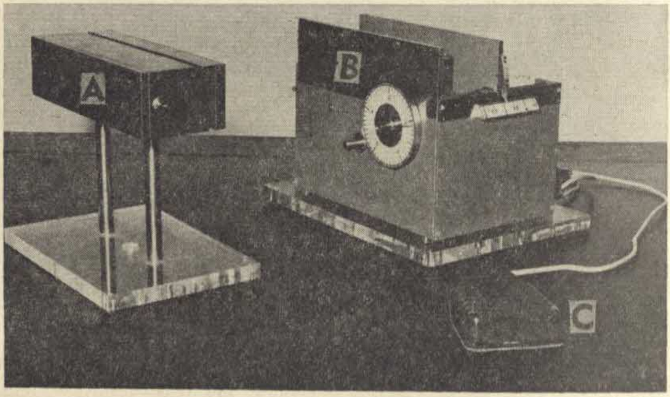

FIG. 1

This apparatus is at present in use for haptical studies of width and the effect of tension and strain upon those judgments, but it may be adapted for studying after-effects or for other parameters affecting accuracy of judgments of width, *e.g.* vibration of 'A'.

University of Alberta, Canada — EDGAR HOWARTH

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[1] R. S. Rogers and Peter McEwen, A new device for the study of kinesthetic after-effects, this JOURNAL, 72, 1959, 290-291.

# APPARATUS NOTES

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## AN INEXPENSIVE DEVICE FOR MEASURING LOCOMOTOR ACTIVITY OF SMALL ANIMALS

The device described here is basically an adaptation of Hill and Stellar's drinkometer-circuit for registering contacts.[1] The circuit consists of a 9-v. battery, a 20-MΩ resistor, and a 6SN7 vacuum tube. Two leads to this circuit are so arranged that an animal will make simultaneous contact with them under circumstances which must operate the externally-powered unit. This contact completes the circuit, biasing the tube-grid which energizes a relay in the circuit whose contacts are connected to the externally-powered unit. Since the 6SN7 tube is of the diode-type, two such circuits can be conveniently constructed around one tube. We have designed and used a single-chassis unit which consists of six such circuits utilizing three tubes and one power supply at a moderate cost.

To measure activity level in rats, each contact-relay was connected to individual cages in the following manner: Three evenly-spaced slats of varnished masonite, 2½ in. wide and ¼ in. thick, were placed across the floor of each cage. Each slat had a 2-in. rectangle painted on its face with silver-based conductive paint.[2] An insulated terminal post was attached to the bottom of each slat and connected to the silver-paint rectangle via a brass screw. These slats were so placed that *S* could stand conveniently on Slats 1 and 2 or 2 and 3, but never on Slats 1 and 3. Slat 2 was connected to one lead into the electronic circuit and Slats 1 and 3 to the other lead. Each time the animal made contact with either pair of slats, the electronic relay activated an externally-powered counter in series with the contacts of the relay.

In one experiment, this apparatus was used to measure the activity of 12 *Ss* (rats) over a 4.5-hr. evening period. The animals had lived in the apparatus for two-weeks and had stabilized their activity-levels. A plot of the average activity-levels for each 30-min. period revealed a 3-hr. cycle similar to the cycle obtained frequently in our laboratory with comparable *Ss* run in activity wheels during the same hours under similar conditions.

This combination of the electronic contact relay with silver-based conductive paint has many other uses. It is considerably less expensive and bulky than photobeams and can readily be installed in runway-experiments to activate timers and stimuli and in checker-board mazes to measure areas explored. Many other uses will suggest themselves to the reader.

University of Michigan
Flint College

ALFRED C. RAPHELSON
BERNARD M. RABIN

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[1] J. H. Hill and Eliot Stellar, An electronic drinkometer, *Science*, 114, 1951, 43. Other electronic contact relays could also be so adapted, *e.g.* T. A. Hunter. An electronic contact relay, this JOURNAL, 65, 1952, 458-459; W. W. Dawson, R. S. Lindner, and Nathan Perry, The electrical quantification of the consumption of liquids by rats, this JOURNAL, 76, 1963, 687-690.

[2] For instructions in using silver-based conductive paints, see Julian Hockberg and M. E. Bitterman, The use of painted grids and circuits in the construction of psychological apparatus, this JOURNAL, 63, 1950, 90-91.

# NOTES AND DISCUSSIONS

## SIZE-CONSTANCY IN A PICTURE

The long straight reach of track runs off into the distance, the two rails are parallel and yet converge.[1] Is there a contradiction here, or is the convergence phenomenal and the parallelism an inference? Helmholtz was blamed because he dared to introduce inference via the *unbewussster Schluss* into the inscrutable dynamics of perception,[2] but nowadays we are fully aware of how many unconscious inferences determine the perceptual whole to make the totality often quite different from its reduced core. Gibson handles the paradoxes associated with the perceived constancy of size with distance variant by asserting that perceived size, being indubitably relative, is experienced or estimated—for you cannot really distinguish between the two mechanisms—in relation to a scale, which itself depends on the perceived distance at which it is applied.[3] The two tracks converge in the distance because the scale by which they are judged contracts with increasing distance. If anything remains constant, it is the total width of the visual field, which includes more and more material as it reaches farther and farther away—as so many of Gibson's illustrations show. If one thinks only of the 90° in the center of the total 180°-binocular field of vision, then the width of the field is twice as great as its distance—20 feet at 10 feet away, 2 miles at a mile away. Thus the standard of comparison, Gibson's 'scale,' shrinks in proportion to its distance away. If the 'scale,' while shrinking, nevertheless maintains its identity at different distances, then size-constancy holds; but if the 'scale' shrinks phenomenally in comparison with the total field, size-constancy will be diminished, although perhaps it will not be wholly abolished as it is by complete reduction.

It is said that size-constancy cannot be shown in photographs and pen-and-ink drawings, that such perceptions lack depth—in the first place, because they are not stereoscopically binocular, and, in the second place, because it is impossible wholly to eliminate the perception of the paper's surface which diminishes the perceived depth. Fig. 1 shows how completely size-constancy may fail in a photograph, for here the man's feet in linear measure are three times the size of his head. This picture is one-half of a stereogram that was furnished by Underwood & Underwood fifty or sixty years ago and that was at one time included by C. H. Stoelting Company in their set of 37 Titchener stereograms, although Titchener does not describe this one.[4] The whole of the stereogram viewed in a stereoscope gives perfect size-constancy or may, with some adjustment, make the man seem a little too tall and thus his feet a little too small. It is a striking example of how retinal disparity alone can give the perceived distance needed to meet the requirement for seeing size-constancy with distance variant.

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[1] E. G. Boring, Visual perception as invariance, *Psychol. Rev.*, 59, 1952, 141-148.

[2] Boring, *A History of Experimental Psychology*, 2nd ed., 1950, 308-311, 315.

[3] J. J. Gibson, *The Perception of the Visual World*, 1950, 180-183.

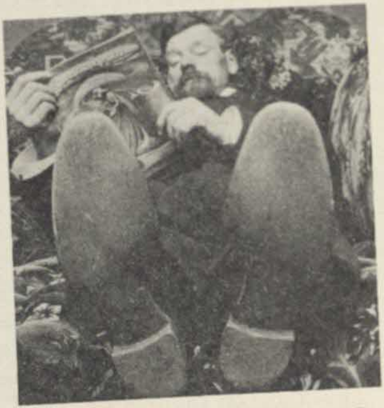

FIG. 1. FAILURE OF SIZE-CONSTANCY IN A PICTURE

Size-constancy fails in an ordinary photograph when the perception of the surface of the picture kills the perception of depth, but viewed through a reading-glass this picture gains considerable constancy of size. This photograph is half of a stereogram, which, when observed in a stereoscope, shows complete size-constancy, since retinal disparity then dominates the perception of the picture's surface. Reproduced by permission of Underwood & Underwood.

Retinal disparity is not, however, the only cue to the perception of distance that favors constancy of size with distance variant. If you look at this stereogram in a stereoscope and close one eye, the picture does not revert to what it was in direct vision without the stereoscope. That is because you are still looking through a lens. Examine Fig. 1 through a large reading glass and you will find that the camera's error is largely corrected, though not entirely, by diminution of the visibility of the surface which supports the picture. This increase in depth and solidarity of a picture when it is viewed through a lens, monocularly or binocularly, had long been known and has been discussed fully by Schlosberg.[5] The phenomenon is due to the fact that the introduction of the lens destroys in part the surface effect of the paper, furnished an instrument for viewing pictures in depth, which they called a Verant stereoscope, but it was only a large convex lens, vertically mounted, with a place beyond to put a picture. It worked well but no better than a reading glass, and Schlosberg says that Zeiss later withdrew it.

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[4] E. B. Titchener, *Experimental Psychology,* I, ii, 1901, 273-289.

You can get perfect size-constancy and also perfect reduction on television. There size-constancy can be given by the perspective motives for distance on a table which supports various objects. Reduction is obtained by aiming the camera above the table, showing only a near object and a far duplicate, and having a neutral background that looks like a fog out of focus—and that is as untextured a perceptual nothingness as a gray can be.

Herewith I present a pair of photographs which I think are the best photographic example of size-constancy that I have ever seen in a picture. The two girls of Fig. 2 are seated 9 and 27 feet from the camera. In real life they show good size-constancy. (Actually the more remote girl is just a few millimeters taller than the near girl.) In the photograph of Fig. 2 some constancy is lost. The more distant girl is smaller than the near one, but not by a great deal, although the half-tone representation shows more surface and thus less constancy than the original glossy photograph. Remember that, since the distances are in the ratio 1:3, the heights of the sitting girls should also be in the ratio 1:3, but in perception they are not.

The situation here is the reverse of Gibson's hallway perspective with the three cylinders set up in it, all three the same size on the paper.[6] The far cylinder is seen as the largest and the near one as the smallest. You can say of this figure that contrast is working, because the large cylinder nearly touches the ceiling of the corridor as the passage shrinks in perspective, but that, says Gibson, is what size-constancy is: a shrinking of the 'scale' so that objects seem large when they do not shrink on the paper as the perspective distance increases. In the Münsterberg *Pseudoptics* there was a card showing a corridor-perspective with a near man and a far boy, both the same size on the paper. The distant boy looked much larger than the man.

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[5] Harold Schlosberg, Stereoscopic depth from single pictures, this JOURNAL, 54, 1941, 601-605. See also Ernst von Aster, Beiträge zur Psychologie der Raumwahrnehmung, *Zsch. f. Psychol.*, 43, 1906, 161-203, esp., on the Verant stereoscope, 201-203.

[6] Gibson, *op. cit.*, 182.

Schlosberg notes that the surface anchorage in a picture is reduced not only by viewing the picture through a reading glass, but also by observing its reflection in a mirror, and further that the perception of surface is further destroyed when these two devices are used together: one views the mirrored reflection through a reading glass. These means increase the perception of constancy of size in Fig. 2. Doubtless a stereoscopic view of this scene would restore entirely the size-constancy that was apparent when the real girls, near and remote, were viewed binocularly without camera intervention.

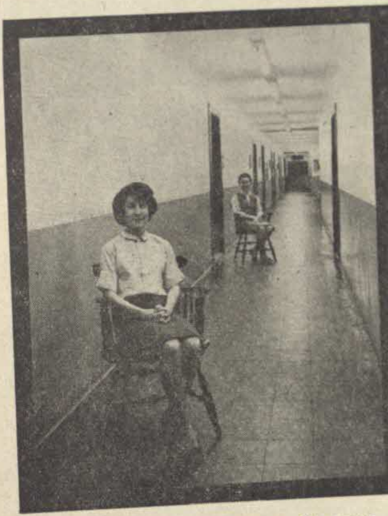

Fig. 2. Size-Constancy in a Perspective Picture

The near girl is 9 feet from the camera; the far girl is 27 feet. The relative distances, and thus the relative retinal heights, are in the ratio 1:3. Size-constancy is not complete; the distant girl is somewhat smaller, but not so much as the ratio 1:3, which is correct for the two girls on the paper. Compare with Fig. 3. Size-constancy is increased when this figure is viewed through a reading glass; it is diminished when viewed upside down.

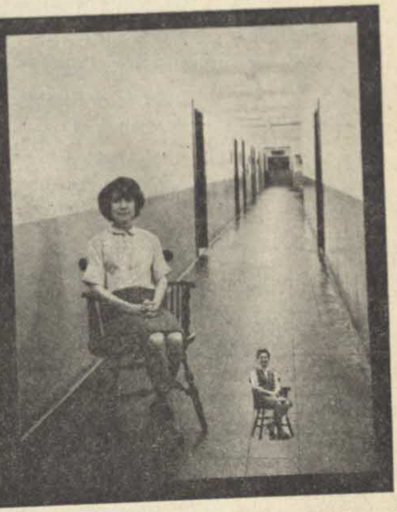

Fig. 3. Reduction of Scene in Fig. 2

The far girl of Fig. 2 is cut out of the picture with scissors, brought forward next to the other girl, and pasted there, with distance equal for the two, a change that amounts to reduction. The ratio 1:3 is now apparent. That the right-hand girl looks smaller in Fig. 3 than she did in Fig. 2, although unchanged in size on paper, is an example of size-constancy under a perspective motive for distance in a picture.

In Fig. 3 we have rephotographed the near girl with the far girl missing, and then we have cut out the far girl from a print of Fig. 2 and pasted her in front at the same distance as the other girl. Placing both girls at the same distance does not change retinal size, and presumably amounts to complete reduction, for there is left now no cue to differential distance between the images of the two girls. Presumably the girls are perceived in the linear ratio of 1:3, the proportion of the retinal images. In Fig. 2 the far girl looks somewhat smaller because you cannot, without the optical intervention of lenses or mirrors, get rid of the effect of the perceived surface. The diminution in size of the right-hand girl from Fig. 2 to Fig. 3 is an exhibit of the operation of size-constancy.

This effect can be exhibited almost as well if a duplicate of the far girl be pasted in next to the near girl in Fig. 2, so that you have a smallest girl in front at the right and a larger but otherwise identical girl 18 feet back over toward the left. This arrangement gets the phenomenon into a single picture, but the judgments are more involved and most observers find the demonstration less striking.

If the mirrored images of the two figures, or of the single combined figure, be viewed, right side up, through a reading glass, size-constancy is increased, and the apparent ratio of the right-hand girl to her duplicate may be as great as 2:1, even though the two pictures are identical on paper and thus in projected retinal size.

A very large number of unconscious cues enter into this and every perception. You would think that the foregoing discussion would apply equally well if Figs. 2 and 3 were viewed upside down, but it does not. The girl that changes size from one figure to the other, remains the same size when the figures are turned through 180°. The perspective of the hallway is still apparent, but these girls, now stuck to the ceiling, behave differently. The stereoscopic view of the recumbent man shows depth with corrected distortion even when it is viewed upside down, probably because binocular disparity dominates in stereoscopic perception. One thinks in this connection of the way in which the appearance of the landscape is altered by viewing it between one's legs, with inverted head, and of how artists try looking at a picture with head turned through 90°. Helmholtz was familiar with these phenomena and the present instance suggests that it might be just as well to invert the picture as the head.[7] Perhaps the inversion is equivalent in some circumstances to reduction.

Harvard University EDWIN G. BORING

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[7] H. L. F. von Helmholtz, *Helmholtz's Treatise on Physiological Optics,* 1866, Eng. trans., 1925, III, sect. 26, 1-37, esp. 8 f.

## USES OF THE CENTRIFUGE IN STUDIES OF THE ORIENTATION OF SPACE

As Howard and Templeton recently pointed out, a given relation between the perceived position of the body and the axes of the surrounding visual field can be created by means of either a tilting-room-tilting-chair apparatus or a centrifuge.[1] They also note, correctly, that failure to recognize this has led, in a number of studies, to the use of the cumbersome and expensive centrifuge, whereas the simpler device would have served as well. The only occasion when the centrifuge may be used to advantage, they suggest, is in the study of the effects of increased gravity.

Howard and Templeton cite Witkin's studies as instances of the unnecessary use of the centrifuge.[2] In drawing this conclusion, they overlook another advantage of the centrifuge, in addition to the one they recognize; and they overlook it because they fail to distinguish between the sensory and judgmental aspects of the task of determining the upright. In the centrifuge, S knows he is being rotated; in the tilting-room-tilting-chair device he knows he is not being rotated. These "known facts" make the total complex of cues available for *judgment* of the upright different in the two kinds of devices, even when the sensory components of the complex are identical. Because there is this difference in the judgmental situation, the use of both devices is appropriate for particular researches. This was true in the case of our centrifuge studies cited by Howard and Templeton. The value to us of using the centrifuge will be evident from a brief review of the stage at which we were in our program of research at that time.

Prior to undertaking work with the centrifuge, we had observed that people differ in the way they determine the upright in space. In one test, S was seated in a tilted chair within a room that was also tilted and he was required to make his chair straight.[3] In carrying out this task, some persons, at one extreme, adjusted their chairs close to the true upright; others, at the opposite extreme, tended to align their chairs with the tilted room, even though this meant that they were in a markedly tilted position. In a second test, S sat in a completely darkened room facing a tilted luminous square frame, within which was a tilted luminous rod, pivoted at the same center as the frame.[4] *S*'s task was to adjust the rod to the upright, while the frame remained in its initial position of tilt. Here again some *S*s brought the rod close to the true upright, regardless of frame position, whereas others tended to align the rod with the tilted frame. Of particular interest—and important in leading us to the centrifuge studies—was the observation of consistency in the way people performed in these two tasks.[5] The person who tilted his body far toward the tilted room to perceive himself as straight was also likely to be the person who tended to align the rod with the tilted frame. Conversely, the person who was able to adjust his body close to the true upright, independently of the tilted room, was also likely to bring the rod close to the true upright, regardless of frame position.

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[1] I. P. Howard and W. P. Templeton, A critical note on the use of the human centrifuge, this JOURNAL, 76, 1963, 150-152.

[2] H. A. Witkin, Perception of the upright when the direction of force acting on the body is changed, *J. exp. Psychol.*, 40, 1950, 93-106; Further studies of perception of the upright when the direction of the force acting on the body is changed, *J. exp. Psychol.*, 43, 1952, 9-20.

[3] Witkin, Perception of body position and of the position of the visual field, *Psychol. Monogr.*, 63, 1949 (No. 302), 1-46.

In interpreting such self-consistency in perception, two alternative explanations suggested themselves. One hypothesis was that people characteristically rely on the surrounding context or remain independent of the context in their perception of an item within it, *i.e.* they tend to be what we later called relatively "field dependent" or "field independent." A second hypothesis was that some people are relatively accurate in determining the upright and others are not. This hypothesis further proposed that accuracy is a function of ability to integrate available knowledge and sensory cues in "figuring out" the location of the true upright. Whereas the first hypothesis emphasizes the sensory basis of determination of the upright, the "accuracy hypothesis" emphasizes the combined use of sensory cues and knowledge in making judgments. The question of which hypothesis is valid was to us an empirical one. The tilting-room-tilting-chair and rod-and-frame situations did not permit a choice between these two hypotheses, since predictions made from each are not distinctly different. Thus, both perceiving an item independently of context (field-independent perception) and being accurate result, in these particular situations, in adjustment of body and rod close to the true upright. In the centrifuge, however, a situation may be created in which each course of action leads to a different outcome. Hence, examining performance in the centrifuge, in relation to performance in the tilting-room-tilting-chair and rod-and-frame tests, offered us the possibility of choosing between the alternative hypotheses.

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[4] H. A. Witkin and S. E. Asch, Studies in space orientation: IV. Further experiments on perception of the upright with displaced visual fields, *J. exp. Psychol.*, 38, 1948, 762-782.

[5] Witkin, The nature and importance of individual differences in perception, *J. Pers.*, 18, 1949, 145-170.

The centrifuge used in the study we carried out with this rationale consisted of a small, fully enclosed room, driven around a circular track, within which was a chair for *S* facing in the direction of rotation. Room and chair could be tilted by *E* independently of each other, toward or away from the center of rotation. The effective force acting on *S*'s body, as he rode around the track, was the resultant between the centrifugal force generated by rotation and the pull of gravity. The force on the body thus no longer coincided in direction with gravity, representing the true upright. By leaving the experimental room erect, it was possible to separate the upright indicated by the surrounding visual field from the upright indicated by the force acting on the body, as they had been in the tilting-room-tilting-chair situation. There was the important difference that in the rotating room the *S*s *knew* they were being rotated. We did not tell them this, but with very few exceptions they became aware of it during the initial period of acceleration of the apparatus. The outcome to be predicted on the basis of each of our hypotheses is evident:

*(1)* If the observed self-consistency in performance across the tilting-room-tilting-chair and rod-and-frame tests is a function of a characteristic tendency to adhere or not to adhere to the surrounding visual field, the persons who align body and rod with tilted room and frame in these situations would also align the body with the room, now objectively upright, in the rotating-room test. Whereas their determinations would be far off the upright in the first two situations (highly inaccurate), they would be close to the true upright (highly accurate) in the third.

*(2)* On the other hand, if persons who bring body and rod close to the true upright in the tilting-room-tilting-chair and rod-and-frame situations do so as a result of a process of effectively "figuring things out," they would in the rotating-room be able to integrate the knowledge that they are rotating with their visual and bodily sensations and again arrive at a correct determination of the upright. In other words, persons who are accurate in the first two situations would also be accurate in the third.

The results obtained when the same *S*s were tested in all three orientations were clearly and strongly in favor of the first hypothesis and against the second hypothesis. This experiment, in which the centrifuge played a decisive role, was a critical one in the development of our conception of field dependence as a cognitive style.

That the distinction we drew in our centrifuge studies between judgments based on sensory cues and judgments based on sensory cues plus "known facts" is a real and important one is suggested by another study, also making use of the centrifuge.[6] Through a series of specific training experiences in the tilting-room-tilting-chair apparatus, as well as a factual lecture about the nature of human space-orientation, an attempt was made to change the mode of orientation in the direction of a more accurate approximation of the true upright. Both training techniques encouraged *S* to "interpret" and "make allowances for" his sensory experiences in relation to his knowledge of what was going on. This procedure resulted in adjustments of the body within the tilted room which were significantly closer to the true upright. Moreover, the training given on the tilting-room-tilting-chair was transferred to the basically similar rotating-room. Thus, through emphasizing the use of knowledge, along with sensory experiences, it was possible to "push" the person away from a limited sensory basis in making his judgments. This change was achieved without apparently affecting *S*'s preferred mode of perception, based directly on sensory experiences.

By failing to recognize the distinction, in perceptual performance, between making judgments on a sensory basis and making judgments on the basis of sensory cues plus relevant knowledge, Howard and Templeton have overlooked one of the important values of the centrifuge in research.

State University of New York HERMAN A. WITKIN
Downstate Medical Center

## THE AUDITORY AUTOKINETIC EFFECT

While many investigations have been conducted on the visual autokinetic effect, few have been made on analogous phenomena in the other sensory modalities. Bernardin and Gruber recently reported on a study on the auditory autokinetic effect.[1] They found that all of their *S*s observed spatial displacement of sounds as well as variations in loudness and in pitch. The purpose of this Note was to extend their investigation.

Thirty-six student volunteers from a beginning class in psychology, served as the *S*s. They were randomly assigned to 12 groups of 3 *S*s each. Testing was conducted in a light- and sound-proof room. Tones of 1000, 2000, 5000, or 10,000~ were presented, respectively, in combination with a 20%, 40% or 60% output of a Heathkit audio oscillator, Model A6-7, at each frequency-level for six 30-sec. periods. A rest-interval of

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[6] Witkin, The effect of training and of structural aids on performance in three tests of space orientation, *Rep. No. 80,* 1948, Div. Res., CAA, Washington, D.C.

[1] A. C. Bernardin and H. E. Gruber, An auditory autokinetic effect, this JOURNAL, 70, 1957, 133-134.

10 sec. was introduced between successive periods. No group received more than a single frequency-intensity combination.

A calibrated rheostat was used to record movement. The pointer of the rheostat was so mounted (perpendicular to a plane running parallel to the face of the sound source) that, by directing the pointer of the rheostat according to instructions, *S* was able to indicate accurately the amount of movement he perceived. Results of a pilot study suggested that a few *Ss* might perceive vertical and 3-dimensional as well as horizontal spatial displacement of the tone. Accordingly, instructions to *S* included the suggestion that any apparent movement of the sound would be on a horizontal plane in front of them. Appropriate voltage changes as a function of rheostat movements were recorded in an adjacent room by *E*. Two measurements, amount of movement and number of movements, were obtained.

Movement was reported by 28 of the 36 *Ss*, and was not a function of the frequency- or intensity-levels employed in this study (all *F*-tests for frequency and intensity effects based on either amount or frequency of movement were non-significant). Two of the 28 *Ss* reporting movement also indicated vertical or 3-dimensional changes.

We conclude, therefore, that changes in frequency and intensity have little systematic effect on perception of horizontal movement, and that individual auditory autokinetic judgments of horizontal movement can be quantified with great precision.

Washburn University, Topeka, Kan. D. CHRIS ANDERSON
Veterans Hospital, East Orange, N.J. CHARLES A. MOSS

## THE WALTER BOWERS PILLSBURY ENDOWMENT IN EXPERIMENTAL PSYCHOLOGY

The Walter Bowers Pillsbury Endowment in Experimental Psychology has been estblished at Cornell by Professor Pillsbury's widow and children for the purpose of "furthering his scholarly ideals." The first use of the Endowment has been to institute a series of public lectures in experimental psychology with special emphasis upon the topic of attention.

The first Walter B. Pillsbury Lecturer was Professor Donald B. Lindsley of the University of California at Los Angeles, who spoke on "The neurophysiological basis of attention" February 21, 1963. The second lecture in the series was given on March 20, 1964 by Professor Daniel Berlyne of the University of Toronto, who spoke on "The concept of attention in behavior theory." The next lecture in the series will be given by Professor

Karl M. Dallenbach in the Fall of 1964 on the history of the concept of attention.

Professor Pillsbury spent the years 1893 through 1897 at Cornell, receiving the Ph.D. in 1896 and remaining another year as an assistant.[1] His son, Walter M. Pillsbury, recalls that his father "frequently spoke in warmest terms about this period of his life and about his association with Professor Titchener."[2] For this reason, the family considered that the furtherance of experimental psychology at Cornell would be a most suitable memorial to Professor Pillsbury, and the Department of Psychology at Cornell has felt deeply honored by the gift. Professor Pillsbury was a Cooperating Editor of this JOURNAL for 64 years, from 1897 to 1960.

Cornell University T. A. RYAN

## THE 1964 MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY

The Annual General Meeting of the American Philosophical Society was held in Philosophical Hall in Independence Square, Philadelphia, on April 23-25, 1964.

The morning of April 24 was devoted to a symposium of six papers under the chairmanship of Edwin G. Boring and titled "Psychology: A behavioral reinterpretation." The symposium was designed to present to the general public of scholarship and letters the nature of the present development of objective psychology as it replaces idealistic psychology in the gradual resolution of Cartesian dualism. The six examples of the *status praesens* were as follows. R. J. Herrnstein of Harvard University spoke, under the title "Will," on motivation and purpose as causal relations. Norman Guttman of Duke University, under the title "Experience," detailed experiments on sensory discrimination of rats and pigeons in which animal sensibility resembles human discrimination or even surpasses human capacity. Philip Teitelbaum of the University of Pennsylvania, with the title "Appetite," described experiments on brain insults and stimulation and discussed the role of the hypothalamus in motivation and especially in desire for food and aversion to it. P. B. Dews of the Harvard Medical School, calling his topic "Humours," described the modern advances in psychopharmacology, especially as based on investigations with animals. Murray Sidman of Massachusetts General Hospital, speaking on "Angst," evaluated the relative roles of punitive reinforcement and aversive stimulation. Finally, B. F. Skinner of Harvard spoke on "Man," showing the nature of man's uniqueness in a deterministic world and making it clear why a revolution in thinking that robs man of freedom need not diminish his dignity.

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[1] K. M. Dallenbach, Walter Bowers Pillsbury, 1872-1960, this *Journal,* 1961, 74, 165-173.

[2] Letter from Walter Milbank Pillsbury to President Deane W. Malott July 6, 1961.

The Penrose Lecture was given by President Julius A. Stratton of the Massachusetts Institute of Technology on "Learning and action," a summary of the changing relation of scientific scholarship to the advance of civilization as it is now promoted in the universities and in nonprofit research organizations. The address at the annual banquet was made by President Clark Kerr of the University of California under the title "Education: Genie or master?" President Kerr developed the view that the present explosion in education and research will place the intellectual in a new role of responsibility as a social and political force. He was optimistic as to the effect of this change on the advance of civilization. Both speakers stressed the importance of maintaining the usefulness of knowledge, citing Benjamin Franklin's title for the society which they were addressing: "The American Philosophical Society held at Philadelphia for promoting useful knowledge."

The Karl Spencer Lashley Prize in Neurobiology was awarded to Dr. Walle J. H. Nauta, an Indonesian who received his M.D. and Ph.D. degrees in Holland and served in three European universities. He came to the United States in 1951, joined the staff of the Walter Reed Army Institute of Research, and was later a member of the faculty of the University of Maryland's School of Medicine. In the fall of 1964 he will become Professor of Neurophysiology and Neuroanatomy in the new Department of Psychology at the Massachusetts Institute of Technology. He is known chiefly for his silver staining technique, his studies of the limbic system, and his contributions to the neurology of emotional tone.

Carl Pfaffmann of Brown University was elected to membership as a psychologist. The psychologist-members in attendance were E. G. Boring, Leonard Carmichael, W. R. Miles, C. P. Richter, B. F. Skinner, and S. S. Stevens.

EDWIN G. BORING

Harvard University

## THE 1964 MEETING OF
## THE NATIONAL ACADEMY OF SCIENCES

The 1964 Meeting of the National Academy of Sciences was held at the building of the Academy in Washington on April 26-29, 1964.

Donald R. Griffin, Judith H. Friend, and Frederick A. Webster of Harvard University presented a paper on "Target discrimination by echolocation of bats." Lorrin A. Riggs, E. Parker Johnson, and Amy M. L. Schick of Brown University reported on "Electrical responses of the human eye to moving stimulus-patterns." W. K. Estes of Stanford University described "A detection-method and probabilistic models for assessing information processing from brief displays."

William D. Neff of Indiana University was elected a member of the Academy. The membership of the Section of Psychology is thus increased from 24 to 25.

The psychologists in attendance were E. G. Boring, Leonard Carmichael, W. K. Estes, C. H. Graham, N. E. Miller, Carl Pfaffmann, C. P. Richter, L. A. Riggs, and S. S. Stevens.

Harvard University EDWIN G. BORING

## THE FIFTY-SIXTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The Southern Society for Philosophy and Psychology held its fifty-sixth annual meeting at the Phoenix Hotel in Lexington, Kentucky, on March 26-28, 1964. The meeting was held in conjunction with the annual meeting of the Kentucky Psychological Association. Approximately 325 members, friends, and guests attended. Hosts were the Departments of Philosophy and Psychology at the University of Kentucky.

The program consisted of 1 symposium in philosophy and 4 in psychology, 25 papers in philosophy and 31 in psychology, and 3 joint sessions. Two Junior Awards were given: one for a paper presented by John T. Wilcox (Emory University) on "One psychological method of closing the is-ought gap," and the other for a paper presented by Vesta C. Skees (University of Louisville) on "List difficulty and replacement factors in paired associate learning." These awards are citations for general excellence; they are given annually for the papers of highest merit presented by members who have not yet received the doctoral degree or who have received it within the preceding five years.

At the first of the three joint sessions, Dan R. Kenshalo (Florida State University) introduced William D. Neff (Indiana University) who presented an invited address on "The temporal lobes and hearing." Leroy E. Loemker (Emory University) served as Chairman of the subsequent two joint sessions; as Past President of the Society, in the first of these he introduced John W. Oswald (President, University of Kentucky), who presented a "Message of welcome," and Dan R. Kenshalo (Florida State

University), who delivered the presidential address entitled, "The origin of quality in cutaneous sensations." The third joint session was an interdisciplinary symposium on "Pavlov processes"; the invited principal speaker was Cletus J. Burke (California State College at Hayward), and the respondents were C. Allen Boneau (Duke University) and Harold N. Lee (Tulane University). Many of the members also attended a dinner on Friday evening, arranged by the Kentucky Psychological Association, and heard an invited address by Erwin W. Straus (Veterans Administration Hospital, Lexington) on "The expression of thinking."

Twenty-three philosophers and thirty-seven psychologists were elected to full or associate membership during the annual business meeting. Reports of the officers and of the several standing and ad hoc committees were read and accepted, and a resolution was passed instructing the secretary to notify the Southern Association of Secondary Schools and Colleges, as well as other appropriate agencies, of the Society's profound concern with the attempt of certain parties to have revoked the charter of Tougaloo Southern Christian College for reasons unrelated to the institution's academic excellence.

The following officers were elected to serve during 1964-1965: Charles Hartshorne (University of Texas), President; Leland E. Thune (Vanderbilt University), President-elect; and Dan R. Kenshalo (Florida State University), Past President. Earl A. Alluisi (University of Louisville) Continues to serve as Secretary, and Dwight Van De Vate, Jr. (University of Mississippi) was elected Treasurer. Edward G. Ballard (Tulane University) and James S. Calvin (University of Kentucky) were elected to the Council as representatives of philosophy and psychology, respectively. Other members of the Council are Edward A. Bilodeau (Tulane University), John J. Compton (Vanderbilt University), E. Maynard Adams (University of North Carolina), Malcolm D. Arnoult (Texas Christian University), and Herbert C. Sanborn (Brentwood, Tenn.).

The members accepted the invitations of Emory University and Morehouse College to hold the Society's fifty-seventh annual meeting at Atlanta, Georgia, on April 15-17, 1965.

EARL A. ALLUISI

University of Louisville
Louisville, Kentucky

## THIRTY-SIXTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The thirty-sixth annual convention of the Midwestern Psychological Association was held at the Chase-Park Plaza Hotel, St. Louis, April 30-

May 2, 1964. The number attending was 1856. The number of members newly elected or reinstated during the year brought the total membership of the Association to 2803.

The Program Committee, Allen Rechtschaffen, University of Chicago, Chairman, selected 269 papers to appear on the program from a total of 400 abstracts submitted. To be able to schedule more papers at future conventions, it was voted to add a 2-hour paper-reading session on Thursday afternoon.

The paper-reading sessions were concerned with: short-term memory; measurement; social psychology; fear and anxiety; human operant and programmed learning; developmental psychology; visual perception; comparative psychology; verbal learning; small group processes; probability learning; vision and audition; human brain damage; animal learning; physiological psychology; industrial psychology; schizophrenia; psychophysics; attitude change; classical conditioning; motive-incentive conditions; clinical psychology; animal operant learning; information-processing; mediational processes; social attitudes and values; personality; stimulus-generalization; concept-formation and problem-solving; avoidance conditioning; ablation; and engineering psychology. A paper-reading session of 13 papers and an invited address were presented by Psi Chi.

There were five symposia on the topics of: Recent computer developments of interest to psychologists; strategies for the psychological diagnosis of brain damage; the actuarial approach to personality assessment; 'arousal' research in animals and humans; and the interaction of process and analogue research to psychotherapy.

G. Robert Grice, University of Illinois, delivered the Presidential Address entitled: "Do responses evoke responses?"

Melvin H. Marx, University of Missouri, was elected President, 1964-65, and David Ehrenfreund, Southern Illinois University, Council Member, 1964-67.

The meeting in 1965 will be held at the Palmer House Chicago, April 29, 30, May 1.

University of Wisconsin FREDERICK A. MOTE

## THIRTY-FIFTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The thirty-fifth annual meeting of the Eastern Psychological Association was held April 16-18, 1964 at the Bellevue-Stratford and Sylvania hotels in Philadelphia, Pa. Total registration at the meeting was 2,365 persons. Of this number 1,137 were members of EPA, 178 were new members who joined the Association at the meeting, and 1,050 were guests. The present membership of EPA totals 3,824.

Jeanne L. Smith was chairman of local arrangements, assisted by George Spivack, Thomas E. Shipley, Jr., Armin Loeb, Bernard Mausner, and Clifton E. Mayfield. The Program Committee consisted of Leonard W. Ferguson, Chairman, John L. Kennedy and Bernard Saper. The Committee scheduled 311 papers (presented in 54 sessions), nine symposia, two special sessions, seven special group meetings, and two invited addresses.

The majority of individual research papers related to topics in experimental, comparative and physiological psychology. Titles of paper sessions in these areas were as follows: visual perception, auditory perception, vision, sensorimotor behavior, perception, discriminative learning, animal learning, conditioning, operant behavior, avoidance behavior, animal motivation, human motivation, early experience and stress, comparative, psychopharmacology, physiological, brain stimulation, brain lesions, verbal behavior, cognitive processes, human learning, cognitive styles, verbal learning, paired-associate learning, memory, decision processes, and semantic constructs.

Three paper sessions were devoted to developmental and child behavior. Social psychology was covered by five sessions entitled social cognition, social behavior, and social attitudes. Papers in clinical research were grouped under projective techniques, schizophrenia, attitudes and rehabilitation, psychotherapy and hypnosis. Other sessions related to personality, programmed instruction, tests and statistics, industrial and educational psychology.

Brief titles of symposia were "Current psychological legislation and the science of psychology," "Recent developments in constitutional psychology," "Operant studies over extended time periods," "What help for parents: family therapy, psychotherapy, or guidance," "Variability of response to psychotropic drugs," "Cross-cultural research in the Far East," "Stress and motivational factors in sensory deprivation," "Projective techniques revisited," and "An industry-wide study of programmed instruction."

Silvan S. Tomkins gave an invited address with the title "Homo patiens." An invited address entitled "Visual noise as a tool of research" was given by Donald M. McKay of the University of Keele, England. Psi Chi had Daniel S. Lehrman as an invited speaker. A special program session was conducted in honor of Theodor Reik. Charles N. Cofer delivered the annual Presidential Address, "On some factors in the organizational and other characteristics of certain verbal processes."

During the business meeting, David McClelland was announced as the President-elect and William N. Schoenfeld, Richard L. Solomon, and E.

Joseph Shoben as new members of the Board of Directors. The membership passed a resolution opposing state legislation which requires the certification or licensing of research psychologists. Other actions were to adopt an official corporate seal and to establish a President's award. The award will enable the President to designate the recipient of a fund, with the purpose of furthering the interests of psychology.

The 1965 meeting of the Association will be held at the Chalfonte-Haddon Hall in Atlantic City, N.J., April 22-24.

Queen's College MARVIN A. IVERSON

## THIRTY-FOURTH ANNUAL MEETING OF THE ROCKY MOUNTAIN PSYCHOLOGICAL ASSOCIATION

The Rocky Mountain Psychological Association held its 34th Annual Meeting in Salt Lake City, Utah, on May 6-9, 1964. A Professional Development Institute on "The psychology of creativity" preceded this year's convention, and was held at the University of Utah on May 6th.

Forty-seven papers were presented, and the topics varied from "Some personality correlates of the ability to stop smoking cigarettes" to "The influence of axial gradients and maturation on the light responsivity of planaria regenerates." Three symposia dealt with (1) "Some psychological attributes of intracranial self-stimulation," (2) "Application of group psychotherapy to meet varying treatment needs in a radically reorganized state hospital," and (3) "Revision of the professional preparation of clinical psychologists."

Merrell E. Thompson's Presidential Address was on "Psychopathology on the university campus." The invited address by Dean R. Brimhall was on "Reminiscence and perspectives."

There were approximately 210 persons in attendance, and the members gave strong endorsement to this year's program and to the publication of the first journal issue of the *Rocky Mountain Psychologist*. Richard A. Lake was named Editor, and Malcolm N. Liedbroder was appointed as Publication Manager of this new journal to be published quarterly by the association.

John D. Cambareri succeeded Merrell E. Thompson as President for the 1964-65 year, and William H. Brown was elected as President-elect. Wilfred H. Higashimachi succeeded John R. Thompson as Secretary, and Helen E. Wilson succeeded Richard A. Lake as Treasurer.

Utah State Department of Health WILFRED H. HIGASHIMACHI

## ELEVENTH ANNUAL MEETING OF THE SOUTHWESTERN PSYCHOLOGICAL ASSOCIATION

The eleventh annual convention of the Southwestern Psychological Association was held April 9-11th at the Granada Hotel, San Antonio, Texas. Some 425 persons from the 8 states in the Southwestern Psychological Association registered. Total attendance was well over 600. Some 55 scientific papers and 11 symposia were presented. A highlight was the presentation by the six finalists in the graduate student research paper reading contest. Each student received a certificate of merit, and in addition the winner of the first prize received $50, and the second and third prize winners received checks of $25. This contest was jointly sponsored by Southwestern Psychological Association and Psi Chi. Dr. Harry Helson, Kansas State University, delivered the distinguished psychologist invited address entitled "Implications of frame of reference psychophysics for personality and social psychology." Irwin Berg delivered the Presidential address entitled "Cultural trends and the task of psychology." At the business meeting of the Association, a formal resolution was introduced recognizing the significance of Karl M. Dallenbach's contributions to psychology and his efforts to strengthen and build it in the southwest during the last fifteen years. The resolution was unanimously approved and after a standing ovation Dr. Dallenbach thanked the membership.

For the year 1964-65, the officers will be as follows: Ira Iscoe, President; Ben Fruchter, President-Elect; Charles Cleland, Secretary-Treasurer; and Irwin Berg, Past President. Representatives to the Executive Committee are: Saul B. Sells, Philip Worchel, and Oliver Bown.

The next meeting of the Association will be April 8-10 in Oklahoma City. Oscar A. Parsons, University of Oklahoma Medical School, is in charge of the program.

IRA ISCOE

University of Texas

## TENTH ANNUAL MEETING OF THE SOUTHEASTERN PSYCHOLOGICAL ASSOCIATION

The Southeastern Psychological Association held its Tenth Annual Meeting in Gatlinburg, Tennessee, with convention headquarters in the Civic Auditorium. One hundred nine papers, nine symposia, two invited addresses, and one Presidential Address made up the program. There were approximately 460 persons registered for the convention.

The Presidential address on "Deprivation and development" was delivered by Susan W. Gray, retiring President. Nicholas Hobbs gave the Psi Chi invited address, "The re-ëducation of emotionally disturbed children." The general invited address was given by Stuart W. Cook, whose subject was "Desegregation: A psychological analysis." Paul Anthony of the Southern Regional Council served as discussant for this address.

Louis D. Cohen was elected President for 1964-65. Other officers for the year 1964-65 are: Ralph Mason Dreger, President-Elect; Susan W. Gray, Past President; Dorothy C. Adkins, Secretary-Treasurer. Hold-over Members-at-Large of the Executive Committee are Theodore Landsman and Wilse B. Webb. Newly elected to the Executive Committee is Sam C. Webb.

Chairmen of committees for 1963-64 were: Louis D. Cohen, Program Committee; Thomas F. Staton, Membership Committee; Ohmer Milton, Local Arrangements Committee; and Nicholas M. Vincent, Auditing Committee.

Major items of business which were transacted in the business session include the following: A joint meeting with the Southern Society for Philosophy and Psychology was discussed upon recommendation of the Executive Committee. The membership granted the Executive Committee authority to seek such a joint meeting if possible. Dues for membership were raised to the limit permitted by the present SEPA Constitution. An amendment to the Constitution will be submitted to the entire membership by mail ballot on the question of eliminating the present constitutional limit of $3.00 dues. Authorization to employ an Executive Secretary was extended to the Secretary-Treasurer after it was pointed out how vastly increased the duties of the office are with the increase in membership.

An announcement was made by the President that two members had died during the year, Marjorie Davidson and G. J. Rich. Two members were placed in the status of Life Members, Marion Stanland and Rolland H. Waters.

In calling attention to the large increase in members, from 629 to 825 in one year, the Secretary commended the Membership Chairman, Thomas F. Staton, for his efforts in bringing about the major portion of this increase.

Jacksonville University RALPH MASON DREGER

# Heinz Werner: 1890-1964

Heinz Werner was born in Vienna, February 11, 1890, the second child, and first son, of Emilie Klauber Werner and Leopold Werner. His father, a manufacturer, died in 1894, and his mother was left with the task of raising four children, all under six years of age. Fortunately, provision was made so that the family did not suffer any material deprivation.

All of Werner's schooling took place in Vienna. After attending public elementary and high schools, Werner entered the Technische Hochschule in 1908 with the thought of becoming an engineer. In 1909, he decided that engineering was not his vocation and transferred to the University of Vienna, still undecided as to a career but inclined toward musicology.

At the University, Werner began his studies on the history of music, but found himself increasingly attracted by the intellectual currents in philosophy and psychology: evolutionary and developmental theories were strong, neo-Kantian and neo-Hegelian viewpoints were still powerful, and the positivist-empiricist movement was flourishing. Moving more and more in the direction of psychology, Werner published several articles in 1912 and 1913 that foreshadowed life-long interests in problems of cognitive development and perception. Among these was one setting forth the thesis that sensorimotor activity constituted the underpinning of the higher reaches of conceptal thought. Another early paper, summarizing researches on 'the blind spot' won him the Trebitsch prize. At the same time, he was integrating his interest in esthetics with his concerns for psychology and philosophy by immersing himself in an examination of the process of 'esthetic enjoyment.' For a thesis based on this research, he was awarded the Ph.D. degree in 1914 *summa cum laude*. This dissertation was subsequently (1916) published as the monograph, *Zur Psychologie des ästhetischen Genusses*.

After his graduation from the University, Werner spent one postdoctoral year (1914-1915) as assistant in psychophysiology in the University's Institute of Physiology, then under the direction of Sigmund Exner. He had a brief period of military service, and from 1915 to 1917 he conducted research at the Psychological Institute of the University of Munich. His interest in esthetic phenomena united his work in the Institute with his outside researches: while he was studying the role of rhythmic factors in perception, he was also investigating the invention of melodies in early childhood. His developmental studies were published in 1917 in the monograph, *Die melodische Erfindung im frühen Kindesalter*.

In 1917, Werner was appointed as Research Assistant in the Psychological Institute at Hamburg, then under the direction of William Stern. One year later he married Jo Gervai, a woman very much interested in esthetics. Despite the stress of the times, Werner continued his scholarly work. In 1919 he published his classic monograph, *Die Ursprünge der Metapher*. After the publication of this book, he was appointed (1921) as Privatdozent in the Philosophical Faculty of the newly established University of Hamburg, which had absorbed the Psychological Institute.

In the stimulating atmosphere of the University of Hamburg, Werner found his element. During the twelve years of his stay, he had such outstanding colleagues as Ernst Cassirer in epistemology and philosophy of language, Carl Meinhof in comparative linguistics, Erwin Panofsky in art history and comparative esthetics, Jakob von Uexküll in theoretical biology and comparative ethology, and William Stern in psychology and philosophy. There were also outstanding younger colleagues and students including the renowned psychopathologist, Alfred Storch and the psychologists, Fritz Heider, Martin Scheerer and Karl Zietz.

In these intellectual surroundings, Werner's productivity increased and his already wide interests were further extended. His monograph, *Grundfragen der Intensitätspsychologie,* was published in 1922. In 1924, *Die Ursprünge der Lyrik* was issued. In 1926, the first German edition of his classic introduction to developmental psychology made its appearance: this volume has now gone through four editions in German, has been translated into Finnish, Japanese, and Spanish and, in revised form, has gone through four editions (1940, 1948, 1957, 1961) in the United States, under the title, *Comparative Psychology of Mental Development*. Finally, in 1932, his book, *Grundfragen der Sprachphysiognomik,* was published. During this period, Werner published in addition some twenty-five articles dealing with problems of perception and language behavior. He also served as co-editor of the *Zeitschrift für Psychologie* and Director of the laboratories of the Psychology Department.

In July of 1933 his positions at the University of Hamburg were summarily terminated by the Nazi regime. Invited by Geza Révész, he was permitted to go to the Netherlands on a temporary visa. While in Holland, he received an invitation from Walter Pillsbury to come to the University of Michigan. Allowed to obtain an emigration visa, Werner jettisoned all his possessions in Germany, and left with his wife for America in October 1933. He stayed at Michigan from 1933 to 1936, aided in part by the McGregor Fund. During this period, he began his well-known studies on contour and metacontrast and also undertook research on binocular perception of depth.

In 1936, the Wayne County Training School offered him the position of Senior Research Psychologist. Although he accepted this appointment, he did not fully embark on his work there until the following year, because of an invitation to come to Harvard University as Visiting Professor for the year 1936-1937. During his year at Harvard, he wrote the provocative paper, "Process and achievement," a critique of emphases in mental testing on end-products and scores rather than on underlying operations. During this period, his monograph, *Dynamics in Binocular Depth-Perception* (1937), also appeared.

After his return from Harvard to the Wayne County Training School, Werner began his pioneer researches on endogenous and exogenous feeble-mindedness, work which continues to exert a considerable influence in the field of mental retardaton. These investigations provided the basis for over thirty articles, some in collaboration, published between 1938 and 1946. His continuing interest in esthetics during this period is reflected in a number of papers he wrote dealing with the 'microgenesis' of musical melodies.

In the fall of 1942, Jo Gervai Werner passed away. Desolate, Werner left the Wayne County Training School the following year to join the faculty at Brooklyn College, accepting an appointment as an Instructor, despite his world-wide renown. Transcending his sorrow, Werner immersed himself in work—continuing his investigations on the effects of brain damage, in collaboration with Zelda Sobel. In 1945, he married Erica Gervai, the younger sister of his first wife, whose devotion to him revitalized and transformed his life.

In 1947, Heinz Werner's enduring interest in language behavior led him to undertake a new series of researches on the acquisition of the meaning of words. In this venture, one of his undergraduate students, Edith Freund, collaborated. This work had just begun when Werner was invited by Clark University to accept a position as Professor in the Department of Psychology and Education.

Werner accepted the call to Clark and was joined shortly afterward by one of his younger colleagues at Brooklyn College, Seymour Wapner. It was at this time that a long-range program on perception was begun. In 1949, the University reëstablished an autonomous Department of Psychology, which had been combined with the Department of Education after Walter S. Hunter left Clark in 1936. Werner was appointed Chairman of the newly independent department and was named G. Stanley Hall Professor of Genetic Psychology.

With its independence thus reëstablished, the Psychology Department began, under Werner's leadership and example, to expand. Graduate training flourished. Despite his administrative duties, Werner continued his research activities, focusing mainly on the areas of perception and language. During his tenure as Chairman (1949-1960), Werner published over 70 articles on problems of perception, in collaboration with Wapner and their students. In 1957, he co-authored a monograph of *Perceptual Development.* At the same time, he was deeply involved in programmatic research, in collaboration with Bernard Kaplan, on problems of symbolization. In 1952, he co-authored the monograph, *The Acquisition of Word-Meaning.* In 1955, he edited an important interdisciplinary monograph, *On Expressive Language,* containing contributions by the psychopathologist, Silvano Arieti; the philosopher, Susanne Langer; the linguists, Roman Jakobson and Hans Sperber; and the psychologists, J. K. Adams, Solomon Asch, and Bernard Kaplan. During these years he also co-authored a number of artcles on processes of symbolization with Kaplan.

Beginning in 1955, a number of honors were bestowed upon him. In recognition of his work, The Ford Foundation provided him with an unsoliciated grant of $5,000. He was elected (1956) to the newly-formed Clark chapter of Phi Beta Kappa; this honorary membership, reflecting the great prestige he enjoyed among colleagues at the University, meant a great deal to him. Also in 1956 he was elected a member of the American Academy of Arts and Sciences. In 1957, the University of Hamburg, recognizing the disservice done to him in 1933, reinstated Werner to the Faculty of that Institution, with the title, Ordentlicher Professor Emeritus.

In February 1960, on the occasion of his 70th birthday, a number of scholars contributed to a Festschrift in his honor. This volume, *Perspectives in Psychological Theory,* contained lengthy contributions by S. Arieti, Solomon Asch, J. S. Bruner, Tamara Dembo, G. S. Klein, Roman Jakobson, N. R. F. Maier, A. H. Maslow, Gardner Murphy, David Rapaport, Martin Scheerer, T. C. Schneirla, and H. A. Witkin.

In June of 1960, Werner became Professor Emeritus at Clark. Officially retired, he now embarked on a new career. Appointed Chairman of the Board of Directors of a recently founded Institute of Human Development, he continued to participate in research, to teach advanced seminars and to write. During the four years following his nominal retirement, he authored or co-authored some twenty-five papers. He also published (with Kaplan) *Symbol Formation* (1963), a volume integrating approximately fifteen years of research. Despite his failing health during the early months of 1964, he co-edited (with Wapner) *The Body Percept* (1964). Although now declining rapidly, he sustained himself on his scholarly activity working almost till the end. He died May 14, 1964, after a short illness.

In honor of his contributions to the University and to his discipline, the Board of Trustees of Clark University, on June 10, 1964, authorized renaming the Institute of Human Development at Clark as the Heinz Werner Institute of Developmental Psychology.

Some months before his final illness, Werner had been invited to write about his career for Volume V of *A History of Psychology in Autobiography*. Despite the pressure of his scholarly work and the increasingly debilitating effects of his illness, he accepted the invitation and undertook to complete the story of his life by early 1965. For those of us who savor intimate accounts of the lives of psychology's men of note, the loss of Heinz Werner's personal history is deeply to be regretted. For Werner, however, whose motto might have been "Man's life is man's work," his books and articles would surely themselves have been autobiography enough.

SEYMOUR WAPNER
Clark University
BERNARD KAPLAN

used a generation or two ago. Considering the present rate of progress, it is well within the realm of probability that the next generation of psychologists will regard such books with the same astonishment we feel today for the writings of Descartes on the relation of body and mind.

Loyola University, Chicago MAGDA B. ARNOLD

*Selectivity, Intuition and Halo Effects in Social Perception.* By RAGNAR ROMMETVEIT. New York, The Humanities Press, 1960. Pp. x, 173. $3.00.

This monograph reports the results of a theoretical and experimental study of interpersonal perception undertaken first in Minneapolis and later in Oslo during 1957 and 1958. Rommetveit argues that the basic psychological processes in person-perception are the same as in object-perception. Both being distal in character, the author believes that the appropriate theoretical system for their analysis is that of probabilistic functionalism. The alternative view that the perception of personality involves the projection of the perceiver's own feelings and characteristics is noted but dismissed. Within the framework of probabilistic functionalism, Rommetveit sets himself the task of describing the variables and processes entering into person-perception and of examining their implications for the "lens model." The fact that person-perception has as its distal focus covert and not directly observable attributes raises the question, for example, of how distal attributes are assessed. Brunswik's suggestion that they are determined through the operation of physical measurements is no longer applicable and is rejected. Rommetveit incorporates into the "lens model" the view of Bruner that perceptual attributes are the result of acts of categorization. The perception of personality, thus, becomes a matter of attaining perceptual foci or attributes that are instrumentally important in social behavior (e.g. personality characteristics such as honesty, hostility, intelligence, etc.) and of the inferential use of cues to mediate information about the distal attributes focussed upon.

The aim of the experimental studies is to investigate the theoretical system described. Stimulated by the methodological device of using photographs varying in the degree to which they reveal different attributes, subjects' discriminations were inferred from their preferential rankings of stimulus-persons (*i.e.* photographs and biographical sketches of male students) as personal friends. The results give tentative support to several hypotheses about the selectivity of social perception. They suggest that individuals will be selectively oriented toward attributes of high instrumental relevance. More strikingly, the results indicate that the stronger an individual's selective orientation toward an attribute the less likely will the individual be aware of the cues mediating information about that attribute. The experiments, thus demonstrate the occurrence of discrimination without awareness. Both the conditions and functional properties of intuitive personality judgments as compared with more deliberate intentional judgments would appear to be of considerable interest. An interesting finding not directly related to the author's theoretical orientation is the low correspondence between subjects' choices and their expressed value judgments. That is, subjects who reported intelligence to be important for determining friendships did not make their choices based upon intelligence. There was, however, a significant relationship between a pre-experimental evaluation of what personal attributes were important and a post-experimental retrospective account of how they made their choices. Thus, subjects showed consistency in their verbal behavior, but their verbal behavior was not in accord with what they did. This emphasizes again the dangers of using questionnaires and verbal tests for assessing behavior. Since the experimental situation differs in significant ways from a "real life" situation, a similar caution may be applied to the present experimental study. Representative experimental design (*i.e.* the ecological sampling of stimulus-situations) to insure generality of findings, is of course the touchstone of Brunswik's methodological principles.

Without careful analysis and definition, the author's assertion that object-perception and person-conception involve the same processes has the status of a theoretical slogan. Since experiments in both involve judgments it is not surprising that factors important in object-perception are also important in person-perception. Thus one would expect to find contrast, assimilation and halo effects in both the perception of objects and personality. Perception, however, involves a variety of processes. The question of the degree to which various kinds of perception involve the same or different processes should be a matter of empirical investigation rather than a matter of *a priori* decision. If an analogy to object perception is to be made, it would seem to the reviewer that the perception of personality is more similar to the perception of shape-similarities than it is, as the author suggests, to the perception of object color. Common to both person-perception and the perception of shape-similarity is the fact that individuals choose their own classificatory criteria (*i.e.* decide when a person is intelligent or two shapes similar). Moreover, the relative salience of the variety of factors involved in both judgments and the weightings given them may change from time to time. The perception of object color, on the other hand, depends to a large extent on sensory functions such as adaptation, contrast, and contour processes which limit the effects of attitude and preparatory sets.

Though the introduction of systematic experiments is of considerable significance, one has the impression on finishing the book that the area of investigation is still too complex for available methods to produce definitive results. In part, this feeling may be due to the fact that because of its style and organization the monograph is difficult to read. The complexity of the experimental situation, however, often leaves the reader feeling that in considering the results he has been cast adrift on a sea of many equally possible interpretations. The significance of statistical tests is also not always clear because of the multiple comparisons which are made. The author, however, has written a monogarph which represents a serious attempt to fit person-perception within a comprehensive theory of perception. Though it clearly cannot handle the many difficulties which a careful reader might raise, it does represent an interesting and stimulating study deserving the attention of both perceptual and social psychologists.

JACOB BECK

Harvard University

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*The Great Psychologists: From Aristole to Freud.* By ROBERT I. WATSON. New York and Philadelphia, J. B. Lippincott Company, 1963. Pp. xiii, 572. $5.50.

This is both a most welcome and a very disappointing book. It is welcome and timely because the history of psychology needs new blood and new presentations, and it badly needs texts to accommodate our course-oriented educational system. We have had to depend too long on Brett and Boring for extensive and intensive treatments of the history of psychology and, particularly for the undergraduate student, both of these—and Brett in particular—have been somewhat too formidable. It is therefore a pleasure to welcome a book that is clearly useful for an advanced undergraduate or a graduate course in the history of psychology.

The book is disappointing because it is no more than a useful book for these courses. Watson fails to tell us in his preface to whom his book is addressed, but he does claim that his goal is reappraisal, re-evaluation and re-examination. None of these goals is realized. There is little in this large volume that the student of history has not seen or heard or read before. In short, this is not an original book in the history of psychology in the sense that Sarton has developed new material in the history of science or that Sigerist, whose *The Great Doctors* most directly invites comparison with the present volume, developed in the history of medicine.

As an introduction to the history of psychology, the book combines the virtue of covering all the major trends and highlighting the great names. While the writing is on the whole undistinguished, the biographical material, social background, and some gossipy tidbits serve to spice up the sometimes dreary course of historical development. The student who has read this book will take with him not only an appreciation of psychology but will also have an image of the men who brought it to its present stage. Since nearly all the important figures are at least mentioned, the instructor will find it easy to replace some of Watson's biases with his own. The general level of psychological knowledge required of the student is not too high, so that no extensive concentration in psychology need be required of the potential user of this volume. Whenever necessary, the author patiently—sometimes too patiently—develops the necessary concepts.

The reader who expects a scholarly history of psychology will be rather disappointed. To cite a few examples: The reader is given the impression that Brett's *History* consists of two volumes—or at least that the third volume was not consulted. Most of the numerous references were apparently obtained from secondary sources—how else is one to explain as many as ten misprints, some of them copied from other secondary sources, on a single page of the bibliographic notes? And why cite James' *Principles* from the *Great Books* reprint when the original is so easily available? Too often original sources, though promised in the preface, are not given in the bibliography. More generally then, this is a derivative history; it contains very little original historical research. Furthermore, it is apposite that psychology be provided with a history that has a consistent point of view. Watson follows his predecessors on the whole by telling us who did what, where and when. There must be discernible conceptual developments to be advanced, but psychologists have generally hesitated to be that adventurous, and Watson is no exception.

As far as the author's emphases are concerned, this reviewer can only cite differences of opinion—absolute judgments are difficult here. But considering the extensive treatment of the classic Greeks and the middle ages, it is unfortunate that the Islamic and Jewish traditions have been slighted; certainly Avicenna and Maimonides seem to belong in this book. Watson also underestimates the influence of Hume and Kant, and, in this reviewer's opinion, undeservedly glosses over Vives, Selz, James Mark Baldwin, among others.

But these are minor failings in a text, particularly in contrast with the book's major virtue of presenting at length material on the Greeks and on the medieval period that has generally not been available in a short history. No student of psychology, after reading this book, will be able to maintain the fiction that psychology started with Wundt, or even Descartes. Completely apart from its other merits, this virtue alone deserves wide use of this book for the first undergraduate or graduate course in history. It is unfortunate that it does not have much utility beyond that point.

GEORGE MANDLER
University of Toronto

*Social Learning and Personality Development*. By ALBERT BANDURA and RICHARD H. WALTERS. New York: Holt, Rinehart and Winston, 1963. Pp. xii, 329. $7.00.

The primary focus of Bandura and Walters' contribution to the literature of learning and personality is on imitation. Imitation is conceived of as the learning of new responses or the changing of response hierarchies as a function of the observation of a model without the performance of any overt responses or receiving any direct reinforcement during the acquisition. Bandura and Walters rightly point out that much human learning results from the observation of models and that this area of learning has been badly neglected in the construction of learning theories. The general approach stresses the processes by which one acquires responses rather than the content of what is learned. That is, the authors are concerned with how responses are acquired or performed, not so much with the language of individual differences, the generality of behavior or the categories for which generalization may be expected.

The book presents data from a series of studies by Bandura, Walters, and their collaborators, as well as a review of other directly relevant literature and anecdotal descriptions taken from the field of cultural anthropology. In addition to the role of imitation, data are summarized to show the effects on personality development of direct reinforcement, generalization, and discriminative learning.

Bandura and Walters criticize the approaches of other writers in the area of learning and personality in that they fail to deal with acquisition of new responses or that they fail to provide principles for observational learning. To this reviewer they make a strong case for the importance of observational learning in the development of personality but it is questionable that they have actually dealt with the problem of the acquisition of novel responses more thoroughly than the views which they feel are more limited than their own. Perhaps this is because it is not entirely clear what they mean by a novel response. One example used in their research is the hitting of a "Bozo" inflated doll with a mallet. Children of the age they were working with have used a mallet before on a pegboard, they have hit objects, and they may very well have aggressed toward Bozo dolls. In another example the novel response which was being imitated was not taking a piece of candy. It appears that what they are dealing with is more performance of a previously learned behavior in a new situational or cue context than it actually is with the acquisition of a new response. In any case the research does not deal with why some behaviors are imitated more than others and what are the principles which account for the ability of the subject to perform a response through observation which he has never tried before. What is clear is that if the response or its elements are already in the *S*'s repertoire he can perform it in a novel situation as a consequence of observational learning. A brief discussion of a learning interpretation of identification is provided with research showing the effects of models with different reinforcing properties.

Much of the research reported had to do with the modeling effects on aggressive behavior but the literature is also reviewed in relationship to direct reinforcement and modeling effects on sex behavior, dependency, and the development of self control. In the latter topic the authors deal with another difficult and neglected problem of learning theory, that of self-reinforcement. Again they bring attention to the fact that some such process must be playing a strong role in determining performance but it is not clear just how the process of self-reinforcement develops or affects behavior. Discussions of moral behavior and of implications for psychotherapy are also included. In the latter case direct reinforcement is leaned upon more than other principles but the implications of discrimination learning, social imitation, extinction and counter-conditioning are also examined. It appears that the authors have moved toward an "empirical law of effect" and there is much less reference to drives or drive reduction than in their previous book *Adolescent Aggression,* although the issue of the *nature* of reinforcement is not directly discussed.

The volume is well written and well referenced. It should serve not only as a valuable addition to the theory and research in personality development but also as a stimulus for further research and theory construction in some of the more complex and difficult areas of social learning.

University of Connecticut JULIAN B. ROTTER

*Biotechnology: Concepts and Applications.* By LAWRENCE J. FOGEL. Englewood Cliffs, N.J., Prentice-Hall, 1963. Pp. xvii, 826. $22.00.

Leafing through this book is enough to persuade the cognoscente that this is the most ambitious textbook yet written in the growing field of human factors engineering. It is immediately impressive on two counts: (1) there is an extraordinary amount of material packed into it; and (2) the author is a man who is himself not a life scientist, but an engineer.

The book contains 20 chapters grouped into six main sections as follows: Section A, "The scientific method; Section B, "The human information input channels"; Section C, "Decision-making"; Section D, "The human information output channels"; Section E, "Machine and system design"; and Section F, "An overview of biotechnology." Unusually complete bibliographies are given at the end of nearly every chapter and selected bibliographies appear at the beginning of each section. The references alone should be of great help to scientists and practitioners in the field. The book is liberally illustrated and well indexed.

A detailed inspection of the contents of the book, however, is certain to impress psychologists in quite another way: The author has some rather extraordinary ideas about the life sciences. Take, for example, Section A which is devoted to the scientific method. Almost any working scientist would expect to find something about experimentation in such a discussion, but this is not at all what Fogel sees as science. To him, the scientific method consists exclusively of mathematical modelling. This is a most extraordinary point of view considering that the solid meat of the book, the facts about human behavior which fill most of the rest of the book, were obtained not by mathematical modelling, but rather by empirical experimentation. The author's point of view about the experimental method is even more difficult to interpret when one reads the illustrative problems at the end of the first chapter. To my mind, some of these are not even examples of mathematical models, but are rather cute mathematical 'tricks,' or 'puzzles' of the kind that are used by psychologists to study complex problem solving or rigidity and insightful behavior in the formation of concepts. This entire section left the reviewer with a feeling of complete bewilderment: Just exactly what does Fogel think science is?

Certain other sections of the book also have a rather unusual and strange cast. For example, we find in it a completely uncritical description of Land's work on two-color projections with no mention at all of the many excellent criticisms of Land's work written by such eminent visual scientists as Judd, Walls, and Hurvich. There is also a section on quite a number of esoteric visual illusions and a rather extensive discussion of Luneberg's controversial but highly mathematical theory of space perception, at the expense of much solid stuff of visual psychophysics. It is difficult to avoid the impression that Fogel is captivated by the exotic and the unusual in psychology. But perhaps all this is not so difficult to understand in a man who states that "The field of psychology is concerned with the thinking process." (p. 799) and that "Psychophysics is the study of the psychological behavior of individuals when placed in a problem solving situation" (p. 800).

When Fogel discusses topics which are more clearly within his field of competence, he can be very good. For example, his discussion of system reliability is the best I have ever seen in a book which was not intended for professional engineers. His treatments of manual tracking and cockpit design are examples of other well-written chapters.

To sum up, then, I found this a frustrating book. There are some sections which are really first-rate; others which, in my opinion, are at the least misleading, if not downright wrong. One further point: This is not an easy book to read. Fogel is an engineer, and he never lets you forget it. I have the feeling that if something could have been said either with words or with an equation, Fogel always chose the latter. He is not a teacher and he appears to make no effort to explain anything to anyone who is not already at his own level of mathematical sophistication. This is unfortunate because it thereby guarantees that the book will have an extremely limited audience among psychologists.

ALPHONSE CHAPANIS

Johns Hopkins University

*Sensory Communication: Contributions to the Symposium on Principles of Sensory Communication.* Edited by WALTER A. ROSENBLITH. Cambridge, Mass., M.I.T. Press, and New York, John Wiley & Sons, 1961. xiv + 844. $16.00.

This volume summarizes the first, more formal week of a two week interdisciplinary international symposium on the principles of sensory communication. The conference was held in July, 1959 under the joint sponsorship of MIT and the Air Forces Office of Scientific Research. It brought together forty-two distinguished participants whose names run from Attneave through Zotterman, whose affiliations range from brain research units through departments of zoology, and who discussed sensory performance in contexts as different as the A-neuron and the Y-maze.

An informative summary of the contents of this collection of papers which, in the editor's words, "constitute a record, rich in subscripts of this day, of a many pronged search for principles of sensory communication" is impossible here. From a classical viewpoint, it is a volume on sense physiology and the problems dealt with are mainly those of sensory discrimination and organization.

All sense modalities are treated, and different papers emphasize electrophysiological data, neurochemical properties, or behavioral measures in organisms as different as man and the beetle. Sampled at random, there are papers that deal with the psychophysics of scaling, neural growth in cortical tissue following radiation lesions, statistical detection theory, microelectrode recordings in the visual cortex, a mathematical model of the binaural fusion process, taste relays in the central nervous system, evoked potentials, wavelength discrimination, neuronal recognition mechanisms, a mathematical model for optomotor responses, and the visual quantum theory.

The volume contains much important information, some interesting speculations, and it provides an overview of the wide variety of approaches and techniques now current in the study of sensory problems. There is both a name-index and an excellent 12 page subject-index. The volume is highly recommended.

University of Pennsylvania LEO M. HURVICH
DOROTHEA JAMESON

*Psychology of Judgment and Choice: Theoretical Essay.* By FRANK RESTLE. New York: John Wiley and Sons, 1961. Pp. xiii, 235. $6.95.

The analysis of judgment is the purest example of a methodological area in sensory psychology, and the study of preferences serves much the same role in economics. Each illuminates the operational hazards encountered in an important theoretical area: sensory theory on the one hand and utility theory on the other. Each abounds in principles that are only partly effective and often conflictful: adaptation-level, the law of comparative judgment, probability matching, minimal regret. In short, either of these areas is brutally rough on the reader in search of light entertainment, and the intersection of the literature on judgment and choice must be close to the most forbidding that psychology can provide.

Fortunately Restle's treatment is highly original and very selective. His list of only 65 references attests to his light-footed approach. Actually the essay is written not around the literature but rather to exploit a theoretical idea. Choices are made by comparing the elements of a new situation against a set of ideal referents. These are heuristic rules (called schema) which say "if the situation is like so and so, do such and such." They are built into the chooser by experience, instructions, consequences of prior choices, and indeed anything else that the subject can package and file for future guidance at choice points. A simple data-processing mechanism compares aspects of a new situation point by point with the properties of the situations in selected ideals and brings itself to a choice of one of the latter by determining the closest match. The processing is random and the choice is uncertain. Thus Restle presents a computer analogue of the process of choosing.

These ideas are a bit abstract. "Situations" are ill-defined and probabilities are expressed symbolically as measures over equally ill-defined discriminable "aspects" of the situations. In Restle's clever hands, however, they are applied to a succession of problems ranging from gambling preferences and guessing patterns, to signal detection and psychophysical scaling. The sequential character of the data processor leads to an analysis of simple reaction-times and perceptual shifts, as random counting processes.

Restle's approach is mathematical but not necessarily quantitative. The book is beautifully written but not necessarily easy to read. These paradoxical qualities evolve from his systematic avoidance of hard definitions for situations, aspects, and schemata, and from his disinterest in detailed analysis of the processes he touches. Visual detection and quantum statistics are encountered but they lead Restle to analyze response biases rather than the bloody details of coincidence counting mechanisms believed to operate in visual detection. Even experienced readers will occasionally wonder where they have been when Restle brings a topic to its conclusion.

Taken in sum, the book reinforces a conclusion formed by this reviewer from Restle's earlier writing. Restle is one of the new mathematical psychology's ablest critics. His final chapter on psychophysical judgment is a remarkable example of critical analysis. It clears away much of the snow dumped in the blizzard of words over the pros and cons of the power law.

WILLIAM J. MCGILL

Columbia University

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# THE EFFECT OF IRRELEVANT MATERIAL ON THE EFFICIENCY OF SELECTIVE LISTENING

By ANNE M. TREISMAN, University of Oxford

In the last decade there have been a number of studies of the variables influencing the efficiency of performance in selective listening-tasks where verbal responses are made to one of two or more competing, simultaneous messages. Broadbent reviews this field and proposes the general model of a selective filter operating at a central stage between reception and response, and whose main function is to spare an information-channel of limited capacity.[1] Cues which are effective in allowing selection are the general physical characteristics of the messages as sounds, *e.g.* the long-term frequency-spectrum, the intensity, the spatial localization.[2] Classes of signals distinguished by such features are said to come on different functional channels. In the absence of such distinctions between classes of sounds, some selectivity of response appears to be possible on the basis of transitional probabilites between words, but this is much less effective.[3]

The effect of varying the amount of irrelevant material has been investigated by Poulton, who found that it decreased his *S*s' chances of noticing and responding to their particular call-sign and message, whether

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[1] D. E. Broadbent, *Perception and Communication,* 1958, 36-45.

[2] Broadbent, Failures of attention in selective listening, *J. exp. Psychol.,* 44, 1952, 428-433; Role of auditory localization in attention and memory span, *J. exp. Psychol.,* 47, 1954, 191-196; W. Spieth, J. F. Curtis, and J. C. Webster, Responding to one of two competing messages, *J. acoust. Soc. Amer.,* 26, 1954, 391-396; J. P. Egan, E. C. Carterette, and E. J. Thwing, Some factors affecting multi-channel listening, *J. acoust. Soc. Amer.,* 26, 1954, 774-782.

[3] E. C. Cherry, Some experiments on the recognition of speech with one and with two ears, *J. acoust. Soc. Amer.,* 25, 1953, 976-977.

these came on the same channel or on a different one.[4] He used a task which required monitoring of one or both channels, rather than a continuous selective response to one and exclusion of the other. Here the 'filter' would either have to shift between channels, or not be used at all. Webster and Solomon varied the number of competing messages in an experiment on the effects of complexity of response in listening-tasks.[5] They found that *S*s responded to one message with progressively decreasing efficiency as one, two or three competing simultaneous messages were introduced, when these differed either in location or in frequency-range from each other and from the relevant one. However, part of the difficulty could probably be attributed to the *S*s' initial identification of the relevant message, since this was distinguished only by its prefixed call-sign. A selective filter could not therefore be set in advance for any particular channel, since all would have to be monitored initially. No experiment seems yet to have isolated the effects of an increased load of competing material on selective attention to a given channel, known in advance, where problems of initially 'setting the filter' correctly play no part.

The object of this paper is to throw more light on the nature of selective filtering in auditory attention by holding the selected channel constant and distinct, and varying the following features of the irrelevant, competing material: (1) the number of separate input-channels on which it is presented (or the number of discriminable classes of sounds it comprises); (2) the number of messages or verbal sequences presented; (3) their informational content, meaningfulness and verbal similarity to the selected message. In all cases the discrimination between the selected channel and the irrelevant channel or channels was made as easy as possible, since this was not a present point of interest.

The theoretical points to be discussed are: the level at which the filtering occurs; the functional order in which features of the input-messages are analyzed; and the site of any overloading which may be caused when the amount of irrelevant material is increased and selective attention breaks down. There has recently been some controversy about the level at which selection occurs—whether it precedes some stages of input-analysis or operates only on the fully identified signals. The first alternative, implied by Broadbent, is that irrelevant messages are discarded before all the information is extracted and their verbal content is identified, while the second suggestion, recently put forward by Deutsch and Deutsch,[4] is that all inputs are fully analyzed and the limits of attention apply only to 'awareness,' memory and response.

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[4] E. C. Poulton, Two channel listening, *J. exp. Psychol.*, 46, 1953, 91-96.

[5] J. C. Webster and L. N. Solomon, Effects of response complexity upon listening to competing messages, *J. acoust. Soc. Amer.*, 27, 1955, 1199-1203.

Since most of the general physical characteristics of the messages can be used as a basis for selection, they must be discriminated either before or by the filter. One might distinguish whether all inputs are first separated onto functional channels by discrimination of all their physical features and then passed to a filter which discards some messages and selects others, or whether a filter can also operate at an earlier stage to select which of these general physical features will be discriminated. If two irrelevant messages differ from each other in localization or quality of voice, must they always be separated onto different channels in the brain or can they be discarded together as soon as they have been distinguished from the relevant message? In the latter case, performance should be identical to that with two irrelevant messages which do not differ in any physical feature, *i.e.* which are in the same voice and position.

It should also be valuable to determine at what stages the overloading occurs when too much irrelevant material is presented, since again this must be either before or at the filter. Messages which have been rejected should cause no interference at more central stages of analysis. There are several points at which this overloading might occur: It might be in the analysis of signals onto channels, or in the choice of which channels to select and reject, or during identification of the verbal content, or finally in the selection and organization of responses. If the limit were set by the separation of input-channels, the efficiency of selective attention should decrease as both the number and the difficulty of the discriminations at this level were increased. If it lay in the selection and rejection of channels by the filter itself, only the number of channels active would be relevant. If the overloading occurred either in identifying the verbal content or in selecting the responses, one would expect differences in efficiency as the informational content, meaningfulness, and similarity of the messages were varied.

In the series of experiments to be described, the *S*'s task was always to 'shadow' (attend to and repeat back continuously) a passage presented to his right ear or to both ears over headphones. The competing messages were always presented on one or more separate channels, differentiated from the relevant message by their apparent localization. The smallest difference was that between a binaural message presented at equal intensities to the two ears over the headphones and a monaural message matched in loudness. Cherry has shown that when two messages are presented dichotically, one can be shadowed very efficiently, with little or no sign of interference from the other.[7] Similar results are obtained when both messages are presented to both ears either in different voices (male and female) or with different apparent localization produced by differences in intensity of 4 or 5 db at the two ears.[8] Thus the 'nearest' irrelevant message in the present investigation was always clearly discriminable from the one to be shadowed, and the *number* of irrelevant messages could be varied without increasing their physical similarity to the relevant message. Broadbent has shown that the difficulty of discrimination between relevant and irrelevant channels affects the efficiency of performance,[9] so it is important that this should be controlled.

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[4] J. A. Deutsch and Diana Deutsch, Attention: Some theoretical considerations, *Psychol. Rev.*, 70, 1963, 80-90.

## GENERAL METHOD

*Passages of prose.* Except in the last experiment of the series, the passages used were all narrative or descriptive extracts from "Lord Jim" by Conrad, 150 words long and lasting 1 min. They were read as far as possible in a monotone, with pauses between words or phrases minimized. The passage to be shadowed always started a few words before the others, to avoid any difficulty of initial selection.

*Apparatus.* The passages of prose were recorded on a Brenell Mark 5 two-channel tape-recorder, modified to give a separate output through independent amplifiers from the two tracks of the tape. The recordings were monitored on a micro-ammeter and repeated, if necessary, until the mean intensity-level was approximately the same through all passages. The passages were played back on one or two twin-track Brenell tape-recorders. Their outputs were fed in different combinations to the input-sockets of a mixing unit, then passed to attenuators calibrated in decibel-steps, with cathode-followers for isolation. These were then mixed again in various combinations and fed through matching transformers to a pair of Brown Type K moving-coil headphones. Thus the intensities of each of four messages could be controlled independently in the mixing unit. For two-channel dichotic listening, two messages were presented, one to each ear. For three independent channels, three different outputs from the tape-recorders were used, one to each of the two ears and one to both ears, so that two of the messages were localized separately in the two ears and one was heard in the center of the head. To have two messages coming on one channel and one message on another, two tape-recorder outputs were fed either both to both ears or both to the same ear, and a third was given to the other ear only. To keep the apparent loudness approximately equal, the binaural messages were attenuated by 8 db at each ear compared to the monaural ones, which were about 60 db above the threshold for detectability of the speech.

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[7] Cherry, *op. cit.*, 977-978.

[8] A. M. Treisman, "Attention and Speech," Doctoral thesis, Oxford, 1961, 28, 22.

[9] Broadbent, *op. cit.*, 1958, 73-80.

*Subjects.* The *S*s were 45 undergraduates and two research students at Oxford University, who volunteered to help with the research. Three, used in Experiment I, had studied psychology, but all were naïve so far as this experimental problem and method were concerned. They were assigned to the different experimental groups in the order in which they volunteered—effectively at random.

*Procedure.* The *S*s were first given a few trials for practice in shadowing monaural passages of prose with one irrelevant passage on the opposite ear, until they were able to repeat back the relevant message continuously and fluently. They were then given the test-passages with different combinations of irrelevant material on the irrelevant channels and asked to attend to and repeat aloud the passage arriving on the selected channel. They were asked afterwards if the messages all seemed approximately equally loud, and on the one or two occasions when they were not satisfied of this, the results were discarded. Within each experimental group, the order of the conditions was randomized among *S*s. Their responses were recorded on a Grundig Stenorette recorder and were later analyzed. The number of words correct, of omissions and of errors was noted.

## EXPERIMENT I: ONE, TWO AND THREE CHANNELS

The variables tested in this section were: (a) the number of irrelevant messages, (0-2); (b) the number of irrelevant channels used (*i.e.* whether the two irrelevant messages were separate or mixed on one channel); (c) the spatial localization of the irrelevant messages.

*Procedure.* Eight conditions were tested: in Conditions 1 to 6, *S*s shadowed the message on the right ear and were given 0, 1 or 2 irrelevant messages on either the left ear or both ears, while in Conditions 7 and 8, they shadowed the binaural message with respectively 1 and 2 competing monaural messages. The details of presentation are given in Table I.

TABLE I

PERCENTAGES OF WORDS CORRECTLY REPEATED IN EXPERIMENT I

(The three digits indicate the three channels used, left ear, binaural and right ear, reading from left to right respectively; the numbers 0, 1, and 2 give the number of messages on each channel; * indicates the message shadowed.)

| Subjects | Condition | Localization of messages | Experimental | | Control | |
|---|---|---|---|---|---|---|
| | | | *Mean* | *S.E.* | *Mean* | *S.E.* |
| Group II | 1 | 001* | 96 | 1.4 | — | |
| | 2 | 101* | 94 | 1.2 | — | |
| Group I | 2 | 101* | 94 | 1.1 | 97 | 1.1 |
| | 3 | 011* | 88 | 2.5 | 97 | 0.5 |
| | 4 | 111* | 79 | 2.8 | 96 | 0.8 |
| | 5 | 201* | 97 | 0.8 | 97 | 0.5 |
| | 6 | 021* | 94 | 1.5 | 96 | 0.7 |
| | 7 | 01*1 | 95 | 1.1 | 96 | 1.1 |
| | 8 | 11*1 | 66 | 3.2 | 95 | 0.7 |

Three sets of 7 passages, recorded in one woman's voice on three tracks of tape, were used with 14 *S*s for Conditions 2 to 8, and a different set of two pairs of passages was later used to test Conditions 1 and 2 with a different group of 9 *S*s. The assignment of passages to conditions was varied at random for the different *S*s within each group.

A control series was carried out on 7 other *S*s, using as the one or two rejected messages simple, repeated speech sounds, 'a-a-a' and 'bet-bet-bet,' to see if any differences in the first part of this experiment were caused simply by peripheral masking, and also to indicate how far the content of the irrelevant message was affecting the results.

*Results.* The *S*s found no difficulty in identifying which was the message to shadow and always started off correctly. Intrusions from the wrong messages, although not counted, were negligible. Table I shows the percentages of presented words which were repeated correctly in the different conditions.

An arc-sine transformation of the scores was made, to equate differences in variance, and an analysis of variance was carried out on the results of Conditions 2 to 8. Individual comparisons of pairs of conditions were then made, using the $q$-test.[10] Conditions 1 and 2 for the second group of *S*s were compared by means of a $t$-test.

The analysis of variance showed highly significant effects ($p < 0.001$) of both conditions and *S*s. The $t$-test showed no significant difference in shadowing efficiency with one and no irrelevant channels active, but all $q$-test comparisons between conditions with one and conditions with two independently localized competing channels (2 and 4, 3 and 4, 7 and 8, 6 and 4 and 5 and 4) revealed significant differences in performance ($p < 0.01$). However, the conditions with two irrelevant messages on a single irrelevant channel, 5 and 6, gave slightly higher scores than the comparable conditions with only one irrelevant message, 2 and 7, a difference in the reverse direction. This difference did not quite reach significance using the $q$-test, although $t$-tests were in both cases significant ($p < 0.05$). This is good evidence at least that two irrelevant messages on one channel cause no *more* difficulty than a single irrelevant message.

The apparent distance between the irrelevant and the selected messages appears to have a slight effect (as shown by the comparison of Conditions 2 with 3 and 5 with 6), but this was not sufficient to reach significance. This confirms the assumption made in the experimental design that the problem of discriminating selected from irrelevant message would play a minor part, if any. However, it may have contributed to the relative difficulty of Condition 8 (significantly worse than 4, $p < 0.01$), since here both irrelevant messages were at the shorter distance, while only one of the two was as near the shadowed one in Condition 4. Another possible explanation is that the *S*'s own voice shadowing the prose will also be heard binaurally, and may therefore mask the central message more than the others. One *S* spontaneously complained of this effect. However the fact that Condition 7 gives a significantly higher score than Condition 3 makes this doubtful. The control conditions were all found very easy by all 7 *S*s, and the analysis of variance showed the effect of conditions of be quite insignificant ($F = 1.09$).

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[10] W. J. Dixon and F. J. Massey, Jr., *Introduction to Statistical Analysis,* 1957, 169-171.

The *S*s were also asked whether they noticed any of the content of the irrelevant messages and only in two cases did they mention anything. (These were both in fact probably cases of 'significant' stimuli being heard, comparable to Moray's *S*s' hearing their own name despite the 'attention barrier.'[11] One man noticed the title of a book whose author he knew well, and the other noticed that the passage was about a country that he had just visited.) This general lack of knowledge, or at least of recollection of the irrelevant messages, seems to have the somewhat paradoxical implication that, while the meaningful (or perhaps the varied and less redundant) content causes considerably more interference than the repetitive vocalization in the condition with three separate channels, the *S*s are unable to report what is said. These results will be discussed further after the second series of experiments has been described.

## EXPERIMENT II: THREE CHANNELS; CHARACTERISTICS OF THE IRRELEVANT MESSAGES

This further series was carried out on different *S*s to investigate in more detail the following factors: the effect of varying the difference in localization between the two irrelevant messages, *i.e.* increasing the difficulty of discrimination between rejected channels; the effect of recording the irrelevant messages in different voices, a man's and a woman's, rather than in the same voice—*i.e.* increasing the number of dimensions along which the messages differed; the effect of varying the verbal content of the irrelevant messages, to see whether similarity to the shadowed message, meaningfulness or contextual probability led to any differences in interference.

*Procedure.* The experiment was divided into three parts. In the first, the message to be shadowed was always on the right ear only, and one irrelevant one on the left ear only, but the third binaural message was localized at four apparent distances from the left ear by varying the binaural difference in intensity. The attenuators were set at the following levels: right and left ears monaural each at 60 db, and the binaural message in Condition 9 at 59 and 32 db, in Condition 10 at 58 and 41 db, in Condition 11 at 57 and 47 db, and in Condition 12 at 52 and 52 db on left and right ears respectively. These conditions would thus provide information on the intermediate stages between Conditions 4 and 5 in the preceding series.

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[11] Neville Moray, Attention in dichotic listening: Affective cues and the influence of instructions, *Quart. J. exp. Psychol.*, 11, 1959, 56-60.

In the second part, the message to be shadowed was again always on the right ear and in a woman's voice. Condition 12 (see above) was identical to Condition 4 in Experiment I, with two irrelevant messages in the same woman's voice, one on the left ear and one on both ears. In Conditions 13 and 14 two irrelevant messages were also used, but one was recorded in a man's voice and given binaurally at equal intensities so that it sounded central, and the other, in the woman's voice, was given either binaurally with the man's in Condition 13 or monaurally on the left ear in Condition 14. These conditions were compared to Conditions 4 and 6 in the first series, where the localization of the messages was the same but both messages were in the woman's voice.

In the third part, the localization and voice were as in Condition 12, and only the verbal content of the two irrelevant messages was varied. In Condition 12 they were passages of prose from the same novel; in Condition 15 they consisted of decimal digits in ascending order from 1 to 10 given repeatedly; in Condition 16 they were in Czech, simplified and spoken with a deliberately English accent, equivalent to nonsense using approximately the same phonemes as English; in Condition 17 they were second-order statistical approximations to English—prose constructed wtih contextual constraints operating only over consecutive pairs of words.[12]

Eighteen *S*s were used in this second series of experiments; they were divided at random into three groups of six, each of which had a different combination of the passages of prose to be shadowed and a different order of conditions in the three parts of the experiment, in order to cancel out as far as possible any differences in 'difficulty' of prose, and any effects of practice or fatigue.

TABLE II

PERCENTAGES OF WORDS CORRECTLY REPEATED WITH IRRELEVANT MESSAGES DIFFERING IN LOCALIZATION

| Condition | Intensity of binaural messages | | *Mean* | *S.E.* |
|---|---|---|---|---|
| | *L* | *R* | | |
| 9 | 59 | 32 | 91 | 1.7 |
| 10 | 58 | 41 | 90 | 2.2 |
| 11 | 57 | 47 | 87 | 2.6 |
| 12 | 52 | 52 | 86 | 2.0 |

*Results: Localization of irrelevant messages.* An analysis of variance on an arc-sine transformation of the scores showed the effect of conditions to be just significant ($p < 0.05$), and that of *S*s to be highly significant ($p < 0.001$). (The variance between *S*s here includes that between the dif-

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[12] G. A. Miller and J. A. Selfridge, Verbal context and the recall of meaningful material, this JOURNAL, 63, 1950, 167-185.

and 15 and 17. The repeated numbers were thus slightly easier to reject than the Czech or the second-order approximation, but they were not significantly easier than the normal prose.

Fourteen of the 18 *S*s did notice the numbers, but only one noticed that one run of ten digits half-way through the passage was reversed in order; only five *S*s noticed that the Czech was different from the prose, and only one noticed that the second-order approximation was different.

TABLE IV

Percentages of Words Correctly Repeated with Irrelevant Messages Differing in Verbal Content

| Condition | Verbal content of irrelevant messages | *Mean* | *S.E.* |
|---|---|---|---|
| 12 | Prose | 86 | 2.0 |
| 15 | Numbers | 89 | 1.9 |
| 16 | Czech | 85 | 2.0 |
| 17 | 2nd-order approximation | 84 | 2.1 |

The majority thought the numbers easier to ignore, except at first, when they were surprised and a little distracted by noticing them.

It seems that neither the meaningfulness of the irrelevant messages (compare prose and Czech interference) nor their decreased word-probability or increased informational content (compare prose with second order) had any effect on efficiency in shadowing the selected message. On the other hand, the repeated numbers were slightly easier to reject. These are more markedly dissimilar from the shadowed prose, even at the level of unidentified sounds, through their rhythm and repetitiveness, and also much more redundant in verbal content.

The differences in error-scores among all conditions in both experiments were negligible. (The mean errors were 5%, with standard error of 0.2.) Most of the differences were therefore due to the varying number of omissions made.

## DISCUSSION

The first point to emerge from these results is that adding one irrelevant message on a separate channel does not produce a decrease in shadowing efficiency, but adding two irrelevant messages on two separate channels does. While the dichotic-listening task with two channels is probably too easy and therefore too insensitive to reveal any differences due to characteristics of the irrelevant message (apart from major changes in its physical nature, such as a considerable increase in intensity),[13] this is no longer true in the three-channel condition. It is, therefore, a useful situation for investigating the limits of efficiency of the selective system.

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[13] Moray, The effect of relative intensities of messages in dichotic shadowing, *Language and Speech*, 1, 1958, 110-113.

Experiment I showed that it is much easier to reject two messages coming on the same channel than two messages on separate channels, and, perhaps, easier even than one such message, despite the increased total intensity in the three-message condition. The control experiment confirmed that the decrement on adding a third channel was unlikely to be a simple masking effect and can plausibly be attributed to features of the selective system in attention. The results of Experiment II extend this finding. They show that the presence of even slight differences in the apparent localization of two irrelevant messages increases the interference. It is interesting to compare this result with a finding in another experiment, that efficiency in shadowing one of two binaural messages is more than doubled when the two are differentiated by 3 db at each ear as compared to no difference.[14] Giving the irrelevant messages in different voices but the same localization also lowered shadowing efficiency, and this too is a cue which would facilitate the selection of one of the two messages.[15] It seems, in fact, that any physical cue which makes it possible for one of two messages to be selected will also cause an increase in interference when the two are to be rejected. Broadbent made the related observation that in Cherry's experiment on two-channel listening[16] "the features of the rejected voice which are observed are also those which are useful in picking out relevant from irrelevant words in other experiments."[17] Conversely, it might be true that any characteristic which cannot be reported will not be an efficient cue for selection.

The theoretical points raised in the introduction concerned the order in which information is extracted from input-messages, the level at which the filter operates, and the site of the overloading which occurs when more than one channel is competing. One can infer that the general physical characteristics of the irrelevant messages are analyzed at an early stage, since they are used as a cue for selection. Moreover, the fact that there is a decrement in efficiency when three channels are active, but not when the two irrelevant messages are on the same channel, suggests that the selective blocking or attenuation is a separate process from the analysis of signals onto different channels, and does not, as Broadbent's term 'filter' perhaps implies, coincide with it. If both could occur at the same stage, the filter should be able to pass the relevant channel and treat all others alike as noise, provided that the selected message was not being masked. In fact the discrimination of all classes of sounds at this stage seems to be 'compulsory' rather than optional and determined by the requirements of the task.

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[14] Treisman, *op. cit.*, 17-27.

[15] Broadbent, *op. cit.*, 1952, 428-433; Treisman, *op. cit.*, 28.

[16] Cherry, *op. cit.*, 978.

[17] Broadbent, *op. cit.*, 1958, 23.

It is clear from the present experiment that the use of three competing channels causes overloading at some point in the system, even when responses are made only to one. As suggested, this might occur in the separation of signals onto channels, or in the choice of which channels to filter, or finally during identification of the verbal content if this preceded the filter and all signals were fully analyzed. In this last case, the improvement with two irrelevant messages on the same channel might be attributed to their mutual masking and decreased intelligibility resulting in a failure to be identified as words.

First, the systems which discriminate classes of sounds and allot them to different functional channels may have a limit to their capacity. If so, the greater the number of discriminable classes of sounds the greater will be the overloading at this stage. This prediction is supported by the results of Experiment I. However, one would expect the difficulty of the discriminations as well as their number to affect efficiency, if they precede any selective system and are in some sense compulsory. This idea is not confirmed by the results with the decreased difference in localization between two irrelevant messages (Conditions 9 to 12). Moreover, some ambiguity arises in the concept of discrimination between channels when two cues for classifying the messages are perfectly correlated, for example, when they are in different voices and on different ears. If the two cues were not correlated and both voices were used on both ears, the model would describe this as four separate channels and would predict an increase in difficulty. But it is not clear whether this applies also when the cues are perfectly correlated, or whether the load is actually decreased by having two dimensions of classification rather than one. However, if the overloading could be located at this primary level of analysis, one might expect some change in efficiency, whether improvement or decrement. The evidence from Conditions 12, 13 and 14 (with combinations of one or two voices in one or two positions) is negative: the differences were quite insignificant, suggesting again that the extra load with three channels active does not have its main effect at this stage.

The second hypothesis about the site and nature of the overloading remains possible. It may be that the different channels reach the filter fully discriminated, but that the filter itself has more difficulty in rejecting two channels than one. Certainly the source of overloading seems to lie in the number of discrete channels resulting from the primary analysis of the inputs rather than in the analysis itself.

Finally, the results with differences in the verbal content of the irrelevant messages give little evidence that these receive any further analysis beyond the stage at which their general physical characteristics have served to separate them onto their functional channels. Except with the numbers, most *S*s failed to notice the differences in content, and there is no evidence that purely verbal characteristics caused any differences in interference. The *S*s could have noticed the different rhythm of the numbers and then switched for a moment to an irrelevant channel (many did miss a few words near the beginning). The slightly better performance could be due to the rhythm's improving discrimination between selected and irrelevant channels. The increase in informational content with contextually less probable words had no apparent effect, which again makes it unlikely that the difference between two- and three-channel listening is due to interference in the identification of words or in the selection of responses. The improvement with the two irrelevant messages on the same rather than separate channels cannot be due to decreased intelligibilty resulting from mutual masking, since the unintelligible Czech gave no better results than the prose. The most plausible conclusion is that the filter in selective attention precedes some stages of analysis of the inputs, rather than selecting which of the fully analyzed inputs will control response and memory, as suggested by Deutsch and Deutsch.[18]

The result seems at first sight to conflict with findings by Webster and Thompson and by Peters which suggested that similarity of content, even of messages coming on different channels, did increase the interference caused by unwanted messages.[19] Webster and Thompson found differences in the interference caused by the 'chatter' occurring in air-control towers and by continuous thermal noise; but these would also differ considerably at the levels preceding the recognition of words, for instance in temporal pattern and frequency-spectrum. Peters, using an intelligibility test with multiple-choice response-sheets, showed that efficiency was lower with 'similar' words than with unrelated words presented simultaneously as interference. However, here again the 'similarity of content' was in one case phonetic and in another rhythmic or temporal similarity, that is to say, similarity between the sounds rather than between the meanings of the words. Moreover the task of alternately listening and writing is rather different from the continuous response of shadowing. The irrelevant 'similar' words appeared as alternative responses on the answer-sheets while the unrelated words did not. If *S*s had inadvertently switched between channels and heard the irrelevant words, these could have led to mistakes more easily than words not included on the answer-sheet.

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[18] Deutsch and Deutsch, *op. cit.*, 80-90.

[19] J. C. Webster and P. O. Thompson, Some audio considerations in air control towers, *J. audio Engng. Soc.*, 1, 1953, 171-175; R. W. Peters, Competing messages: the effect of interfering messages upon the reception of primary messages, U.S.N. School of Aviat. Med., Project NM00106401, 1954, Report No. 27.

The present experiments failed to show more interference from an irrelevant message in the same language and from the same novel than from untelligible nonsense (Czech with an English accent) whose general physical characteristics were the same. On the other hand, when the same Czech nonsense-sounds were presented on the same channel as the selected message, they caused considerably less interference than another English message.[20] Similarity of strictly verbal content may therefore produce a deleterious effect, but only in tasks where either shifting of attention between the channels is possible (with a discontinuous stimulus and response) or the irrelevant message cannot be efficiently suppressed or 'attenuated' at an earlier stage because it is not arriving on a separate channel.

## SUMMARY

Efficiency of performance in a task requiring verbal responses to one of two or more simultaneous speech-messages was investigated as a function of (a) the number of irrelevant input-channels, (b) the number of irrelevant messages, and (c) the verbal content of irrelevant messages. A decrease in efficiency of repeating back the prose was found when two irrelevant channels had to be rejected as compared to one, but not when two irrelevant messages were given on one channel. This finding applied to channels distinguished either by spatial localization or by voice (male and female). There was no difference in performance with variations in verbal content of the irrelevant material, from similar prose to statistical approximations to English or to nonsense-words using English phonemes, although repeated sequences of numbers were a little easier to reject.

The results are interpreted in terms of Broadbent's 'filter'-theory to suggest that there are three functional stages: (1) the discrimination of classes of signals (different input-channels) on the basis of general physical characteristics of the sounds; this stage appears to be 'compulsory' for all inputs, at least in this listening task; (2) the selective discarding or attenuation of signals from irrelevant channels by a filter which may become overloaded and inefficient when more than one speech-channel is active; (3) the identification of words and meaning, carried out only for selective signals.

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[20] Treisman, Verbal cues, language, and meaning in selective attentior, this JOURNAL, 77, 1964, 211.

# LEARNING IN ADULT RATS WITH EXTENSIVE CORTICAL LESIONS MADE IN INFANCY

By R. C. GONZALEZ, W. A. ROBERTS, and
M. E. BITTERMAN, Bryn Mawr College

Comparative studies of learning in fish and rat have revealed some striking differences between them: the fish fails to show progressive improvement in experiments on habit-reversal under conditions analogous to those which yield marked improvement in the rat, and it matches in experiments on probability-learning under conditions analogous to those which yield maximizing in the rat.[1] How are these differences to be understood? Can both be traced to a single underlying difference in the functioning of fish and rat, or do the results for habit-reversal reflect one functional difference and the results for probability-learning another? How are the differences in behavior related to differences in brain-structure? To what extent, for example, is the cerebral cortex of the rat responsible for its progressive improvement in habit-reversal and for its failure to match in probability-learning?

Two series of experiments have been prompted by these questions, one on the behavior of normal animals intermediate in complexity between fish and rat, and a second on the behavior of rats surgically deprived of selected brain-tissues. The first series of experiments already has begun to provide some interesting answers. Work with the pigeon suggests that the two behavioral differences cannot be traced to a single difference in underlying function. If a common process were involved, any given animal would behave either like the fish or like the rat in both kinds of experiment, yet the pigeon improves (like the rat) in experiments on habit-reversal and matches (like the fish) in experiments on probability-learn-

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[1] M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this JOURNAL, 71, 1958, 94-110.

ing.[2] The same results also suggest that the cortex plays a different role in habit-reversal than it does in probability-learning. The experiments now to be reported—the earliest in the neurosurgical series—were designed to provide further information on the role of the cortex. Adult rats extensively decorticated in infancy were studied under conditions analogous to those which differentiate normal rat and fish.

Although there have been many studies of learning in brain-injured rats, nothing is known about the effect of cortical ablation on probability-learning, and almost nothing about its effect on reversal-learning.[3] We decided to begin with large lesions in order to maximize the likelihood of finding an effect if it existed, and then later to do whatever more analytical experiments the results might seem to warrant. We decided to begin with lesions made in infancy for several reasons: first, the early lesions would produce *S*s which, having developed with reduced amounts of cortical tissue, would be more comparable to some of the simpler submammalian vertebrates; secondly, making the lesions early would maximize the time available for post-operative recovery; third, extensive bilateral ablation could be accomplished much more easily, more rapidly, and more safely in young than in mature rats. The early lesions were decided upon despite the widespread impression, produced by the early work of Tsang,[4] that cortical destruction in infancy has little effect on the learning of adult rats; some more recent results suggest that Tsang's measure was insufficiently sensitive.[5]

## VISUAL PROBLEMS

In the experiments to which we now turn, visual discrimination-reversal and visual probability-learning were studied in normal and operated rats. Although the comparison of normal rat and fish in visual problems has thus far been restricted to problems of the simultaneous type, the present work was done both with simultaneous and successive problems out of an independent interest in the relation between the two. One group of normals and one of operates were trained in simultaneous problems; another group of normals and another of operates were trained in successive problems.

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[2] D. H. Bullock and M. E. Bitterman, Habit reversal in the pigeon. *J. comp. physiol. Psychol.*, 55, 1962, 958-962; Bullock and Bitterman, Probability-matching in the pigeon, this JOURNAL, 75, 1962, 634-639.

[3] C. H. Bourke found clear evidence of progressive improvement in cortically ablated rats, but the lesions were quite small (1-6% of the total neocortical surface) and restricted to the anterior cortex (The effects of frontal lobe damage upon habit reversal in the white rat, *J. comp. physiol. Psychol.*, 47, 1954, 277-282).

[4] Y. C. Tsang, Maze learning in rats hemidecorticated in infancy, *ibid.*, 24, 1937, 221-248.

[5] H. C. Lansdell, Effect of brain damage on intelligence in rats, *J. comp. physiol. Psychol.*, 46, 1953, 461-464; C. J. Smith, Mass action and early environment in the rat, *ibid.*, 52, 1959, 154-156.

*Subjects.* The *S*s were 20 albino rats of both sexes—10 operates and 10 normals—bred in the laboratory from stock of the CF-Nelson strain. Operations actually were performed on 14 animals, but two died during operation and two more were discarded from consideration after histological examination revealed subcortical damage.

*Surgical and histological procedures.* The operations were performed under deep ether anesthesia with antiseptic technique when the animals were 15-17 days old. Cranial flaps were prepared by making two parallel longitudinal incisions—each about 1.5 mm. away from the midline, and extending from about 1 mm. in front of the posterior transverse sinus to about 1 mm. back of the orbit—and then lateral incisions at the front and back ends of the longitudinal ones. Each flap was bent back to expose the brain for removal of tissue, and, after removal, it was adhered to the narrow strip of intact medial cranium by coagulating excess blood at the site of the incisions. The cortical tissue was removed by aspiration through fine glass tubing. The operates were returned to their mothers about an hour after surgery. Five days later, they were housed in individual cages in the main colony-room of the laboratory where they were maintained on a free-feeding schedule until the age of about 90 days. The normal controls were weaned at the same time and reared in the same way as the operates, but they were not subjected to surgery.

Upon completion of the experiment, the operates were sacrificed, and their brains were removed, frozen, sectioned at 40 micra, and stained (Weil). Every tenth section was mounted. The lesions were reconstructed by Lansdell's modification of Lashley's technique and then plotted on conventional Lashley-diagrams.[6]

*Apparatus.* The apparatus employed was a two-window Lashley-stand modified to eliminate punishment for errors. Narrow runways (2.25 in. wide) branched out from a small starting platform 9 in. away from the two windows, which were 5.5 in. square and 3.5 in. apart edge-to-edge. The response of *S* was to run from the starting platform to one of the windows and to push against the stimulus-card there displayed; the incorrect card was locked in place, but the correct card gave access to a feeding platform in the rear.

*Preliminary training.* When the animals were 90 days old, they were placed on a 24-hr. feeding schedule and reduced to 85% of their satiated bodily weights. After a two-week period of handling, the animals were adapted to the apparatus, being permitted to eat wet mash freely on the feeding platform. Then they were taught to run from the starting platform, first to open windows, and next to unfastened mid-gray cards. Manual guidance was used from the beginning to provide equal experience with both windows. At the conclusion of preliminary training, the 10 operates and the 10 normals each were divided into two groups of 5 *S*s—a simultaneous group and a successive group—matched for position-preferences and for adjustment to the experimental situation.

*Reversal-training.* Throughout the experiment proper, there were 20 massed trials per day. The simultaneous animals were confronted with one white and one black card on each trial, the lateral positions of the two cards being varied systematically from trial to trial in accordance with selected Gellermann-orders. The successive animals were confronted with two white cards on half of each day's trials and with two black cards on the remaining trials, presentation of the two pairs following the same Gellermann-orders as those employed for the simultaneous groups. Training was by the correction-method. Following an incorrect response, *S* was returned to the starting platform and permitted to make as many as three additional (repetitive) errors before being guided manually in the correct direction. Each trial terminated with a correct response.

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[6] Lansdell, *op. cit.,* 461-464.

The first stage of the experiment consisted of a series of 13 problems, an original problem (Reversal 0) and 12 reversals, each learned to the criterion of 17 out of 20 errorless trials in a single day. In Reversal 0, three of the simultaneous operates and three of the simultaneous normals were trained with the black card positive, and the remaining simultaneous animals with the white card positive; three of the successive operates and three of the successive normals were trained with black-right and white-left positive, and the remaining successive animals with black-left and white-right positive. On the day after any animal reached criterion, its training on the first reversal was begun, and so forth, with each animal proceeding at its own pace. For example, a simultaneous animal trained with black positive in Reversal 0 was trained with white positive in Reversal 1, black positive in Reversal 2, and so forth. Similarly, a successive animal for which black-right and white-left were positive in Reversal 0 was rewarded for going left to the black pair and right to the white pair in Reversal 1, and so forth.

On the day after each animal reached criterion on the twelfth reversal, the second stage of its training was begun. The procedure was identical with that of the first stage, except that each animal was reversed *daily* for 14 consecutive days (making a total of 26 reversals in all—12 to criterion and 14 daily).

*Probability-training.* The third stage of the experiment began on the day after the fourteenth daily reversal. Training on the problem which constituted that reversal was continued until *S* reached the criterion of 20 errorless trials in a single day. Then the reinforcement-ratio was shifted from 100:0 to 70:30; that is, the alternative previously rewarded on 100% of the trials now was reinforced on 70% of each day's trials, while the alternative previously unrewarded now was rewarded on 30% of each day's trials. (The distribution of reinforcements was random with the restriction that there must be at least one reinforcement for response to the 30% alternative in each block of five trials.) Specifically, for a simultaneous animal trained with black positive in the fourteenth daily reversal, the black card was positive on 14 of each day's 20 trials and the white was positive on the remaining trials. For a successive animal trained black-right and white-left positive in the fourteenth daily reversal, black-right was positive on seven of each day's 10 trials with the black pair and black-left positive on the remaining three, while white-left was positive on seven of the 10 trials with the white pair and white-right was positive on the remaining three. The 70:30 training was continued until each *S* had reached a stable level of performance, following which the reinforcement-ratio was changed to 50:50; that is, each alternative was reinforced equally often. Throughout this third stage of the experiment, guidance was substituted for correction: after an unreinforced response, *S* was returned to the starting platform and guided manually to the opposite window.

*Histological results.* A reconstruction of the smallest and of the largest lesion in each group of operates is shown in Fig. 1. For the simultaneous operates, the lesions ranged in size from 46.0-65.1% of the total surface of the neocortex, with a mean of 53.9%; for the successive operates, they ranged from 41.1-64.4%, with a mean of 51.6%. There was no apparent relationship between size of lesion and any measure of performance.

*Reversal-learning.* The criterion-reversal performance of the four groups is summarized in Fig. 2 in terms of the median number of initial errors to criterion in the original problem and in each reversal. In the simultaneous discrimination (left-hand portion of Fig. 2), the performance of the normals and operates was much the same to begin with—the median number of initial errors in Reversal 0 was 16 for the normals and 22 for the operates, with some overlap of the two distributions. In the subsequent reversals, the error-scores of the normals rose at first (to a maximum of 32 in Reversal 2) and then declined progressively in characteristic fashion (to a stable level of about eight errors per reversal). By contrast, the error-scores of the operates rose much more markedly and, what is more important, did not decline in subsequent reversals; there was no overlap of the two distributions in the first reversal or in any of the subsequent reversals. In the successive discrimination (right-hand portion of Fig. 2), a quite different pattern of results appeared. The normals and operates differed

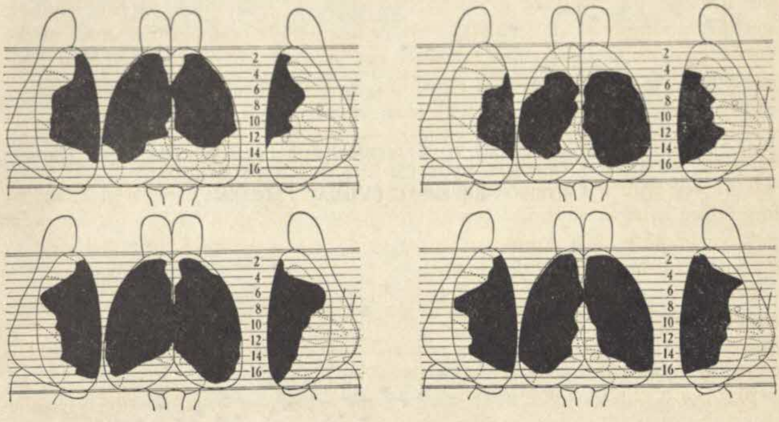

Fig. 1. Smallest and Largest Lesions in Each of the Two Groups of Operates Which Were Used in the Visual Experiments

Upper left, smallest simultaneous; lower left, largest simultaneous; upper right, smallest successive; lower right, largest successive

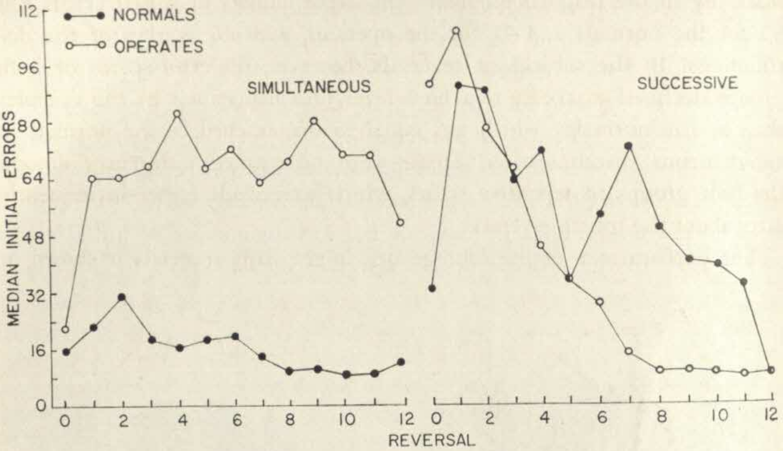

FIG. 2. MEDIAN INITIAL ERRORS TO CRITERION FOR EACH OF THE FOUR GROUPS IN THE ORIGINAL PROBLEM AND IN EACH SUBSEQUENT CRITERION-REVERSAL (VISUAL PROBLEMS)

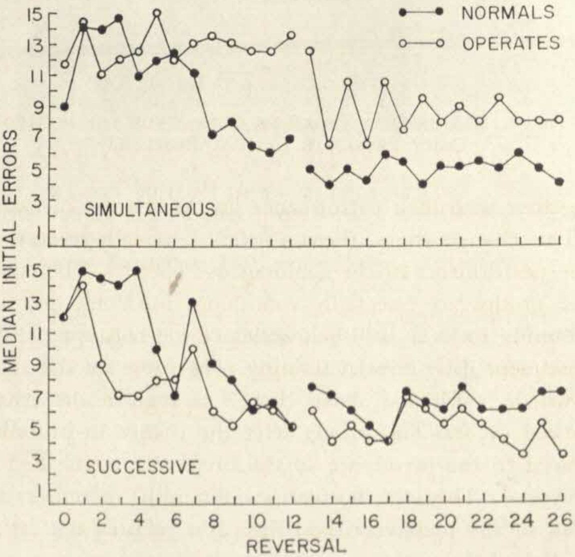

FIG. 3. MEDIAN INITIAL ERRORS MADE BY EACH OF THE FOUR GROUPS ON THE FIRST DAY OF THE ORIGINAL PROBLEM, ON THE FIRST DAY OF EACH CRITERION-REVERSAL, AND IN EACH OF THE SUBSEQUENT DAILY REVERSALS (VISUAL PROBLEMS)

markedly in the original problem—the mean number of initial errors was 32 for the normals and 91 for the operates, with no overlap of the distributions. In the subsequent reversals, however, the error-scores of both groups declined markedly to a final level (reached sooner by the operates than by the normals) which was equal to that reached by the normals in simultaneous discrimination. There were no marked differences among the four groups in repetitive errors, which were made rather infrequently after about the fourth reversal.

The performance of the four groups in the daily reversals is shown in Fig. 3, together with their performance on the first day of each criterion-reversal. The change from criterion-reversal to daily-reversal is evident only in the performance of the simultaneous operates. The curves for the other three groups are essentially continuous functions, declining in the criterion-training to levels well below chance and remaining at those levels in the subsequent daily-reversal training. The curve for the simultaneous-operates, which is flat and above chance throughout the criterion-stage, shows marked cyclical fluctuations after the change in procedure, which may be traced to the persistence of the preference established in the last criterion-reversal. Then the fluctuations diminish, reflecting the gradual washing out of the preference, and the curve remains at a level which is not substantially below chance.

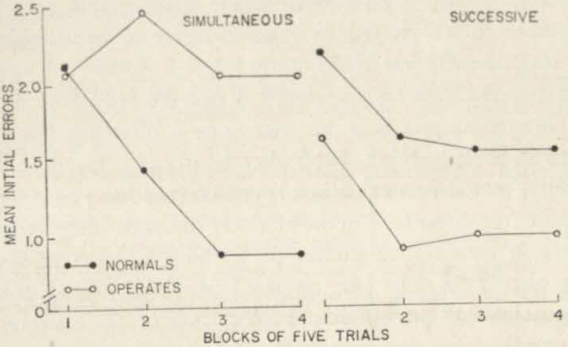

FIG. 4. WITHIN-DAYS LEARNING CURVES FOR THE POOLED DAILY REVERSALS (VISUAL PROBLEMS)

Within-days curves for the pooled daily reversals are shown in Fig. 4. The curve for the simultaneous operates is essentially flat, with as many errors made in the final block of five trials as in the first, while those for the remaining groups reflect appreciable improvement. The error-curve for the successive operates, consistently below that for the successive normals, attests the remarkable degree of flexibility displayed by these extensively decorticated animals in daily reversal.

How is the failure of progressive improvement in the simultaneous operates to be explained? It may be that the failure reflects nothing more than a quantitative deficit. The possibility must be considered that reversal-learning simply progresses more slowly in the operates than in the normals, and that with further training the operates, too, would have come to show improvement. A direct test of this hypothesis would be provided by a further experiment with a much longer series of reversals. The simultaneous-operate curves plotted in Figs. 2-4 give no reason, however, to think that improvement was in the offing.[7]

Another possibility to be considered is that the failure of improvement in the simultaneous operates is due, not to be a difference (quantitative or qualitative) in their learning, but rather to a change in some other function—sensory, motor, or motivational—which may have been produced by the operation. We come here upon precisely the same interpretive problem which arises in comparative studies of learning in different species,[8] and its solution also is the same (the operated rats being treated in all respects as a new species). If the different performance of the operates in simultaneous reversal is due, say, only to the fact that the operation reduces the incentive-value of the reward, an experiment with larger rewards should produce progressive improvement, and the role of other non-learning factors may be assessed in like fashion—that is, by systematic variation. If no *sensory* conditions can be found which will produce progressive improvement, then the failure of improvement is not simply a product of *sensory* deficit; and so forth. Until such experiments have been done, of course, the interpretation of the results for the simultaneous operates must remain in doubt, although the fact that we were led to look for those results by the failure of the pre-cortical fish to show progressive improvement lends considerable weight to the assumption that cortical ablation destroys the rat's capacity for improvement in reversal-learning.

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[7] The only suggestion of improvement which appears in any of the curves is the drop in the criterion-curve of Fig. 2 at Reversal 12, which can be traced to the performance of a single animal (the median animal of Reversals 10 and 11). The improvement in its performance relative to Reversals 10 and 11 appeared, furthermore, only after three days of training in Reversal 12. The number of initial errors made by this *S* on the first three days of the twelfth reversal were 15, 11, and 10, respectively; in the tenth reversal they were 14, 10, and 12; and in the eleventh, 12, 12, and 10.

[8] Bitterman, Toward a comparative psychology of learning, *Amer. Psychologist*, 15, 1960, 704-712.

If that assumption is correct, however, the progressive improvement shown by the successive operates remains to be explained. One possibility is that different processes—differentially dependent upon the integrity of the cerebral cortex—underlie improvement in simultaneous and successive reversal. There is good evidence, certainly, that qualitatively distinct processes operate in the initial solutions by normal rats of the two kinds of problem.[9] The simultaneous problem, which provides differential reinforcement in the visual but not in the spatial dimension, may be thought of as one in which the dominant process is stimulus-selection—the selection of one of two distinctive stimuli which compete for the common response of approach. The successive problem, which provides no differential reinforcement in either dimension (black is as often reinforced as white, and left is as often reinforced as right), may be thought of as one in which the dominant process is response-selection—the selection of one of two spatially distinctive and competing responses to each pair of stimuli functioning as a perceptual unit. Furthermore, there is evidence from work with brain-injured rats that retention of the two types of discrimination is affected differently by locus of cortical damage; simultaneous retention is impaired by posterior (striate) but not by anterior lesions, while successive retention is impaired either by posterior or by anterior lesions.[10] It would not be surprising, then, if the processes underlying progressive improvement in the reversal of the two kinds of discrimination differed in some important respects.

Another possibility to be considered is that the successive animals were not reversing at all in the usual meaning of the term. It is conceivable that what developed in these animals was, not an increased readiness to reverse specific response-tendencies to specific pairs of stimuli, but what might be called a *nonspecific discrimination of change*— a tendency to go on any trial to the side reinforced on the preceding trial when the stimuli remained unchanged, but to change sides on any trial when the stimuli were changed. (The use of correction—as in the present experiment—or guidance, which guarantees that each trial will end with a reinforced response, might be expected to favor this mode of solution.) The possibility of such a process first was suggested by the results of a previous comparison of simultaneous and successive discrimination-reversal in the normal rat.[11] In that experiment, with daily reversal from the outset of training, and with a more difficult discrimination (vertical and horizontal stripes) than the one used here, the performance of the successive group was far superior to that of the simultaneous group—the two error-curves began at about the chance-level and then declined progressively, the successive more rapidly than the simultaneous. In the present experiment, by contrast, the performance of the successive normals was far inferior to that of the simultaneous normals, which suggests that the change-discrimination develops less readily, if at all, when training on each reversal is carried far enough to produce strong specific preferences. The superiority of the successive operates to the successive normals in the present experiment is what might be expected on the assumption that cortical ablation impairs the capacity for reversal, and that the impairment of the capacity for reversal facilitates the development of other modes of solution. Discrimination of change does not develop in the *simultaneous* problem because the difference between *pairs* of stimuli is small relative to the difference between the *members* of each pair, which makes for a within-pairs or component organization rather than a between-pairs or configurational one.[12]

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[9] M. E. Bitterman, D. W. Tyler, and C. B. Elam, Simultaneous and successive discrimination under identical stimulating conditions, this JOURNAL, 68, 1955, 237-248.

[10] K. S. Lashley, *Brain Mechanisms and Intelligence*, 1929, 1-186; Robert Thompson, Retention of a brightness discrimination following neocortical damage in the rat, *J. comp. physiol. Psychol.*, 53, 1960, 212-215; Robert Thompson and C. F. Malin, The effect of neocortical lesions on retention of a successive brightness discrimination in rats, *ibid.*, 54, 1961, 326-328.

[11] R. C. Gonzalez and Bryan Shepp, Simultaneous and successive discrimination-reversal in the rat, this JOURNAL, 74, 1961, 584-589.

*Probability-learning.* The performance of the animals in probability-learning is summarized by Table I, which shows the percentage of trials in the last 10 days of 70:30 training on which the more frequently reinforced alternative was chosen, and the percentage of trials in the last 10 days of 50:50 training on which the same alternative was chosen.

TABLE I

PREFERENCES FOR THE MORE FREQUENTLY REINFORCED ALTERNATIVE OF THE 70:30 PHASE IN THE 70:30 AND IN THE SUBSEQUENT 50:50 TRAINING (VISUAL PROBLEMS)

| | Simultaneous problem | | | Successive problem | | |
|---|---|---|---|---|---|---|
| | *S* | 70:30 | 50:50 | *S* | 70:30 | 50:50 |
| Operates | O-1 | 68.0 | 49.5 | O-6 | 62.0 | 54.0 |
| | O-2 | 69.5 | 53.0 | O-7 | 66.5 | 56.0 |
| | O-3 | 71.5 | 47.0 | O-8 | 69.5 | 53.5 |
| | O-4 | 73.5 | 57.0 | O-9 | 71.5 | 61.0 |
| | O-5 | 76.0 | 62.0 | O-10 | 76.0 | 56.0 |
| Normals | N-1 | 64.5 | P* | N-6 | 83.5 | 81.0 |
| | N-2 | 79.0 | P | N-7 | 87.5 | P |
| | N-3 | 89.5 | 86.0 | N-8 | 88.0 | 88.0 |
| | N-4 | 90.0 | P | N-9 | 88.5 | P |
| | N-5 | 90.0 | 80.0 | N-10 | 89.5 | 86.5 |

* Response to one position on 90% or more of the trials.

The operates tended to match both in simultaneous and in successive discrimination, and at both ratios of reinforcement. In the 70:30 problem, the simultaneous operates came to choose the 70% stimulus on 71.7% of the trials; in the 50:50 problem, they came to choose the same stimulus on 53.7% of trials. In the 70:30 problem, the successive operates came to make the more frequently rewarded response to each pair on 69.0% of trials; in the 50:50 problem, the same response was made on 56.1% of trials. Analysis of the data for individual animals gave no sign of sequential dependencies such as 'positive-recency' (reinforcement-following) or 'negative-recency.' A one-sample runs-test also was made for the distribution of choices of each animal at each ratio of reinforcement, and in no case was it possible to reject the hypothesis of randomness at the 10% level of confidence.

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[12] Bitterman and Wodinsky, Simultaneous and successive discrimination, *Psychol. Rev.*, 60, 1953, 371-376.

Unlike the operates, the normal animals tended to maximize both in simultaneous and in successive discrimination when the ratio of reinforcement was 70:30. The single exception was simultaneous N-1, whose behavior was indistinguishable from that of the operates in the 70:30 problem. The relatively low percentage for simultaneous N-2—the only other normal *S* to come within 10% of matching—is a function merely of a slower drift toward maximizing in the choices of this animal; on each of the last three days of 70:30 training, it chose the 70% alternative on 90% of trials. In the 50:50 training, none of the 10 normals gave any indication of matching. Five developed strong position-preferences (at least 90% of responses to one side, as indicated by *P* in the Table), and the rest continued to show strong preference for the previous 70% alternative.

Random matching of the kind shown by the operates of this experiment has appeared previously only in submammalian forms.[13] Analogous experiments with the normal rat and the monkey usually have produced maximizing.[14] When, on occasion, a correspondence between choice-ratio and reinforcement-ratio has appeared in mammals, it has turned out to be a non-random one, based on systematic 'strategies' of one sort or another. Here again, then, the decorticated rat behaves like the fish under conditions patterned after those which differentiate fish and normal rat. Can this difference between operated and normal rat be attributed to a difference in some non-learning function (sensory, or motor, or motivational) produced by the operation? This question, which has been asked also about the difference between *fish* and normal rat, can be answered by the same kinds of experiment. Intensive studies of the normal rat must be made to determine whether there *are* certain experimental conditions in which it will show random matching. The range of conditions already studied is broad enough, however, to render the likelihood of positive results extremely small.

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[13] E. R. Behrend and M. E. Bitterman, Probability-matching in the fish, this JOURNAL, 74, 1961, 542-551; Bullock and Bitterman, *op. cit.,* 634-639; Nicholas Longo, Probability-learning and habit-reversal in the cockroach, this JOURNAL, 77, 1964, 29-41.

[14] Bitterman, Wodinsky, and Candland, *op. cit.,* 94-110; D. R. Meyer, The effects of differential probabilities of reinforcement on discrimination learning by monkeys, *J. comp. physiol. Psychol.,* 53, 1960, 173-175; W. A. Wilson, Jr., Supplementary report: Two-choice behavior of monkeys, *J. exp. Psychol.,* 59, 1960, 207-208.

## SPATIAL PROBLEMS

We turn now to some experiments on the role of the cortex in spatial (as contrasted with visual) reversal-learning and probability-learning. The differences between normal rat and fish which prompted the work with the decorticated rat hold for spatial as well as for visual problems, but there is some indication from work with intermediate species that the two modalities do not give the same results. For example, random-matching in the pigeon has appeared only in visual problems; in some preliminary work with spatial problems, the pigeon has shown nothing but a strong tendency to maximize.

The effect of cortical injury on spatial performance was studied first with an automated panel-pushing apparatus—a much more efficient apparatus than the modified Lashley-stand. When the results obtained proved to be different from those for visual problems in the Lashley-stand, further work was done with the Lashley-stand to separate the effects of modality and apparatus.

*Subjects.* The *S*s were 28 albino rats of both sexes—18 operates and 10 normals—bred in the laboratory from stock of the CF-Nelson strain. The rearing procedure for all *S*s, and the surgical and histological procedures for the operates, were the same as in the visual experiments.

*Apparatus.* The automated situation was a rectangular compartment of black Plexiglas (10.5 in. long, 8.5 in. wide, and 8.5 in. high) which was set into a ventilated, sound-attenuating picnic chest. The front wall of the compartment displayed two circular response-panels (2 in. in diameter, 3.5 in. apart on center, and 3 in. above the floor) made of clear Plexiglas. In a light-box behind each panel was a 15-w. lamp which served to illuminate the panel. Fixed to the back wall of the compartment (on the left-hand side) was a food-cup of clear Plexiglas into which Noyes pellets could be discharged by a Gerbrands feeder, and (on the right-hand side) a simple rat-lever. Two white Christmas-tree lamps in suitable housings were mounted on the back wall in such a way that the food-cup and lever could be illuminated separately. The chamber was connected to a programmer, which could be patched to generate a variety of spatial-learning experiments, and to an event-recorder which registered *S*'s response. In later stages of training, the modified Lashley-apparatus was employed once more, but with two gray cards.

*Preliminary training.* At the age of 90 days, the 22 animals (12 operates and 10 normals) to be trained in the automated situation were placed on a 24-hr. feeding schedule and reduced to 85% of their satiated bodily weights. After a two-week period of handling, the animals were adapted to the apparatus, being permitted to eat freely from the lighted food-cup for short periods of time. Then they were feeder-trained; each *S* was placed in the compartment, and, at irregular intervals, the food-cup was illuminated and a 0.045-gm. pellet was discharged into the cup. The period of time during which the feeder-light stayed on was decreased progressively until the pellet was being taken within 3 sec., the time to be allowed during the experiment proper. Then the animals were panel-trained. Each trial began with the illumination of one of the panels—the left on a random half of the trials, and the right on the other half; pushing the illuminated panel initiated a reinforcement-cycle (illumination of the food-cup and delivery of a pellet). When the panel-response was well established, lever-training was begun. Each trial now began with the onset of the lever-light; depression of the lever by the animal turned off the lever-light and illuminated one of the panels (sometimes the right and sometimes the left, in random order), and pushing the panel initiated a reinforcement-cycle. On the basis of their performance during this stage of training, the operates were divided into two matched groups of 6 *Ss* each—a Reversal and a Probability group—and the normals into two corresponding groups of 5 *Ss* each.

*Experimental training.* Throughout the experiment-proper, a correction-method was used, each trial terminating with a reinforced response. A trial began with the onset of the lever-light. Pressing the lever turned on both panel-lights, and response to either panel immediately turned off both panel-lights. If *S* had chosen the positive panel, a reinforcement-cycle was initiated, following which there was a 6-sec. intertrial interval in the darkness; if *S* had chosen the negative panel, there was a 6-sec. time-out in darkness, following which the lever-light came on, and response to the lever provided *S* with another opportunity to choose between the two illuminated panels. After four errors in succession, depression of the lever illuminated only the positive panel, and response to it initiated a reinforcement-cycle followed by an intertrial interval.

*Reversal training.* The Reversal animals were given 20 trials per day. In all, there were 21 problems, an original problem (Reversal 0) and 20 reversals, each learned to the criterion of 17 out of 20 errorless trials in a single day. The left panel was positive for all animals in Reversal 0 and on all subsequent even-numbered reversals, while the right panel was positive on the odd-numbered reversals.

*Probability-training.* In the first stage of their experimental training, the Probability animals were given 20 trials per day, with the left panel positive, until they met the criterion of 20 errorless trials in a single day. Thereafter, the number of trials was increased to 40 per day and the ratio of reinforcement was shifted—for three of the operates and three of the normals to 70:30 (left correct on 28 of each day's 40 trials), and for the remaining animals to 30:70 (left correct on 12 trials per day). The rules governing the distribution of reinforcements were the same as those used in the visual experiments. The 70:30 (and 30:70) training was continued for 31 days, following which the ratio was shifted to 50:50 for 34 days.

Upon the completion of this training, the operates were pretrained in the modified Lashley-stand, and then given 20 massed trials per day (with the left window positive) to the criterion of 20 errorless trials in a single day. After reaching criterion, they were shifted to the 70:30 problem until their performance stabilized, following which they were shifted to the 50:50 problem until their performance stabilized again. As in the work with the automated apparatus, the correction method was used, with manual guidance after four successive errors.

Six additional operated animals (23-28), without previous experimental experience, then were pretrained in the modified Lashley-apparatus. After 100:0 training (left positive) with two black cards, the reinforcement-ratio was changed, first to 70:30, and then to 50:50. There were 20 trials per day by the guidance method (no opportunity for repetitive error)—the method employed in the visual experiments.

*Results.* The lesions produced in the spatial operates were very much the same in locus and extent as those produced in the operates of the visual experiments. The lesions ranged in size from 47.3-68.2% of the total neocortical surface; the mean size was 56.8%.

In Fig. 5, the performance of the two Reversal groups is plotted in terms of median initial errors to criterion in the original problem and in each reversal. In this spatial experiment, the operates gave unmistakable evidence of improvement. Their error-curve is somewhat higher than that of the normals in the early reversals, but both curves decline progressively to the same final level of 2-3 errors per reversal (which means, incidentally, that most animals were meeting the reversal-criterion in a single day). Each animal in each group showed improvement.

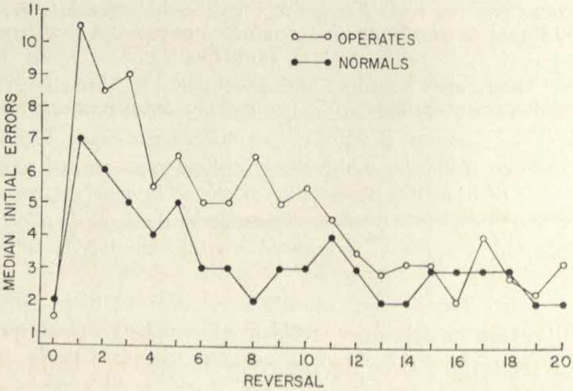

FIG. 5. MEDIAN INITIAL ERRORS TO CRITERION FOR NORMALS AND OPERATES IN A SPATIAL PROBLEM

In Table II, the performance of the Probability groups trained in the automated apparatus is summarized in terms of the percentage of initial response to the more frequently reinforced side of the 70:30 stage both in the last 10 days of 70:30 training and in the last 10 days of 50:50 training. The results for the spatial problem are quite different from those afready reported for the visual problem. In the 70:30 stage, the operates tended to maximize, more so if anything than the normal controls; in the 50:50 stage, their preferences varied widely, as did those of the normals. While an occasional correspondence between choice-ratio and reinforcement-ratio appears in the Table, no animal of either group shows such a correspondence at both ratios. In general, the spatial performance of operates and normals was very much the same.

Transferred to the modified Lashley-stand, the operates showed an even more pronounced tendency to maximize in 70:30 spatial training, no animal going to the 70% side on less than 90% of trials. These preferences were maintained in the subsequent 50:50 training, no animal going to the previous 70% side on less than 88% of trials. The six new operates (23-28) which were trained from the outset in the modified Lashley-apparatus, and with guidance (the procedure employed in the visual problems) instead of correction, behaved in similar fashion. In 70:30 training, none chose the more frequently reinforced alternative on less than 85% of trials, nor was there any substantial change in preference when the reinforcement-ratio was changed to 50:50. A comparison of the results obtained with the automated apparatus and those obtained with the modified Lashley-apparatus suggests that rats are somewhat more flexible in the former—that the spatial preferences which they develop in the course of training with inconsistent reinforcement are less strong—and it would be interesting to inquire further into the details of behavior in the automated apparatus. For present purposes, however, it is enough merely to note that neither situation yields any substantial indication of random matching in spatial problems.

TABLE II

PREFERENCES FOR THE MORE FREQUENTLY REINFORCED ALTERNATIVE OF THE 70:30 PHASE IN THE 70:30 AND IN THE SUBSEQUENT 50:50 TRAINING (SPATIAL PROBLEMS)

| Operates<br>Reinforcement-ratio | | | Normals<br>Reinforcement-ratio | | |
|---|---|---|---|---|---|
| *S* | 70:30 | 50:50 | *S* | 70:30 | 50:50 |
| O-17 | 69.0 | 7.1 | N-16 | 70.5 | 20.5 |
| O-18 | 79.5 | 19.5 | N-17 | 77.0 | 91.5 |
| O-19 | 90.3 | 75.3 | N-18 | 84.0 | 67.5 |
| O-20 | 92.5 | 64.3 | N-19 | 88.0 | 14.7 |
| O-21 | 93.3 | 91.5 | N-20 | 90.3 | 72.3 |
| O-22 | 94.3 | 45.5 | | | |

In general, then, extensively decorticated rats studied in experiments patterned after those which differentiate fish and normal rat give fish-like results in visual but not in spatial problems. It is interesting in this connection that the visual-spatial discrepancy has appeared also in some subsequent work with the pigeon; the pigeon, like the decorticated rat, shows random matching in simultaneous and successive visual problems, but tends strongly to maximize in spatial problems.[15] The discrepancy does not, however, appear in the habit-reversal data of the pigeon, as it does in those of the decorticated rat; the pigeon shows progressive improvement in simultaneous as well as in successive and in spatial reversals.[16] These relationships will perhaps be clarified by experiments with still other species intermediate between fish and rat, and by neurosurgical experiments of a more analytical kind.

## SUMMARY

Extensively decorticated rats and unoperated controls were studied in experiments on habit-reversal and probability-learning patterned after those which differentiate the normal rat and the (pre-cortical) fish. The work was done with spatial and with visual problems, both simultaneous and successive. In the spatial experiments, no differences were found between operates and normals: the operates, like the normals, showed progressive improvement in habit-reversal and tended to maximize in probability-learning. In the visual experiments, by contrast, marked differences appeared: the operates failed to show progressive improvement, at least in simultaneous habit-reversal, and showed random matching both in simultaneous and in successive probability-learning, while the normals behaved as they did in the spatial experiments. These results are considered in relation to those of analogous experiments with fish and pigeon.

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[15] Virgil Graf, D. H. Bullock, and M. E. Bitterman, Further experiments on probability-matching in the pigeon. *J. exp. Anal. Behav.,* 7, 1964, 151-157.

[16] Bullock and Bitterman, *op. cit.,* 958-962.

# SIZE-CONSTANCY AS DEPENDENT UPON ANGLE OF REGARD AND SPATIAL DIRECTION OF THE STIMULUS-OBJECT

By J. P. VAN DE GEER AND E. J. ZWAAN, Institute for Perception
RVO/TNO, Soesterberg, The Netherlands

In 1926 it was shown in experiments by Schur that the size of an object in vertical direction is underestimated, and that this tendency increases with increasing distance of the object.[1] Schur's conclusion was that size-constancy is more pronounced in the horizontal than in the vertical direction and that this difference in size-constancy ultimately gives rise to the well-known moon illusion (the illusion that the moon appears larger on the horizon than in the zenith). Schur, however, used *O*s in upright position only. Holway and Boring demonstrated that the position of *O* is an important factor in the moon illusion.[2] They found evidence, particularly, that the illusion depends upon the position of the eyes in *O*'s head. Any object looked at with the eyes turned upward, so that the direction of regard is along the eyebrows, would appear smaller than when it is looked at with the eyes in normal position, and this would be true when *O* is in the upright as well as in the supine position. The conclusion would then be that the spatial direction of the object is irrelevant; that the angle of regard in Schur's experiments caused the difference in size-constancy. The following experiments were undertaken to assess the role of both spatial direction and angle of regard as factors in determining size-constancy.

## EXPERIMENT I

The experimental set-up is shown schematically in Fig. 1. The stimulus-objects are two circular fields—windows of two boxes that are dimly and evenly illuminated from within. The boxes are placed upon a mobile table; both in *O*'s full view, with one 50 cm. nearer *O* than the other. The windows are variable in diameter by means of a diaphragm which can be manipulated by *E*. *O* has two switches ($S_1$, $S_2$) at his disposal, one for each

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* Received for publication January 15, 1963.

[1] Erna Schur, Mondtäuschung und Sehgrössenkonstanz, *Psychol. Forsch.*, 7, 1926, 44-81.

[2] A. H. Holway and E. G. Boring, The moon illusion and the angle of regard, this JOURNAL, 53, 1940, 109-116; The apparent size of the moon as a function of the angle of regard: Further experiments, this JOURNAL, 53, 1940, 537-553.

light-source. A surface mirror is placed 50 cm. above *O*'s head, at an angle of 45°.

*Procedure.* The experiment was performed in a long dark-room. *O*'s task was to compare a horizontal with a vertical stimulus in both the normal upright and supine positions. In the upright position, the horizontal stimulus was seen directly, straight ahead, whereas the vertical stimulus was seen through the mirror. In the supine position, the vertical stimulus was seen through the mirror in straight forward direction, whereas the horizontal stimulus could only be seen directly if *O* bent his head back and looked along his eyebrows. Care was taken that *O*'s eyes were always at the same location. For this purpose *O* had to sit on a low chair when in the normal position, and he had to lie upon a high table when in the supine position. *O* was instructed to indicate whether the horizontal object was smaller or larger than the vertical one; *E* changed the horizontal stimulus until is appeared equal to the vertical one. The stimuli were compared successively; each time *O* changed fixation he had to manipulate the switches to change the light.

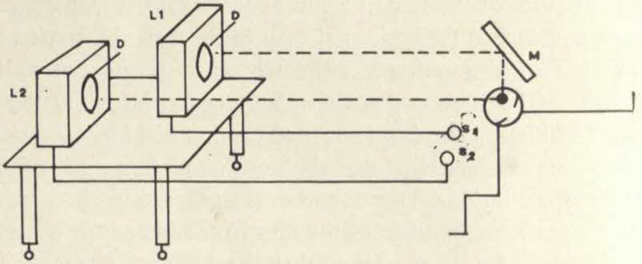

FIG. 1. SCHEMATIC DIAGRAM OF THE APPARATUS

The distance of the stimuli was varied at six values: 3, 4.8, 6, 13.5, 20, and 45 m. The size of the vertical standard object was simultaneously varied in such a way that this stimulus always subtended the same visual angle of 30′, except at the greatest distance. The six diameters were 6.8, 11.1, 13.6, 30.6, and 45 cm., respectively, for constant angle, and another 45 cm. for the greatest distance. The *O*s numbered 26; 22 with air-force ratings, the remaining 4 were staff members of the Institute.

*Results.* The results are given in Fig. 2. When in the supine position, the *O*s appear to succeed remarkably well in adjusting the variable size to the standard. When in the normal position, however, they tend to overestimate the size of the horizontal object, and, accordingly, they adjust the size of the horizontal object to a value which is smaller than the size of the vertical standard. The deviations from the standard are significant, at least to the 5% level from 4.8-m. distance upwards.

*Conclusion.* The obvious conclusion is that there are differences in size-judgment between the two conditions. Further comment will be given in the discussion of Experiment III.

## EXPERIMENT II

In the second experiment, the same equipment was used as in the first, with the following differences in procedure.

The standard object was fixed at 3-m. distance and 10-cm. diameter. The variable object was placed at varying distances: 5, 10, 15, 20, and 40 m. The *O*s had to make their judgments either in the normal or supine position. The stimulus-objects were both placed in the same spatial direction, which was either the horizontal direction, or a direction raised 60° in the medial plane, which will hereafter, for the sake of convenience, be called vertical. There are four experimental conditions, accordingly: (1) HP (stimulus-objects in the horizontal direction, *O* sits upright and looks at objects with eyes in primary position); (2) HD (objects in the horizontal direction and *O* looks at them while being in supine position with eyes turned upwards); (3) VP (objects in the vertical direction, *O* lies down and looks at them with eyes in primary position); and (4) VD (objects in vertical direction, to be looked at with raised eyes by *O* in normal sitting position).

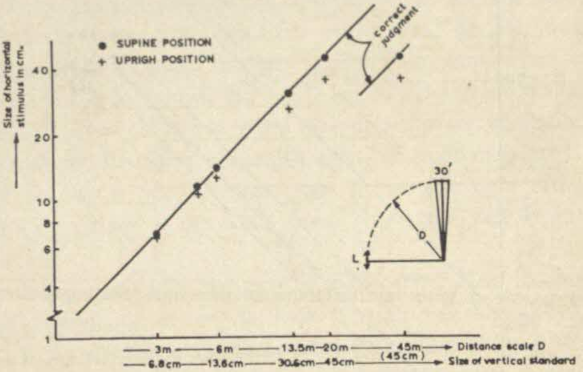

FIG. 2. SIZE OF VARIABLE HORIZONTAL STIMULUS-OBJECT JUDGED EQUAL TO VERTICAL STANDARD OBJECT UNDER 30′: TWO CONDITIONS
(Stimulus-object at 45 m. was 15′.)

The stimuli were compared successively; the more distant one being adjusted until its size appeared equal to that of the nearer one. The *O*s were 6 staff members of the Institute, all of them serving twice in all four conditions, divided between two sessions at one week intervals. Conditions were so balanced among the *O*s that order-effects were minimized.

*Results.* The results are shown in Fig. 3, after logarithmic transformations.

There are several reasons for this transformation. First, given linear coördinates, it is reasonable enough to assume that all four curves will include the same point ($D = 3$ m., $L = 10$ cm.), which means simply that two equal objects at the same distance are perceived as equal if they are in the same direction and at the same elevation. If, therefore, the curves are linear, they will have the form $(L-10) = b(D-3)$, where $b$ indicates the slope of the curve. If $O$ relies on size-constancy, the curve will be a horizontal line and $b$ is zero; the slope will be steeper to the extent $O$ deviates from size-constancy and approaches angular-constancy in his judgments. The value of $b$, therefore, is an indication for the relative weight of size-constancy in the actual compromise between size—and angular-constancy. A size-constancy ratio can be derived from $b$, as is explained in the APPENDIX. If the common point ($D = 3$, $L = 10$) is taken as a new origin of the coördinates which are thereafter transformed to a logarithmic scale, differences in $b$ will be revealed as differences in the intercept of parallel lines. The transformation, therefore,

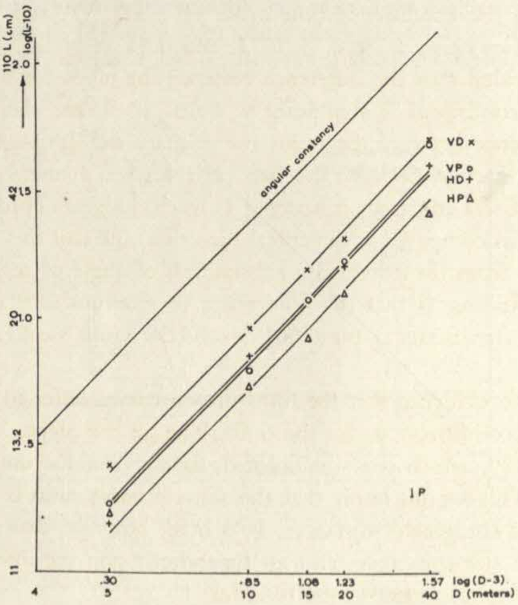

FIG. 3. SIZE 'L' OF STIMULUS-OBJECT AT VARIABLE DISTANCE JUDGED EQUAL TO STIMULUS-OBJECT OF 10 CM. AT 3 M. DISTANCE UNDER FOUR DIFFERENT CONDITIONS

(Differences on 'L'-axis are significant at 0.05 level if they are greater than $p$.)

simplifies the data so that they can be treated by analysis of variance. A second reason for the transformation is that the reliability of the absolute settings may be assumed to decrease at greater distances. A third reason may be added: if the original curves are straight lines, the transformed curves will be parallel lines under 45°. Any convexity in the original curves can be approximated from the value of $\gamma$ in the form $\log(L-10) = \gamma\log(D-3) + \log b$. If $\gamma > 1$, the curves are convex towards the axis; if $\gamma < 1$, they are concave.

Turning to the actual results now, we find that there is decreasing size-constancy for the conditions in the order HP, HD, VP, and VD; the size-constancy ratios being 0.79, 0.73, 0.72, and 0.64, respectively. Analysis of variance revealed that the difference between the ratios for horizontal and for vertical conditions is significant at 0.001 level, and the same applies to the difference between the ratios for primary and deviated angle of regard. No interaction between the two variables was found, hence we may conclude that size-constancy is better if *O* has to judge an object in horizontal direction as compared with vertical direction, and that this effect is valid independent from the effect, also substantial, of angle of regard.

Another finding is that the differences in size-constancy ratio between *O*s are also significant at the 0.001 level (the ratios vary from 0.847 to 0.686).

There is no evidence that the transformed curves differ in slope, either for the four conditions, or for the 6 *O*s. The general slope, $\lambda$, can be estimated at 1.029, which is a significant deviation from the unit-slope at the 0.05 level. This would imply that the size-constancy ratio is not constant, but decreases for greater distances. It is held, however, that for restricted distances the size-constancy ratio as dependent upon the intercepts of the transformed curves remains a useful index.

The tantamount interpretation of our main results is, then, that *O*s make optimal use of distance- and size-cues when they have to judge objects in horizontal direction and with primary angle of regard, and that the ability to use these cues is impaired if the object is in vertical direction or if the object has to be looked at with raised eyes. It might be that this state of affairs is related to learning conditions: the *O*s have experience mainly with situations where objects are in horizontal direction and can be looked at directly.

## EXPERIMENT III

According to Boring *et al.*, the moon illusion is absent for one-eyed persons, and would gradually disappear for binocular observers using one eye only.[3] Since, in their opinion, the moon illusion is dependent upon angle of regard, the implication is that with monocular viewing the effect of the angle of regard would disappear. They also found that the size-constancy ratio for monocular viewing conditions is substantially less than that for binocular viewing conditions. This finding is checked in our third experiment.

*Apparatus and procedure.* The same apparatus was employed as in the previous two experiments, with some modifications in procedure. Comparisons were made at 40 m. distance only, both for upright and supine bodily position. The horizontal standard had a diameter of 45 cm. The vertical object was, different from the procedure in the first experiments, a circle projected upon a screen; its size could be varied by adjusting the distance between projector and screen. *O* had to indicate when the vertical stimulus appeared subjectively equal to the horizontal standard. Each *O* was asked to match the objects binocularly, and immediately thereafter another time monocularly. Then one eye remained covered for 6 hr.; experimental settings were made again after 2, 4, and 6 hr. of monocular vision. Thereafter another binocular match was made. Four *O*s, staff members of the Institute, served in this experiment.

TABLE I

SIZES OF THE VERTICAL VARIABLE JUDGED EQUAL TO A HORIZONTAL STANDARD OF 45 CM. AT 40 M. UNDER UPRIGHT AND SUPINE VIEWING CONDITIONS

| *S*s | Upright | | | | Supine | | | |
|---|---|---|---|---|---|---|---|---|
| | *Z* | *M* | *S* | *K* | *Z* | *M* | *S* | *K* |
| Binocular before: | 58 | 58 | 57 | 51 | 46 | 45 | 46 | 44 |
| Monocular after: | | | | | | | | |
| 0 hr. | 58 | 57 | 48 | 50 | 47 | 50 | 42 | 42 |
| 2 hr. | 57 | 52 | 54 | 55 | 48 | 50 | 46 | 47 |
| 4 hr. | 59 | 62 | 55 | 61 | 50 | 48 | 48 | 61 |
| 6 hr. | 55 | 58 | 55 | 59 | 53 | 48 | 48 | 63 |
| Binocular after: | — | 54 | 51 | 64 | — | 41 | 44 | 49 |

*Results.* The results are given in Table I. An analysis of variance reveals that the difference between bodily positions is significant at 0.001-level, the horizontal object being overestimated considerably more in the upright than in the supine positions, as was found in Experiment I. The table shows, furthermore, that the underestimation of the vertical object increases with prolonged monocular vision. This tendency reaches significance, however, only for one *O*, for whom after 4 and 6 hr. of monocular vision the underestimation is greater than after 0 and 2 hr. at 0.05-level.

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[3] Holway and Boring, Determinants of apparent visual size with distance variant, this JOURNAL, 54, 1941, 21-37; D. W. Taylor and E. G. Boring, Apparent visual size as a function of distance for monocular observers, this JOURNAL, 55, 1942, 102-105.

A tentative interpretation of these findings is as follows. With binocular vision we found (Experiment II) that the size-constancy ratio is highest for horizontal direction and primary angle of regard; it follows that if an *O* is sitting upright, he will tend to underrate the size of a vertical object as compared with that of a horizontal object. To have a clearer undersanding of what happens, the reader might think along the following lines: a horizontal object far away is judged about equally large as a similar near object (high size-constancy ratio); a vertical object far away, however, is judged to be much smaller than a similar near object—then, if *O* has to compare two far objects, a horizontal one and a vertical one, objectively equal in size, the latter will be seen smaller than the first.

If *O* is in supine position, the vertical object will be underrated because it is vertical, and overrated because it is looked at with direct regard; the effect will be that these tendencies cancel one another, their effects being similar, as appeared in Experiment II, and size-judgment will tend to be veridical. This was found in Experiment III under supine conditions, and gives also the explanation of the results of Experiment I.

With prolonged monocular vision the moon illusion is found to disappear, according to the results of Boring *et al.*, and this is explained by these authors as due to the reduction of the effect of angle of regard. If this were true, one should expect that after longer monocular vision a vertical object will become underrated if it is looked at from the supine position. That is, in fact, our result for one *O*. On the other hand, one might argue that this interpretation implies that the underestimation should become less for the upright condition. This, however, is not a necessary implication once it is seen that there will be not only a reduction of the effect of angle of regard, but also a general decrease of size-constancy ratio in all directions for all angles (see APPENDIX). Stated otherwise: The amount of underestimation of the vertical object is dependent only upon the ratio of the respective constancy-indices, and even if the difference between these indices decreases, their ratio may increase if both indices fall at a lower level.

*Discussion.* Summarizing the results of the three experiments, we found that the size-constancy index depends upon angle of regard (Boring *et al.*) as well as upon objective spatial direction (Schur). Some discrepancies remain to be explained, however. Among them is the fact that Schur found a much higher difference between size-constancies in horizontal and vertical directions. The obvious explanation seems to be that Schur performed her experiments in the interior of spacious buildings which were not completely dark, so that her *O*s had additional cues. These cues will have been effective mainly in the horizontal direction, since vertical directions tend to be relatively empty in spacious buildings.

Our results also conflict with those of Boring *et al.*, who did not find any difference between size-judgments in horizontal and vertical direction. As regards the moon illusion, one explanation for this discrepancy might be that the spatial direction is not effective for objects at very great distances such as that of the moon. Another consideration is that Boring *et al.* did their experiments on the top platform of a high building, and it might be so that the *O*s did experience the horizontal direction as a direction into patently empty space, thereby refusing to project their normal expectations of sizes and distances along this empty dimension. Evidence that the direction of the object is important even for the moon illusion, can be found in their work. First the fact that "many persons who are permanently monocular and who are sophisticated enough to be interested in the illusion, insist that they perceive the horizon moon as much larger than the moon in elevation,"[4] albeit this claim could not be substantiated in the experimental proceedings. Secondly, there is the fact that, viewed from the primary angle of regard, the moon in the zenith is not judged large, whereas the horizon moon evokes exclamations of surprise. Boring *et al.* give an explanation of this fact in terms of the presence of a frame of reference made up by the visible objects at the horizon. They also claim the expected size to be intermediate between the apparent size of the moon on the horizon and in extreme elevation; that is why the horizon moon seems "surprisingly small" (*i.e.* smaller than the zenith moon) when *O* has his head half-inverted and looks between his legs (with the eyes lowered). The consequence of this view would be, however, that the horizon moon for *O* in the upright position with his head bent down, must also look surprisingly small, for here the expectation is also defeated in terms of a frame of reference. This does not, however, meet the facts. The size of the moon on the horizon shrinks indeed for most *O*s when they bend their heads down, but certainly not so much that it is judged smaller than the size of the zenith moon. On the contrary, the moon on the horizon remains large for an *O* in the upright position with head bent down.

Kaufman and Rock recently explained this result by the role of the distance-impression which is lessened if the scene is inverted.[5] These authors attempt to explain the moon illusion in terms of an apparent distance-theory. Since in our experiments these cues as such were not varied, no verdict can be given upon Kaufman and Rock's conclusion that distance-cues play the major role in the illusion. They did also try to find out if the angle of regard had any effect. Their finding that this factor is of negligible importance is, however, questionable, because this conclusion is based upon monocular viewing conditions inherent in the device used. Now it is clear from the work of Boring *et al.*, as well as from the results of Experiment III, that these conditions may reduce the effect of the angle of regard.[6]

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[4] Taylor and Boring, The moon illusion as a function of binocular regard, this JOURNAL, 55, 1942, 189-201.

[5] Lloyd Kaufman and Irvin Rock, The moon illusion, *Scient. Amer.*, 207, 1962 (No. 1), 120-130.

A suggested interpretation of our results is that man has to learn to use cues for sizes- and distance-perception, and that he achieves this mainly in situations where he is upright and looks straight ahead, or, at least, where he does not look with extreme angles of regard. The learning is generalized to situations of the same sort, to the effect that *O* makes less optimal use of cues when he is looking with extreme angles of regard, or when he finds himself viewing in unusual spatial direction. The first may result from unreliable stereoscopic vision once the eyes are extremely elevated or lowered, which may be explained in terms of disturbance of correspondence between retinal areas by rotation of the eys. The argument of the unusual spatial direction implies that there is a close integration of bodily and visual cues.

## SUMMARY

Three experiments were set up to assess the influence of both the angle of regard and the spatial direction of a stimulus-object upon the size-constancy ratio at distances up to 40 m. It was shown that both factors have an effect in the sense that the normal, straight forward, regard and the horizontal direction yield a higher size-constancy ratio than, respectively, the elevated regard and the vertical direction.

Prolonged monocular viewing conditions decrease the effect of the angle of regard.

The suggestion is made that the use of size- and distance-cues is learned mainly in situations where *O* looks straight ahead at objects in a horizontal plane, and that this learning is generalized to situations of the same sort. When *O* is looking with extreme angle of regard, or finds himself viewing in unusual spatial direction, he makes less optimal use of the cues available.

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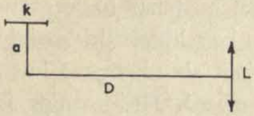
[6] A further discussion of the role of spatial direction in the illusion is given by E. J. Zwaan, The moon illusion, *Report IZF, 1958 (No. 10)*, Institute for Perception RVO/TNO, Soesterberg, The Netherlands.

## APPENDIX

*An index for relative size-constancy.* *O*'s task in Experiment II is schematically indicated in Fig. 4. The size, L, of a disk at varying distance, D, should be adjusted until it looks equal to the size, k, of a standard disk at fixed distance, a. If the adjustments were dictated by size-constancy, L should be made equal to k for each value of D. If, however, angular constancy is the rule, L should increase proportionally with D. This is pictured in Fig. 5. Empirical adjustments will supposedly lie somewhere between the extremes of size-constancy and angular-constancy, and we shall assume that the empirical data can be matched with straight lines. What we want, then, is an index for the relative degree of size-constancy. Such an index can be derived from the ratio between the slopes of the empirical line and the angular-constancy line. The slope of the latter is $\tan \psi = k/a$; the slope of the former is $\tan \gamma$, say. A size-constancy index R can be defined as $R = 1 - \tan \gamma / \tan \psi = 1 - a \tan \gamma / k$. R will have unit value in the case of perfect size-constancy, and zero value in the case of perfect angular-constancy. This index R is used in the presentation of the results of Experiment II. Holway and Boring used a similar index. Their experimental procedure, however, was slightly different from that in Experiment II, and it will be shown that this difference has a bearing upon the properties of the index of size-constancy. Their *O*s were asked to adjust the size k of a disk at constant distance a, until it looked equal to the given size L of a disk at varying distance D, where the angle subtended by L was held constant. This is schematically pictured in Fig. 6. A graph of the adjustment under this condition would look like that in Fig. 7, where k is plotted against D. It will be noted that this graph is different from that in Fig. 5, in that angular-constancy now appears as a horizontal line (at intercept $k = 2a \tan \alpha$), whereas size-constancy is given by a line with a certain slope ($\tan \psi = 2 \tan \alpha$). For empirical data, assumed to be on a straight line, the index of size-constancy was defined as $\tan \gamma / \tan \psi$, where $\tan \gamma$ is the slope of the empirical line. This reduces to $R^* = \tan \gamma / \tan \psi = \tan \gamma / 2 \tan \alpha$.



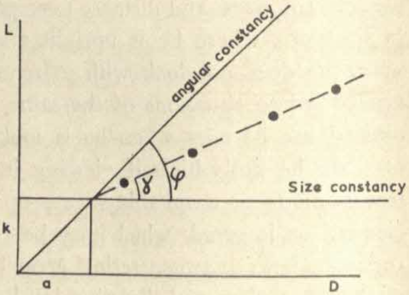
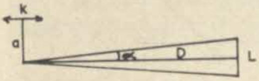


FIG. 4. SCHEMATICAL REPRESENTATION OF EXPERIMENT II
(The values 'a' and 'k' are constant; 'D' is varied by *E*; 'L' is adjusted by *O*.)

FIG. 5. GRAPH OF HYPOTHETICAL RESULTS OF EXPERIMENT II

The question arises whether R and R* are interchangeable. For both indices the unit value represents perfect size-constancy, and perfect angular-constancy is indicated by zero. For values different from 1 or O, however, the relation is not so simple. For instance, in the Holway and Boring graph a straight line with constancy index R* can be expressed as $k = 2 \tan \alpha[DR^* + (1-R^*)a]$. Since $L = 2D \tan \alpha$, this relationship can be translated into $L = k/[R^* + (a/D)\ (1-R^*)]$, and this function can be plotted in the type of graph corresponding to our Experiment II, as is done in Fig. 8. What we find is *not* a straight line, but a curve with concave bend and a horizontal asymptote at $L = k/R^*$ for $D \to$. It follows that R and R* do not express the same state of affairs. Conversely, it can be shown that a straight line with size-constancy ratio R in Fig. 5 will appear in the Holway and



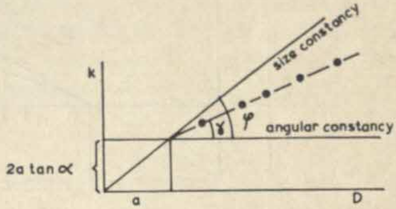
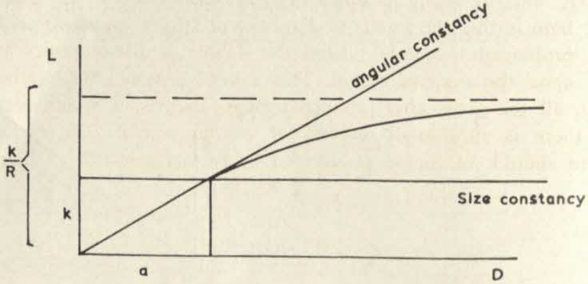


FIG. 6. SCHEMATICAL REPRESENTATION OF HOLWAY-BORING PROCEDURE (The values 'a' and 'α' are constant; 'L' = 2 'D' tan 'α'; 'D' varied by *E;* 'k' is adjusted by *O*.)

FIG. 7. GRAPH OF HYPOTHETICAL RESULTS OF HOLWAY AND BORING

Boring picture as a function $k = (2D \tan \alpha)/[R + (D/a)(1-R)]$, which is again a curve with concave bend and a horizontal asymptote, now at $k = (2a \tan \alpha)/(1-R)$, as illustrated in Fig. 9. The obvious question is: What are the relative merits of R and R* Which index has the best match with empirical data? The curve plotted in Fig. 8 is not very likely for two reasons. First, the empirical lines found in Experiment II show a convex bend rather than a concave one. Secondly, the curve in Fig. 8 implies that at very great distances there is a tendency towards a sort of delayed size-constancy, in the sense that a near object will be seen subjectively equal to a very far object if the size of the first is a constant fraction of the size of the latter. This implication is clearly against all evidence. On the other hand, Fig. 9 gives a reasonable picture, for two reasons again. First, some of Holway and Boring *O*s (including E.G.B.) indeed do show a concave function. Secondly, at high distances, Fig. 9 indicates a delayed tendency towards angular-constancy, which is reasonable enough. For these reasons, R should be prefered to R*.

*Prediction of size-comparison from constancy-ratios.* One may wonder whether the results of Experiment III could be predicted from those of Experiment II, or, generally, whether the point of subjective equality for equidistant objects in different directions can be predicted from constancy-ratios for those directions. Suppose an object with size $L_1$ at distance D is perceived in a certain direction for which the size-constancy ratio $R_1$ is valid. Suppose further that an object with size $L_2$ is perceived subjectively equal to the first object; $L_2$ is at the same distance, but the ratio for its direction is $R_2$. To understand the relationships involved, it is necessary to postulate an imaginary object at distance d. That is, $L_1$ is evaluated in terms of a reference object at short distance d, and the same applies to the evaluation of $L_2$. $L_1$ and $L_2$ are then judged if their respective reference sizes at distance d are judged equal. It can be shown that $L_2/L_1 = [(D - d)R_1 + d]/[(D - d)R_2 + d]$, or, if $d \ll D$, that $L_2/L_1 = R_1/R_2$. From Experiment II the various size-constancy ratios are estimated, HP: 0.79, HD: 0.73, VP: 0.72, and VD: 0.64. Supposing that these values are valid for Experiment III, too, we can predict the outcomes



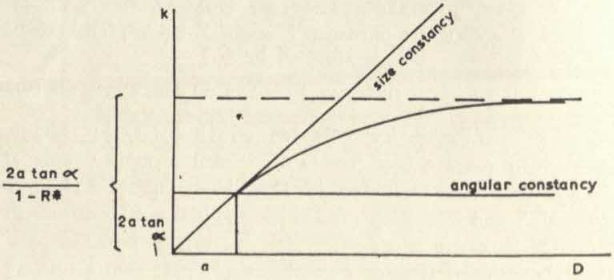


FIG. 8. HYPOTHETICAL RESULTS WITH CONSTANT SIZE-CONSTANCY RATIO R* FROM HOLWAY AND BORING PLOTTED IN GRAPH OF EXPERIMENT II

FIG. 9. HYPOTHETICAL RESULTS WITH CONSTANT SIZE-CONSTANCY RATIO R FROM EXPERIMENT II PLOTTED IN GRAPH OF HOLWAY AND BORING

of the binocular comparisons: Binocular Upright position $L_2 = 45\ (0.79)/(0.64) = 56$, and Binocular Supine position $L_2 = 45\ (0.73)/(0.72) = 45.5$. These predictions are in close agreement with the factual results, which are 56 and 45.25 respectively. It is also possible to estimate the value of 'd', if the complete formula is used and not the approximation, 'd' is found to be about 40 cm, a result not without some interest by itself.

Now for the monocular data. The hypothesis is, that with prolonged monocular adaptation the value of R will decrease. Particularly: for the conditions with primary angle of regard, R will decrease so as to become much more equal to the value for conditions with deviated angle of regard. Since the decrease of R is not known, the only thing one can do is to estimate values which are consistent with the results of Experiment III after 4 and 6 hr. of monocular vision. Such values are:

R in condition HP was 0.79, becomes 0.46; a decrease of 0.33.
R " " HD " 0.73, " 0.35; " " " 0.28.
R " " VP " 0.72, " 0.39; " " " 0.33.
R " " VD " 0.64, " 0.36; " " " 0.28.

These values predict for monocular comparison upright position $L_2 = 58$ (real value 58.0), and for supine position $L_2 = 52$ (real value 52.4). The main point of interest here is that the results of Experiment III are consistent with the theory that after prolonged monocular vision the degree of size-constancy is no longer dependent upon the angle of regard. That a vertical stimulus-object becomes more underrated, all the same, after prolonged monocular vision, would result from the fact that there is an over-all decrease of constancy-ratios. The validity of these assumptions should, of course, be decided by further research.

# SEMANTIC POWER MEASURED THROUGH THE INTERFERENCE OF WORDS WITH COLOR-NAMING[*]

By GEORGE S. KLEIN, New York University

Show the words 'red,' 'green,' 'yellow,' and 'blue,' printed in colored inks but in incongruent combinations of color and word, *e.g.* the *word* 'red' printed in the *color* yellow, the *word* 'yellow' in the *color* blue, and so on. The *S*s are to name the colors (of the inks) as quickly as possible, ignoring the words. It is not easy to do. Invariably, the colors are harder to name than when they are shown in simple strips uncomplicated by words. The phenomenon was noticed by Jaensch, and was first reported in this country by Stroop.[1]

To say that the word interferes with the naming of the color is a fair reflection of the *S*'s experience. Volume of voice goes up; reading falters; now and then the words break through abortively; and there are embarrassed giggles. These and other signs of strain and effort are common. The sources of the word's power to interfere with color-naming and the events involved in the interference itself have not received much attention, and are the main concerns of the present study. Published reports of the phenomenon have all employed the single condition described—colors in the form of incongruent color-names.[2] None has tried to manipulate the interference-effect by varying the verbal text in which the colors are embedded.

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[*] Received for publication August 14, 1963.

[1] Erich Jaensch *et al., Grundformen menschlichen Seins,* 1929, 383-392; J. R. Stroop, Studies of interference in serial verbal reactions, *J. exp. Psychol.,* 18, 1935, 643-661.

[2] Stroop, Factors affecting speed in serial verbal reactions, *Psychol. Monogr.,* 50, 1938, 38-48; G. W. Doten, The effects of rest periods on interference of a well-established habit, *J. exp. Psychol.,* 49, 1955, 401-406; L. L. Thurstone, *A Factorial Study of Perception,* 1944, 76-78; G. J. W. Smith and G. S. Klein, Cognitive controls in serial behavior patterns, *J. Pers.,* 22, 1953, 188-213; Klein, Need and regulation, in M. R. Jones (ed.), *Nebraska Symposium on Motivation,* 1954, 224-274; D. M. Broverman and R. S. Lazarus, Individual differences in task performance under conditions of cognitive interference, *J. Pers.,* 26, 1958, 94-105; G. J. W. Smith and G. E. Nyman, Psychopathological behavior in a serial experiment: Investigations of neurotic, psychotic, psychopathic, and normal subjects, *Lund Universitets Arsskrift,* N. F., Avd. 2, 56, 1959, No. 5, 3-73; Riley Gardner, P. S. Holzman, G. S. Klein, H. B. Linton, and D. P. Spence, Cognitive control: A study of individual consistencies in cognitive behavior, *Psychol. Issues,* 1, 1959, No. 4; Enoch Calloway and R. I. Band, Some pharmacological effects of atropine, *A. M. A. Arch. neurol. Psychiat.,* 79, 1958, 91-102; R. R. Holt, Cognitive controls and primary processes, *J. Psychol. Res.,* IV, 1960, 1-8.

Such variations of verbal text are a reasonable first step in exploring the bases of this interesting phenomenon. It is surely important that the color-names and the irrelevant color-words involve identical motor-responses. Consider the basic features of the task: (a) The critical response is a vocal linguistic one. It is not enough to see the color: it must be named aloud; (b) Two motor-linguistic responses are implicated in the word-color unit—that involved in vocalizing the color-name and that associated with reading the color-word; (c) Only one of these two responses must prevail—calling out the color-name. It is reasonable to assume that the required response can be obstructed by the arousal of a motor-response that is incompatible with it. Since one cannot say two words at once, the greater the tendency to read the word, the harder it would be to name the color. The critical question, then, is what will determine the evocative strength of the printed word, *i.e.* arouse the associated motor-response in competitive intensity? Possibly the word's semantic structure—its meaning and relatedness to the color-naming response—is important. Were word and color-names semantically distant, perhaps the effect would be less. Would it disappear altogether?

## EXPERIMENT I: VARIATIONS OF WORD-CONTEXT

The first experiment investigated the effects of verbal texts varying in their relationship to the colors. Six conditions were used. In each condition, the verbal text consisted of items typed in the colors red, green, yellow, and blue.

*Procedure.* In Condition A, the items were nonsense-syllables (*hjh, evgjc, bhdr, gsxrq*); in Condition B, they were rare English words (*sol, helot, eft, abjure*); in Condition C, they were common English words not closely associated with the color-names either in meaning or in response-class (*put, heart, take, friend*); in Condition D, they were words that are not themselves color-names but implicate the colors in their meaning (*lemon, grass, fire, sky*) and were presented in incongruent combination with the colors; in Condition E they were different words of the same response-class (*tan, purple, grey, black*). Condition F was the 'standard' condition: the words were the same as the color-names, but presented in incongruent combinations of color and word.[3]

Nonsense-syllables should provoke less interference with the color-naming response than actual words; commonly used words should be more interfering than rarely used words; word-meanings that implicate the colors should be more interfering than simply meaningful but unrelated words; words referring directly to color should be more interfering than the more connotatively related words; greatest of all should be the interference of words that are themselves the names of the printed color-names.

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[3] The words have the following frequency of usage-designations in the Thorndike-Lorge word count (E. L. Thorndike and Irving Lorge, *The Teacher's Word Book of 30,000 Words*, 1944.): (AA): put, take, heart, friend, sky, fire, grass, gray, black, red, blue, green, yellow; (1): sol, eft, helot, abjure; (37): purple; (27): lemon; (14): tan.

Separate sheets were made up of the six types of word-color combinations. The pages of the stimulus-words were comparable in the arrangement and spacing of the word-color units and in the number of letters comprising each unit. No one color appeared twice in succession on a page. There were always 10 word-color units to a line, 80 units in all to the page. A practice-line preceded the timed reading of the page.

The *Ss* were tested individually.[4] First, *S* read aloud a 'warm-up' page of the color-names printed in black type. He then read a page of 'colors alone,' *i.e.* he named the colors of the ink in which rows of asterisks were printed. The experimental page—colors in the context of letters and words—followed. For the experimental pages, instructions emphasized speed and accuracy and told the *S* to "ignore the words."[5] A specially-prepared scoring page enabled *E* to follow *S* word-for-word and to record abortive errors and other reading faults. Reading time was recorded every two lines. Only reading time for the page as a whole, however, will be considered here, analyzed in relation to the *S*'s reading time of colors alone taken as a baseline.

TABLE I

COLOR-NAMING TIMES (SEC.) AND INTERFERENCE-INCREMENT SCORES FOR COLOR-WORD CONFLICT CONDITIONS

| | A nonsense syllables | | B rare words | | C common words | | D color-related meanings | | E color-names distant | | F color-names close | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Colors alone (1) | 44.38 | 5.55 | 43.60 | 5.70 | 44.16 | 5.99 | 43.94 | 5.72 | 44.03 | 5.44 | 44.03 | 6.15 |
| Conflict page (2) | 49.68 | 5.33 | 51.18 | 7.12 | 55.95 | 7.75 | 59.43 | 8.02 | 62.17 | 7.17 | 81.47 | 12.11 |
| Increment (2–1) | 5.30 | 3.84 | 7.58 | 4.21 | 11.79 | 3.60 | 15.49 | 5.05 | 18.14 | 5.71 | 37.43 | 7.68 |

Ninety men and women, undergraduate and graduate students, ranging in age from 19 to 28 yr., were distributed among the six conditions, 15 to each group (8 men, 7 women). Previous studies of the standard condition have found reading time on the conflict-page to be highly correlated with color-naming in the colors-alone condition (asterisk rows).[6] Therefore, in assigning *Ss* to the different experimental pages, care was taken that the six finally constituted groups be equivalent in respect to reading time of colors-alone.

*Results.* Table I gives the means and standard deviations of the reading times in the six conditions. In all conditions, reading time was significantly slower on the conflict-pages than on colors-alone. The measure used in assessing degree of interference was the *S*'s reading time of the conflict-page minus his reading time for colors-alone. As the words became more meaningful and more closely related to colors, the interference-increments became increasingly larger. The trend is pictured in Fig. 1, which plots the means of the interference-increments of the six verbal contexts. An analysis of variance of these increment-scores disclosed highly significant differences among conditions ($F = 73.92$, $df = 5$, $p < 0.001$). Differences among the means of the conditions were assessed by Duncan's multiple-range test.[7] The difference between A (nonsense-syllables) and B (rare words) is not

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[4] I am grateful to Stanley Grand, of the Research Center for Mental Health, New York University, for invaluable assistance in all phases of the study, and to Peter Schiller, of the Massachusetts Institute of Technology, for his aid in testing the subjects. This investigation was supported in part by a Public Health Service research career program award, No. MH-06733, from The National Institutes of Health.

[5] The *Ss* were warned against devices that are sometimes resorted to as ways of reducing the irritating intrusions of the irrelevant words, *e.g.* squinting, pointing, sing-song reading.

[6] Riley Gardner *et al., op. cit.,* 57.

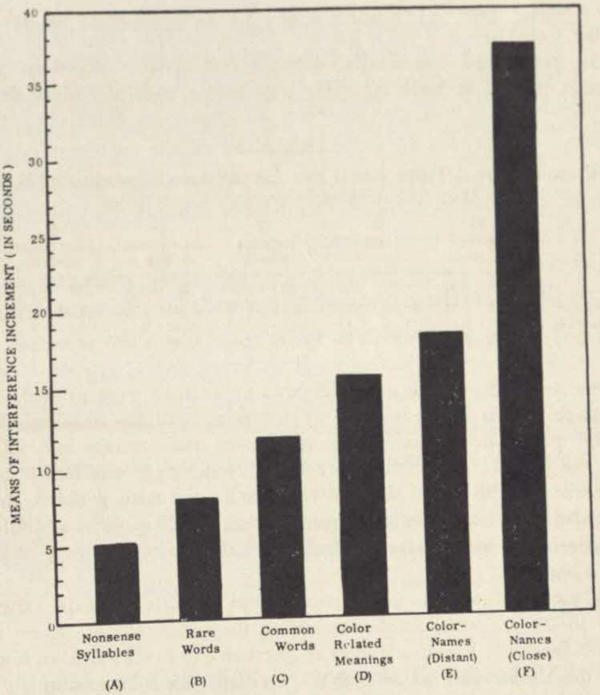

FIG. 1. INCREMENTS OF COLOR-NAMING TIME WITH DIFFERENT WORD-COLOR COMBINATIONS

(From base-line of *Ss*' reading time on colors-alone.)

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[7] A. L. Edwards, *Experimental Design in Psychological Research,* rev. ed., 1960, 136-139.

significant; between B and C (common words) the difference reaches the 5% level of significance; the difference between C and D (color-related meanings) reaches the 10% level; D and E (color-names, distant) are not significantly different; the difference between E and F (color-names, close) is significant at the 1% level. All other comparisons are significant beyond the 0.1% level, *e.g.* A and C, B and D, C and E.

Clearly, different attributes of the words differentially affect the color-naming response. Not only color-words have an interfering effect; a highly familiar word will be intrusive too, though in lesser degree. Even the arbitrary letter-combinations of the nonsense-syllables create a significant rise in interference over the naming of colors in asterisks ($t = 5.25$, $p < 0.001$). In general, the impeding effect of the words upon the relevant motor-response is governed by the relative meaningfulness of the words, and their relation, through meaning or through the linguistic motor-component, to the color-naming response.

While verbal text affects the ease of color-naming, this fact alone does not directly illumine the nature of the word's interference. The word's effect is in the delay it produces in naming the color. An adequate account of the interference would have to tell us something about the events that take place during the reading delay. Prominent among these events are *S*'s efforts to surmount the interference.

When two motor-responses are vying for the one response-channel, it is possible that reading delay is produced by an effort to *hold back* one of them. To make the appropriate motor-response, *S* perhaps has to expend effort momentarily to restrain the near-threshold, irrelevant response. It is this effort that may be reflected in the slowed reading time. A second experiment tested this hypothesis.

## EXPERIMENT II: EFFECTS OF RESPONSE-RELEASE

If the color-naming delay is indeed produced by the necessity of holding back a competing response, it should disappear when *S*s are allowed to say aloud both word and color-name.

*Procedure.* To test this hypothesis, the standard color-word conflict-page, containing 100 color-word units, was used with two variations of instructions. The *S*s in Condition I were instructed to read aloud as quickly as possible *both* the word and the color of each color-word unit, in that order. The *S*s in Condition II also read aloud the two words but in reverse order—first color, then word. It was expected that the double response would favor color-naming only in Condition I, for if holding back the word contributes to interference it would still operate in Condition II when *S* is permitted to release the word only after naming the color. Only when the word is allowed to come out first, and immediately, should interference in color-naming be reduced.

Each condition had 20 *Ss*. All the *Ss* also read the colors alone (100 units)—the first task of the test-session, as well as the conflict-page in its usual administration calling for a single response. In half of each group, the reading of the single response preceded the double-response condition; in the other half, the double-response condition was given before the standard. The final task of the session called for all *Ss* to read aloud as quickly as possible only the *words* of the conflict page.

*Results*. There are, first of all, convincing signs that the double response, word-then-color (Condition I), was the easier task. All *Ss* of Condition I, save one, said it *felt* easier than the standard, single-response task. Typical comments were: "The meaning disappeared and I could spend the time searching for the color-name." "Getting it out of your system, you can concentrate on the color." The situation was quite different in Condition II, where first color, then word, was read in the double response. All but one of the 20 *Ss* said it was harder than the single-response task.

TABLE II

FREQUENCY OF READING FAULTS IN SINGLE (STANDARD) AND DOUBLE-RESPONSE TASKS

| Task | I (Word-then-color) | | | | | | II (Color-then-word) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | given first | | given second | | total sample | | given first | | given second | | total sample | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Standard | 2.40 | 2.84 | 4.30 | 3.20 | 3.35 | 3.10 | 2.70 | 2.35 | 4.00 | 3.40 | 3.35 | 2.93 |
| Double response | .60 | .96 | .50 | 1.08 | .58 | 1.00 | 2.80 | 3.36 | 3.30 | 3.30 | 3.05 | 3.25 |

The relative frequencies of errors in the standard and double-response tasks, shown in Table II, bear out this impression of easier color-naming in the word-then-color condition. Errors refer to instances of misnamed colors—corrected or left uncorrected by *S*—in the course of reading. Such errors are fewer by far in the word-then-color condition than in the single-response task, whether the single-response task preceded or followed it. The errors were also much fewer than in Condition II (color-then-word); in Condition II, errors occurred in about the same frequency as on the single-response task. The results shown in Table II were subjected to an analysis of variance for repeated measurements. Task (single-response or double-response) is significant ($F = 7.81$, $df = 1/36$, $p < 0.01$), Condition (word-then-color or color-then-word) is significant ($F = 11.79, df = 1/36$, $p < 0.01$) as is the interaction of Condition by Task ($F = 5.08$, $df = 1/36$, $p < 0.05$).

There is also no question but that the double-response task of Condition II, color-then-word, took longer to read than word-then-color. Table III shows the means and *SD*s of reading times in Conditions I and II, the double-response times appearing in Row 5. Reading time was faster by far in the word-then-color condition ($t = 4.67$, $p < 0.005$). Clearly, reading felt easier and proceeded more smoothly and quickly in the word-then-color condition.

It may profit understanding of the effectiveness of Condition I to compare the efficiency of color-naming in the double-response and single-response tasks. Is there evidence that the color-response was emitted more *quickly,* as well as with fewer errors, in the double-response than in the single-response task? In the double response both vocalized responses are embedded in the total reading score. To compare the two tasks in respect to speed of color-response only, a subtraction procedure was used. *S*'s reading speed on *words alone* (the final reading task given all *S*s) was subtracted from his double-response time. The residual was assumed to be the time taken by *S* to name colors in the double-response condition, and this was compared with his single-response time.[8]

Note the nature of the null hypothesis implied by this procedure. It assumes that color-reading time, either in the single- or in the double-response condition, is the summed total of *S*'s reading times for colors *alone* and words *alone.* Therefore, there should be no difference between *S*'s single-response time and the combined total of his reading times for colors alone and words alone, nor should there be any difference between the *residual* color-naming time of the double-response task and the single-response time.

In the section of Table III giving the respective means and *SD*s of the various reading times of Condition I, the first fact to be noted is that there is indeed no significant difference between the *single-response* time (Row 4) and the combined total of colors-*alone* and words-*alone* (Row 3) ($t = 0.71$, $p < 0.50$). (It is an interesting ancillary finding that *S*'s reading time on the conflict-page in the standard condition can be fairly well predicted from knowing his reading times for colors alone and for words.) Between residual color-naming time of the double response (Row 6) and speed of the single response (Row 4), however, the difference is highly significant in favor of the double-response condition ($F = 69.58$, $df = 1/18$, $p < 0.005$).[9] All 20 *S*s, without exception, were *slower* in naming

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[8] By the same logic it would be possible to add *S*'s words-alone to his single response-time, and to compare this to his double response-time. The procedures are interchangeable and yield identical results.

[9] All *F*-tests refer to analyses of variance for *repeated* measures.

## TABLE III

### Reading Time in Seconds for Single ('Standard') and Double-Response Tasks

| | Condition I (Word-then-color) | | | | | | Condition II (Color-then-word) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | standard task given first | | standard task given second | | total sample | | standard task given first | | standard task given second | | total sample | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| (1) Colors alone | 66.54 | 14.43 | 65.72 | 13.73 | 66.13 | 13.71 | 57.31 | 9.60 | 60.12 | 10.59 | 58.72 | 9.94 |
| (2) Words alone | 40.80 | 8.17 | 39.06 | 4.67 | 39.93 | 6.53 | 40.80 | 5.37 | 42.04 | 4.68 | 41.42 | 4.95 |
| (3) Combined (1+2) | 107.34 | 21.11 | 104.78 | 17.34 | 106.06 | 18.84 | 98.11 | 13.18 | 102.16 | 12.04 | 100.14 | 12.46 |
| (4) Single ('Standard') response | 110.46 | 26.73 | 111.30 | 21.54 | 110.88 | 23.62 | 104.60 | 21.69 | 116.22 | 37.20 | 110.41 | 30.23 |
| (5) Double response | 121.08 | 22.74 | 123.54 | 23.24 | 122.31 | 22.41 | 184.10 | 63.51 | 175.22 | 35.15 | 179.66 | 50.16 |
| (6) Double words alone | 80.28 | 15.70 | 84.48 | 19.46 | 82.38 | 17.56 | 145.40* | 24.98* | 158.26* | 39.41* | 151.83* | 32.78* |

* Single+words alone (2+4).

colors in the single-response task; and color-naming in the standard task was slower whether or not it preceded or followed the double-response task. In the experience of the *S*s as well as in their performance, naming colors in the word-then-color sequence was easier to do than in the standard task.

What about the relative level of interference in the single- and double-response tasks of Condition II? Because *color* came first in this double-response sequence, the procedure of making single- and double-response times comparable was to *add* to *S*'s single-response time his reading time for words alone. This cumulated total, given in Row 6 in the second section of Table III, was then compared with the double-response time by analysis of variance for repeated measures. (Sequence of tasks—whether the double-response task precedes or follows the single-response task—is insignificant, as in Condition I.) The Task effect (single- or double-response) is highly significant ($F = 11.82$, $df = 1/18$, $p < 0.005$), with the double-response this time definitely the slower of the two. Thus, the color-then-word order of release actually presents more of an impediment to color-naming than is the case in the single-response task. It is clear that there is no advantage to color-naming in reading the word aloud in Condition II; the advantage occurs only in a double-response where the word is allowed release *before* the color-name.

## DISCUSSION

It is necessary to account for: (a) the power of a verbal context to produce difficulty in color-naming; (b) the events that make it possible to overcome this difficulty; (c) the reduction in difficulty that ensues from releasing first the word, then the color.

Interference from the word consists essentially in the disposition to *say* it. It is in this sense that we speak of 'competition' from the word.

The word's capacity to produce arousal of its motor-components we term its attention-catching or *attensive* power, to borrow a term from Titchener.[10] The greater this power, the more interference the word exerts—the stronger the competition it offers for dominance of the single motor-outlet. Experiment I thus showed that attensive power, in the context of a color-naming task, is a function of the meaningfulness of words, the semantic relation of word and color, and the specific relatedness of the linguistic motor-component of the word to color. Motor-antagonism increases the closer the linguistic motor aspect of the word comes to the color-naming response. Where the word-component is distant from the color-response, both in meaning and in response-class, the interference is correspondingly less.

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[10] E. B. Titchener, The term 'attensity,' this JOURNAL, 35, 1942, 156.

Reference to a word's structure implies, following Hebb,[11] that words are embedded in ideational schemata; a word's structure includes not only its motor-components but its links with associative networks. Therefore, the attensive power of a word-stimulus refers to a rise in excitatory level of this representational structure of the word, in part or whole, which includes reverberations to the word's motor-facilitations within the schema. Thus, the motor-component of the word may be aroused more or less indirectly through the activation of different components of the word's structure. In the color-word situation this would mean that activation, say, of the word's meaning produced a rise in activation of the vocal motor-component of the word, and this in turn created a condition of competition for the single-response channel.

From this perspective—that a word's structure embodies more than, but includes, motor-components—when it is said that the critical competition is between motor-responses, it is not meant that other components of the word's structure play no part. Their role is critical in the extent to which they lead to arousal of the motor components of the structure.

Pinpointing the source of interference in motor-antagonism takes us only part of the way in accounting for the slowed reading time. If the word activates a competitive motor-response, we have still to explain what makes it possible for the relevant response to win out, to dominate the other. The slowed reading can be viewed as reflecting not only the disruptive effect of the word but also the effort of overcoming it. We must then ask what this 'effort' consists of. The question is: What else is happening *during* the halts in reading to bring the *relevant* response up to its necessary level of activation?

It is proposed that the motor-antagonism makes it necessary for the *S* to *seek additional stimulation from the region of relevant perceptual information (color) in order to produce the appropriate motor-response (color-name).* The color-naming threshold is assumed to be a constant; reaching this threshold has been retarded by the arousal of a competing motor-response. To reach it, *S restimulates* himself with the color of the word-color combination. Increase of reading time reflects the required amount of restimulation, *i.e.* additional perceptual input from the color. This requirement will depend on the degree of arousal of the competitive response that is to be overcome, namely, the attensive power of the word. The greater the word's attensive power, the more restimulation is necessary.

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[11] D. O. Hebb, *The Organization of Behavior*, 1949, 105, 116-120.

The differences between the double-response conditions can be understood in these terms. First of all, reading a word, especially a common word, makes little demand on *S;* it is highly automatized and little stimulation is necessary to release it. Perceiving the word and speaking it are therefore practically simultaneous. Not so with color-naming. Naming a color always takes more time, and we may surmise the reason is that, between perception of color and its verbalization, a coding operation must intervene in which the color is assigned its correct name. Thus, groping for the right color-name is not an uncommon occurrence in reading colors-alone.

In the word-then-color double response, the easy release of the word makes it possible for the *S* to attend to the color even as he speaks the word. Thus, he readies the color response and brings it close to threshold. The restimulation necessary to produce the color-name in the double-response word-then-color is, therefore, only in the amount determined by how easily *S* can code colors with their proper word-signs. In the word-then-color condition, an easy response requiring little effort is released, permitting the *S* to shift his attention to the harder color-naming response. In Condition II (color-then-word), the very strength of the word which in Condition I made for easy release and, therefore, for uncomplicated color-naming, now interferes with the coding operation and impedes color-naming. Motor-antagonism is greater in Condition II, requiring increased stimulation from the color.

Several implications follow from the outcomes of the present experiments. First is the possibility that the color-word paradigm of interference (two responses competing for a single-response channel) is a useful means of assessing the attention-catching power of combinations of words and letters. The evocative power of a word would be indicated by its impeding effect upon the naming of color, this effect being given by the easily measured index of reading time. Physiognomic qualities of letter-combinations, relatively neutral and affect-laden words, relatively familiar and unfamiliar words, over-learned and weakly-learned words (a variable already explored by Rouse and Maas) may thus be assessed for their evocatice power.[12] The context of the colors may be pseudo-words and nonsense-syllables as well as real words. Variables can be compared in potency. Is a word's meaning more effective than the frequency of its usage or of its physiognomic properties? Or are meaning and frequency together the key to a word's power? Conditions of 'strength' or of word-structure can be explored in similar fashion. Will conditions that 'loosen' the meaningful connections of a word (such as the 'satiation' procedure reported by Lambert)[13] produce a lower interference-effect when the word appears in the color-word matrix? It is to these possibilities of exploring the semantic structure of a word through color-word competition that the title of this paper refers.

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[12] R. O. Rouse and J. B. Maas, Interference with color naming as a function of degree of practice on the color-bearing materials (unpublished manuscript).

The emphasis on restimulation in our account also suggests experimental possibilities. It should be possible to prime or ready, by prior practice, the relevant response-words (colors) and reduce the requirement of restimulation and thereby the interfering effects of words. Such manipulations, either of the attensive power of the words or of the availability of the relevant response through prior priming, should make it possible to increase or decrease the competitive effects of words upon color-naming. In effect, such manipulations would be affecting the amount of restimulation required to release the relevant motor-response.

## SUMMARY

Two experiments investigated the basis of interference in the Jaensch-Stroop phenomenon: the relative difficulty of naming colors when the actual colors are incongruently combined with the words signifying the colors, *e.g.* the *word* 'red' printed in the *color* 'yellow,' etc.

*(1)* The first experiment compared color-naming speeds when the same four colors (red, green, yellow, and blue) appeared in six different verbal contexts. Ninety *S*s were distributed among the six conditions, 15 to each group. Interference of the words with color-naming was least when the verbal text consisted of nonsense-syllables, increased somewhat when the verbal context contained actual though rare words, rose markedly with common words, and still more with words closely related to the color-responses in their meaning or in their direct reference to color; it was greatest of all in the standard condition (where the actual words were 'red,' 'green,' 'yellow,' and 'blue'). Thus, amount of reading delay depends on the relatedness of the vocal components of the word, the word's meaningfulness generally, and the color-specific meanings of the word, to the relevant color-response.

*(2)* If delay in color-naming is produced by the necessity of *holding*

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[13] W. W. Lambert and L. A. Jakobovits, Verbal satiation and changes in the intensity of meaning, *J. exp. Psychol.*, 60, 1960, 376-383.

*back* a competing response, it should disappear when *S*s are allowed to say aloud *both* the word and the color-name of a word-color unit. In a second experiment, two conditions of double-response were studied: 20 *S*s reading aloud word-then-color of a color-word unit in that order, and 20 *S*s reading each unit in the color-then-word order. When *S*s are allowed to say aloud both the word and the color in that order, color-naming is markedly improved. On the other hand, no benefit to color-naming is gained by reading aloud first the color and then the word of a word-color unit; it is, in fact, significantly worse in this condition.

*(3)* Two main determinants of the interference-effect and of the color-naming delay were proposed: (a) the *attensive* power of a word—*i.e.* the word's capacity, when the word is present in the color-context, to bring about a rise in activation of the vocal motor-component of the word's structure; (b) compensatory *restimulation* from the relevant part of the stimulus-field (in this case, color) which makes possible the release of the relevant motor-response and its domination of the vocal output-channel.

# RELATIONSHIP BETWEEN SUPERFICIAL AND DEEP SOMATIC THRESHOLDS OF PAIN WITH A NOTE ON HANDEDNESS

By B. BERTHOLD WOLFF, New York University Medical Center, and MURRAY E. JARVIK, Albert Einstein College of Medicine

The investigation of the relations between various pain-reactions to different stimuli in the same individual—be the stimuli experimental or due to disease-processes—is of considerable importance to psychology and to medicine. In the past, work with experimentally-induced pain has resulted in equivocal findings in regard to its application to clinical pain, and of all laboratory studies on the various sensations, only those for pain appear to have been the least fruitful for an understanding of the naturally occurring phenomenon.

Research with the Hardy-Wolff-Goodell radiant-heat method has probably been the most prolific of experimental pain-studies in the last two decades. This is not only because the radiant-heat method possesses qualities appreciated by the experimental psychologist, but also when it first became known it was initially regarded as a potentially very useful instrument for the measurement of pain relief, *i.e.* in the assessment of the efficiency of analgesic drugs. It is in the latter sphere, however, that the technique appears to lack value. Hardy, Wolff, and Goodell published numerous studies, later collected in book form, demonstrating the sensitivity of their technique to analgesic drugs by significant changes in the threshold of pain.[1] Other investigators, however, have on the whole been unable to support or agree with their findings.[2] Even proponents of the radiant-heat method, such as Kutscher and Kutscher, who cited 90 studies in their review, had to admit the equivocality of the results in this area.[3] One of the most serious

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[1] J. D. Hardy, H. G. Wolff, and Helen Goodell, *Pain Sensations and Reactions,* 1952, 325-378.

[2] J. E. Birren, H. B. Schapiro, and J. H. Miller, The effect of salicylate upon pain sensitivity, *J. Pharmacol. exp. Ther.,* 100, 1950, 67-71; R. A. Kuhn and R. B. Bromiley, Human pain thresholds determined by the radiant heat technique and the effect upon them of acetylsalicylic acid, morphine sulfate, and sodium phenobarbital, *ibid.*, 101, 1951, 47-55.

[3] A. H. Kutscher and H. W. Kutscher, Evaluation of the Hardy-Wolff-Goodell pain threshold apparatus and technique: Review of the literature, *Int. Rec. Med.,* 170, 1957, 202-212 and 228-230.

criticisms to be made against this technique is that much of the fundamental work done by Hardy, Wolff, and Goodell has been based on the responses of from three to five highly motivated, sophisticated and trained *S*s. It is in validity-studies of analgesic sensitivity with large numbers of unsophisticated human *S*s that the radiant-heat technique has not withstood the test of time. Hall, in a critical review of the studies on cutaneous pain, stated that general factros, such as age, sex, verbal intelligence-level, and attitude, had not been adequately controlled in many studies with the radiant-heat technique. He concluded that the major causes of variation in pain-response are due to central factors—differences in capacity to inhibit response or to perceive the whole stimulus-situation in a conceptual setting.[4] It should be pointed out, however, that a modification of the Hardy-Wolff-Goodell method for animal work, introduced by D'Amour and Smith and involving the tail flick response of a rat to stimulation by radiant heat, is still one of the techniques most widely used by pharmaceutical companies in testing the analgesic properties of various drugs.[5] In our opinion, a modification of the radiant-heat technique, described by Wertheimer and involving time as the dependent variable with heat-intensity held constant, is still one of the best available *psychophysical* methods for the study of superficial pain in man, although it appears to possess little in common with clinical pain.[6]

Many other techniques for inducing pain experimentally in man exist, but unlike the radiant-heat method, they have not been thoroughly investigated. Electric shock is a favorite stimulus in the repertoire of the experimental psychologist, and although a large number of pain-studies involving electric shock do exist, many investigators feel obliged to introduce a new, or to modify an existing, electric techniques, giving very few reliability- or validity-studies of a given technique. In general, however, irrespective of the technique or body-locus employed, results from noxious electric stimulation usually are unrelated to clinical pain.

Beecher has questioned the use and value of experimental pain in attempts to understand pain in general, and he has strongly and severely criticized the Hardy-Wolff-Goodell radiant-heat method. Beecher has stated repeatedly that his approach to the experimental study of pain, using only clinical pain, controlled with known quantities of certain analgesic drugs within a double-blind design and using the patient's verbal responses, is far superior to investigations with experimentally induced pain.[7] There is no doubt that the experimental approach of the Harvard group, headed by Beecher, has been of considerable value for studies on analgesic drugs, and has stimulated similar experimental work with clinical pain in many other institutions. This clinical approach, however, has many inherent weaknesses and faults, perhaps even worse than those for the radiant-heat method.

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[4] K. R. L. Hall, Studies of cutaneous pain: A survey of research since 1940, *Brit. J. Psychol.*, 44, 1953, 279-294.

[5] F. E. D'Amour and D. L. Smith, A method for determining loss of pain sensation, *J. Pharmacol. exp. Ther.*, 72, 1941, 74-79.

[6] Michael Wertheimer, A single-trial technique for measuring the threshold of pain by thermal radiation, this JOURNAL, 65, 1952, 297-298.

[7] H. K. Beecher, Pain—controlled and uncontrolled. Rejoinder to Dr. Hardy, Dr. Wolff, and Miss Goodell, *Science*, 117, 1953, 166-167; *Measurement of Subjective Responses*, 1959, 16-26 and 123-26; Pain, placebos, and physicians, *Practitioner*, 189, 1962, 141-155.

The most serious is the lack of knowledge about the pain-producing stimulus, which is generally completely unknown. Furthermore, this clinical approach fails to yield fundamental information on the sensation and perception of pain, although it does produce knowledge about certain drugs.

In the present authors' opinion, the real problems occurring in the scientific study of pain are: (a) the absence of a general definition of pain and (b) the lack of knowledge as to what constitutes an adequate stimulus for pain. In experimental studies, pain can usually be defined operationally and this in turn allows for an operational definition of the adequate stimulus for such a specifically defined pain. Operational definitions have, however, not yet yielded a scientific, general definition of pain or descriptions of the adequate stimulus for pain. Many investigators have pondered upon this difficulty, but cautiously decided not to attempt such a general definition.[8]

The present authors have been investigating experimental methods for the induction of pain in the laboratory and the possible application of those methods to the study of pain in the clinic. Emphasis has been placed upon attempts to simulate clinical pain experimentally in the hope that such a similarity may reduce the novelty of the artificial stimulus. In view of the authors' association with an arthritis research group, their work has been focused upon experimental investigations of deep somatic pain, because the clinical pain of the arthritic patient is of deep somatic rather than of superficial (cutaneous) or visceral origin.

Wolff and his associates have developed a psychophysical technique for the production of deep somatic pain in man by the use of controlled intramuscular hypertonic saline injections using the gastrocnemius muscle.[9] This technique was based on earlier work with intramuscular saline injections by Lewis and Kellgren.[10] More recently, the present authors modified this saline method by using the gluteus medius rather than the gastrocnemius muscles. One of the first studies was the comparison of various stimulus- and response-parameters of the hypertonic saline technique with those of the radiant-heat method. Initially, the obtained response-parameters to these two different experimental techniques appeared to be quite unrelated. In a later study, however, Jarvik and Wolff were able to demonstrate significant and interesting differences in the response to *hypertonic* and *hypotonic* saline injections. The former solution gives rise to a diffuse, deep and dull ache with long latency and duration and a gradual increase in the intensity of pain to a certain maximal level. Hypotonic saline, on the other hand, yields a well-localized, sharp or burning pain with a very brief latency and a relatively short duration and a very fast increase in pain-intensity to a maximum.[11] Subsequently, a study comparing the response-parameters to both hypotonic and hypertonic saline with those for radiant heat showed significant correlations between the *hypotonic* saline and radiant-heat thresholds of pain and also between the hypertonic and hypotonic saline thresholds, but none for the *hypertonic* and radiant-heat thresholds of pain.[12]

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[8] Thomas Lewis, *Pain*, 1942, v-vi; Beecher, *op. cit.*, 1959, 5-8.

[9] B. B. Wolff, J. L. Potter, W. L. Vermeer, and C. McEwen, Quantitative measures of deep somatic pain: Preliminary study with hypertonic saline, *Clin. Sci.*, 20, 1961, 345-350.

[10] Lewis, Suggestions relating to the study of somatic pain, *Brit. Med. J.*, 1, 1938, 321-325; J. H. Kellgren, Observations on referred pain arising from muscle, *Clin. Sci.*, 3, 1937-38, 175-190; On the distribution of pain arising from deep somatic structures with charts of segmental pain areas, *ibid.*, 4, 1939, 35-46.

On the basis of these results, two hypotheses were provisionally postulated. The first hypothesis stated that the pain-thresholds induced by *different* stimuli impinging upon *different* tissues or body sites will be significantly correlated, provided that the resultant subjective sensations of pain are similar in nature. The second hypothesis postulated that the thresholds of pain elicited by *different* stimuli impinging upon the *same* tissue or body site will be significantly correlated, irrespective of the nature of the resultant subjective sensation of pain.[13]

The investigation to be discussed in this paper is a validation-study of these two hypotheses. Specifically, four methods of inducing experimental pain (two chemical techniques yielding deep somatic pain and two thermal techniques producing cutaneous pain) will be compared and discussed.

*Method: (1) Subjects.* The 53 *Ss* (19 men and 34 women) were chronic arthritic patients, aged from 13 to 75 yr. (Median age = 52 yr.), who were free of clinical pain during the experimental session and had also not taken any medication on the day of the experiment. These *Ss* were consecutive routine admissions to an arthritis out-patient clinic or an orthopedic ward of a city hospital. They belonged to a low socio-economic level and had a wide range of ethnic origin. Their median non-verbal intelligence was average, the distribution being skewed toward low average, with very few *Ss* falling either in the borderline or high average categories. All the *Ss* were untrained and unsophisticated in regard to psychological experimentation and coöperated well during the experiment.

*(2) Thermal methods for induction of superficial (cutaneous) pain: (a) radiant-heat technique.* Cutaneous pain was elicited by radiant heat using the single-trial modification of the Hardy-Wolff-Goodell method.[14] *S*'s forehead was thoroughly blackened with India ink, allowed to dry, and placed against the aperture, 2 cm. in diameter, of the Hardy-Wolff-Goodell model ES 3 projector, which was connected to a model ER 3 control box. The apparatus had previously been calibrated by means of a model ET 2 thermopile and model HF 2 potentiometer to give a constant radiant heat flux of 100 m.cal./sec./cm.$^2$ at the aperture. Each *S* was instructed to say 'now' immediately on feeling a change in sensation from warmth

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[11] M. E. Jarvik and B. B. Wolff, Differences between deep pain responses to hypertonic and hypotonic saline solutions, *J. appl. Physiol.*, 17, 1962, 841-843.

[12] B. B. Wolff and M. E. Jarvik, Variations in cutaneous and deep somatic pain sensitivity, *Canad. J. Psychol.*, 17, 1963, 37-44.

[13] Wolff and Jarvik, *op. cit.*, 42-43.

[14] Wertheimer, *op. cit.*, 297-298.

to a pricking or burning pain after the lamp was switched on, which denoted his cutaneous pain-threshold, and then to shout 'stop' when the heat pain became unbearable, *i.e.* his pain-tolerance. Both responses were carefully timed. One practice followed by six test-trials was taken at 2-min. intervals, always using a different forehead area for each trial. Each *S*'s mean pain-threshold and mean pain-tolerance were calculated in seconds from the six test-trials. In addition, the mean pain-duration was also determined, it being the difference between the threshold and the tolerance.

*(b) Ice water (cold pressor) technique.* Each *S*'s right or left hand, according to a randomized design, was placed up to the wrist in a lukewarm water bath maintained at 35°C (±1°C) for exactly 2 min. and then immediately transferred to an ice water bath maintained at 0°C. *S* was again instructed to report 'now' at first sensing the slightest amount of pain (*i.e* pain-threshold) and to shout 'stop' and withdraw his hand when he could no longer tolerate the pain (*i.e.* pain-tolerance). The exact procedure was repeated with the other hand, and again the pain-threshold, pain-tolerance and pain-durations were calculated. Each *S*'s handedness was also recorded.

*(3) Chemical methods for induction of deep somatic (muscular) pain: (a) Hypertonic saline technique.* Twenty-four (8 men and 16 women) of the initial 53 *S*s participated in this test. These *S*s were selected in advance for detailed study for reasons totally unconnected with and independent of the present investigation. Therefore, their selection was *not* related to their performance on the thermal pain-tests.

In each *S*, eight areas of skin overlying the gluteus medius muscles—four on each side—were anesthetized intradermally with each 1 cc. of 1% procaine hydrochloride to abolish cutaneous pain. Through each anesthetized area four hypodermic needles (25 gauge and 1, 1½, or 2 in. in length, depending on thickness of the adipose layer) were inserted intramuscularly at an angle of about 45° in rosette fashion so that the tips of the needles were well separated within the muscle. A total of 32 needles was thus inserted. Injections of 0.2 cc. sodium chloride in water, ranging in concentration from isotonic to 10% weight/volume in 1% steps, were made at 2-min. intervals. Each needle was used once only and selected according to a randomized design, except that any given area received one injection only of a given saline concentration. Prior to the test, *S* had been instructed to report 'now' immediately on sensing any kind of pain, then to describe the nature of the painful sensation, and finally to indicate its cessation. These simple instructions were followed easily. Injections were commenced with isotonic saline and then the concentration was increased gradually until a pain-response was obtained, which signified the pain-threshold. Three additional series of threshold-measures were taken, using alternately ascending and descending series, each series starting at a saline concentration either two steps below or above the previously reported threshold. This is, therefore, the method of limits. Each *S*'s mean deep somatic pain-threshold was calculated as the average of the four reported thresholds, *i.e.* two ascending and two descending ones, and expressed in terms of percentage sodium chloride. No pain-tolerance levels were measured. Each *S* was completely unaware of the concentration to be used, and, in fact, no *S* knew how many needles or injections were to be given. Furthermore, *S*'s gluteal region was also screened from his view. This is thus a single-blind technique.

*(b) Hypotonic saline technique.* Eighteen (6 men and 12 women) of the initial 53 *S*s participated in this test. Once again, these *S*s were selected in advance and *not* on account of their performance on the thermal pain-tests.

This technique for inducing muscle pain simply substitutes hypotonic sodium chloride solutions for the hypertonic solutions. The hypotonic solutions ranged from isotonicity to pure water in steps of 0.1% saline. Otherwise, exactly the same procedure as for the hypertonic saline technique was employed, utilizing the remaining unused needles. Two ascending and two descending pain-thresholds were obtained and the mean calculated.

It should be emphasized that Jarvik and Wolff's findings have demonstrated significant differences in the deep somatic pain-responses to hypertonic and hypotonic saline solutions, as has already been discussed.[15] Their results are considered sufficient justification to regard the hypertonic and the hypotonic saline techniques as two different chemical methods.

*(4) Statistical treatment.* For each variable, the mean, median, standard deviation and range were calculated. Product-moment correlations were determined between means for different measures. Kendall coefficients of concordance were also calculated.[16]

*Results.* It should be pointed out that the various response-parameters for the ice-water technique could be obtained from five possible combinations; namely those for the right, left, dominant or non-dominant hand or from the average of both hands. In the following tables and in the discussion, the parameters for the dominant hand have been used as representative of the ice-water technique, unless specified otherwise.

It will be noted from Table I that the mean pain-thresholds of 1.08 log sec. for both thermal techniques are identical, although there is a slight difference in the medians. The standard deviations for these two methods are also fairly similar, but there is a considerable difference in the range of responses, that for ice water being much longer than that for radiant heat.

The mean deep somatic pain-threshold for hypertonic saline of 3.8% NaCl is about four times as concentrated as isotonicity, whereas that for hypotonic saline (0.41% NaCl) is approximately half as strong as isotonicity (Table I). The mean pain-threshold for hypotonic saline differs somewhat from the median, indicating some skewness in the distribution. In this case, the median is probably a better index of the central tendency of the hypotonic saline distribution, because, empirically, few *S*s had threshold-responses between 0.5 and 0.8% NaCl.

All correlations involving the hypotonic saline technique are negative because the smaller the concentration of sodium chloride, the larger the degree of hypotonicity. It will be noted that the only statistically significant correlations in Table II are those between (a) radiant heat and ice water, and (b) radiant heat and hypotonic saline. The latter correlation is the higher, but in view of the smaller *N*, it is only significant at the 5% level, whereas the former reaches the 1% level. The other correlations are low and insignificant statistically, although that of −0.34 for ice water and hypotonic saline could suggest the possibility of a more significant relation if a larger *N* had been available.

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[15] Jarvik and Wolff, *op. cit.*, 841-843.

[16] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 229-239.

TABLE I

Distribution of the Thresholds of Pain

| | Thermal (superficial) | | Chemical (deep) | |
|---|---|---|---|---|
| | radiant heat $N=53$ (log sec.) | ice water (dominant hand) $N=53$ (log sec.) | hypertonic saline $N=24$ % NaCl | hypotonic saline $N=18$ % NaCl |
| Mean | 1.08 | 1.08 | 3.8 | 0.41 |
| Median | 1.16 | 1.12 | 3.4 | 0.30 |
| *SD* | 0.27 | 0.29 | 1.64 | 0.19 |
| Range | 0.28–1.46 | 0.45–1.83 | 1.5–7.0 | 0.8–0.2 |

TABLE II

Product-Moment Correlations between the Thresholds of Pain

| | Radiant heat $N=53$ | Hypertonic saline $N=24$ | Hypotonic saline $N=18$ |
|---|---|---|---|
| Ice water, (dominant hand) | .42* | .13 | −.34 |
| Radiant heat | | .27 | −.53† |
| Hypertonic saline | | | −.23 |

* $p=0.01$; † $p=0.05$.

Section A, Table III indicates that the likelihood of predicting an *S*'s rank order in pain-threshold on one of the tests from the other three tests is *significantly greater* than chance. This, however, is due to the intercorrelations between the ice water, radiant-heat, and hypotonic saline techniques, rather than those for the hypertonic saline technique, as shown by the difference in statistical significance between Sections B and C. Thus the prediction of an *S*'s rank order on the hypertonic saline technique from his positions on the other tests is less certain than predicting his rank order on any one of the other tests from his positions on the remaining two tests (*i.e.* excluding the hypertonic saline technique).

In Table IV, the data in Table II have been elaborated to demonstrate the correlations of the ice water technique for each hand with those of the other techniques. It will be observed immediately that in every case the correlations for the right hand and for the dominant hand are consistently larger than those respectively for the left hand and for the non-dominant hand. The magnitude of the difference in correlation between the two hands is practically constant for each of the different techniques. This difference between the two hands is significant at the 5% level for radiant heat, but fails to reach statistical significance for the hypertonic and hypotonic techniques due to the smaller *N*. The data for the dominant and non-dominant hands are not identical with those for the right and left hands because there were 50 right-handed and 3 left-handed *S*s. In view of the very few left-handed *S*s in the present sample,

TABLE III

KENDALL COEFFICIENTS OF CONCORDANCE BETWEEN THE VARIOUS PAIN-THRESHOLDS

| Types of pain-thresholds | W | $X^2$ | $p$ |
|---|---|---|---|
| (A) N = 18 | | | |
| (1) Ice water, dominant hand | | | |
| (2) Radiant-heat | | | |
| (3) Hypertonic saline | | | |
| (4) Hypotonic saline | .47 | 31.96 | .02 |
| (B) $N=18$ | | | |
| (1) Ice water, dominant hand | | | |
| (2) Radiant heat | | | |
| (3) Hypotonic saline | .61 | 31.11 | .02 |
| (C) $N=24$ | | | |
| (1) Ice water, dominant hand | | | |
| (2) Radiant heat | | | |
| (3) Hypertonic saline | .52 | 36.16 | .05 |

TABLE IV

PRODUCT-MOMENT CORRELATIONS BETWEEN PAIN-THRESHOLDS SHOWING DIFFERENCES FOR HANDEDNESS

| Ice water | Radiant-heat ($N=53$) | Sig. diff. ($p$) | Hyper-tonic saline ($N=24$) | Sig. diff. ($p$) | Hypotonic saline ($N=18$) | Sig. diff. ($p$) |
|---|---|---|---|---|---|---|
| Right hand | .46* | .05 | .16 | not signif. | −.34 | not signif. |
| Left hand | .31† | | .01 | | −.17 | |
| Dominant hand | .42* | .05 | .13 | not signif. | −.34 | not signif. |
| Non-dominant hand | .28 | | −.06 | | −.17 | |

* $p=0.01$; † $p=0.05$.

little importance can be attached to their contribution. It is, therefore, really the right-handed *Ss* who have produced these different correlations. The important results are that the pain-thresholds for the left or non-dominant hand are almost completely unrelated to those of the other techniques, excepting that for the radiant-heat.

*Discussion.* On the basis of our first hypothesis, which postulates a significant relationship between different pain-thresholds if the resultant pains are similar, it had been predicted that significant intercorrelations should occur between each of the thermal and hypotonic saline thresholds. It had also been predicted, as a corollary to the first hypothesis, that there should be no significant correlations between the pain-thresholds derived from hypertonic saline and the thermal techniques. In actual fact, our current results tend to confirm our predictions because the correlations between the thresholds from radiant-heat and the hypotonic saline and ice water techniques, respectively, were significant, whereas those involving the hypertonic saline method were low and insignificant. The only exception was the failure of the correlation of −0.34 between the ice water and hypotonic saline thresholds to reach statistical significance. This correlation, however, is large enough to suggest a trend for a relationship, and if all 53 *Ss* could have been used instead of only 18, it would actually have reached statistical significance, provided that there would have been no major change in its value. Furthermore, the sensation of pain produced by ice water is a sharp and pricking pain around threshold-values, which, however, tends to change into a diffuse and dull ache as tolerance-levels are reached. Thus the ice water technique is probably not too good a method for producing just a single type of pain, even if the results are restricted to threshold-values, and this also may have contaminated our present findings to some extent.

It is considered that the current results support our first hypothesis, and that in fact there appears to be a significant relationship between different thresholds of pain produced by different stimuli when the subjective qualities of the ensuing pain sensations are similar. This finding, if validated by future work, would have important applications to the study of clinical pain, because it suggests that experimental pain may be substituted for clinical pain if the experimental pains could be made to simulate those observed clinically. This would allow detailed studies of various stimulus- and response-parameters, which cannot be directly obtained from clinical pain. Such an experimental approach may be of value in analgesic studies of drugs.

It had also been predicted that there would be a significant correlation between the hypertonic and hypotonic saline pain-thresholds, using our second hypothesis as a basis, which stated that there should be a significant relation between different pain-thresholds elicited from the same body-locus. This hypothesis has not been supported by our present findings, because the correlation of −0.23 is low and insignificant, unlike that of −0.51 obtained in our previous study.[17] This point will have to be investigated further. It should be emphasized, however, that a comparison of the effects of intramuscular injections of hypertonic and hypotonic sodium chloride solutions by our method allows almost perfect equation of error variables and artifacts, because both types of experimental stimuli can be administered alternately during the same session so that any environmental, internal and behavioral changes affect both types of stimuli almost equally.

Finally, our empirical and unpredicted finding of consistent and occasional significant differences in the pain-threshold correlations between the right and left hands and their relation to the pain-thresholds produced by other techniques is very interesting and intriguing. Obviously, these results will have to be investigated by means of an additional study, especially as we had not used the ice water technique previously. At this stage it is not possible to decide whether the right hand or the dominant hand is responsible for this finding, as we had only three left-handed individuals in our group of 53 *S*s. One could speculate, however, that it is the dominant hand which is responsible for the greater degree of relation between thresholds of pain, which in turn suggests that the dominant side is either more sensitive or more closely integrated with an individual's affective behavior than the non-dominant side.

In conclusion the present results support previous findings, suggesting that different thresholds of pain are significantly correlated whe pains elicited are similar even if the noxious stimuli are differe pinge upon different parts of *S*'s body and irrespective of t ficial or deep somatic origin of the sensations. This finding app value for future studies of clinical pain, as it may permit the substitut of experimental for clinical pain and thus allow for a greater degree of experimental control. There is also a suggestion, as yet unconfirmed, that dominance or handedness may play an interesting role in perception of pain.

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[17] Wolff and Jarvik, *op. cit.*, 40.

# EFFECTS OF SYMMETRY ON THE PERCEPTION OF TACHISTOSCOPIC PATTERNS

By E. RAE HARCUM, The College of William and Mary

This study investigates effects of directional characteristics of letters on the accuracy of their reproduction when exposed tachistoscopically within a group. Ordinarily, when letters are exposed tachistoscopically across fixation, those to the left of fixation are more accurately reported than those in the right visual field.[1] The present objective is to provide inferential evidence that this superiority for letters on the left is caused by a perceptual process' having temporal as well as spatial components.

Although the projection of a pattern on the retina initiates the perceptual process, the process probably does not terminate with the termination of the stimulus-exposure, but continues as an analysis of the persisting physiological traces.

In Heron's postulation of a 'post-exposure process,' the sequential analysis of the individual traces is analogous to the sequence in which the stimulus-elements would be fixated by the eyes during a longer exposure.[2] Evidence for such a mechanism has now been provided by studies of the direction of the first eye-movements after tachistoscopic exposure.[3] When letters appear binocularly both to the right and left of fixation, the greater frequency of such movements is to the left. Presumably, a primacy-effect produces fewer errors for the elements which are read first in the perceptual process.

Harcum, using linear binary patterns which were tachistoscopically exposed across

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[1] A. D. Glanville and K. M. Dallenbach, The range of attention, this JOURNAL, 41, 1929, 225; Irving Anderson and H. R. Crosland, A method of measuring the effect of primacy of report in the range of attention experiment, this JOURNAL, 45, 1933, 701-713; Crosland, Superior elementary-school readers contrasted with inferior readers in letter-position, "range of attention," scores, *J. educ. Res.*, 32, 1939, 410-427; I. H. Anderson, The effect of letter-position on range of apprehension scores, with special reference to reading disability, *Univ. Mich. Sch. Educ. Bull.*, 18, 1946, 37-40; Woodburn Heron, Perception as a function of retinal locus and attention, this JOURNAL, 70, 1957, 38-48; H. S. Terrace, The effects of retinal locus and attention on the perception of words, *J. exp. Psychol.*, 58, 1959, 382-385.

[2] Heron, *op. cit.*, 38-48.

[3] M. P. Bryden, The role of post-exposural eye movements in tachistoscopic perception, *Canad. J. Psychol.*, 15, 1961, 220-225; H. F. Crovitz and Walter Daves, Tendencies to eye movement and perceptual accuracy, *J. exp. Psychol.*, 63, 1962, 495-498.

fixation and at various angles to the horizontal plane in different exposure, generally found fewer errors in reproducing the elements to the left of fixations.[4] When one of the halves of a stimulus-pattern at a given inclination, formed by those elements appearing on one side of the fixation-point, was found to have a minimum of errors relative to the various other halves of patterns, the other half of that same pattern tended to show a maximum of errors. This result occurred presumably because the *O* most consistently scanned that pattern in one direction, and, therefore, the primacy-effect most consistently favored the one half of the pattern over the other half.

Bryden reported greater accuracy to the right or left of fixation in reproducing groups of forms presented to the right and left of fixation if the reporting-sequence began at the right or left, respectively.[5] A similar effect was noticed by Dallenbach in the naming of colors.[6] Regardless of the direction of reporting, Bryden found that letters to the left of fixation were, however, always reported more accurately. He concluded that the perceptual process for letter-patterns exhibits a 'polarization' —a temporal-spatial directional characteristic—whereby the letters to the left are characteristically perceived first. Thus, specific attributes of the stimulus influenced the inferred direction of the perceptual process.

The directionality of the letters within tachistoscopic patterns, therefore, apparently affects relative accuracy of reproducing letters between the right and left visual fields. From the above argument, one can predict that patterns composed of asymmetrical letters, which have more intrinsic directionality than symmetrical letters, should be perceived more consistently in one directional sequence than patterns of symmetrical letters. Thus, asymmetrical letters like 'R' and 'S' should produce larger differences between the hemifields than symmetrical letters like 'M' and 'T,' which are directional only because they are English letters.

Three hypotheses were tested in two experiments: The first is that, even with brief exposure, there is a sequential scanning of the letters, which usually proceeds analogously from left to right across the visual field. Therefore, because of primacy-effects, most *O*s will exhibit fewer errors for letters in the left visual field. The second is that the scanning process for asymmetrical stimuli is more consistent in direction and, therefore, productive of large differences in perceptual accuracy between left and right visual fields, and the third is that, because of the greater conflict between scanning directions with symmetrical letters, they are more difficult to reproduce.

Experiment II used a larger number of *O*s and a shorter exposure-duration than Experiment I. The conditions in the two experiments were otherwise identical, except that the field-luminance in Experiment II was increased in an attempt to compensate for the shorter exposure.

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[4] E. R. Harcum, Three inferred factors in the visual recognition of binary targets, in J. W. Wulfeck and J. H. Taylor (eds.), Form discrimination as related to military problems, *Nat. Acad. Sci., Nat. Res. Coun, Publ., 561,* 1957, 32-37.

[5] Bryden, Tachisotoscopic recognition of non-alphabetical material, *Canad. J. Psychol.,* 14, 1960, 78-86.

[6] Dallenbach, Attributive *vs.* cognitive clearness, *J. exp. Psychol.,* 3, 1920, 223.

*Method.* Eight-letter nonsense-words, printed in capital letters with black ink on white paper, were exposed in a Dodge tachistoscope, at a distance of 26 in. from *O*'s eyes, for 0.25 sec. in Experiment I and for 0.15 sec. in Experiment II. Field-luminance was 2 ft-L. in Experiment I and 3.2 ft-L. in Experiment II.

Letters were uniformly 0.185 in. in height, and about 0.17 in. wide for most letters. Stroke-width was 0.02 in. Total length of the pattern was approximately 3.6 in., with letters evenly spaced. Fixation was at the center of the pattern.

One set of 56 stimulus-cards employed 7 symmetrical letters (H, M, T, V, W, X, Y); this is called the Symmetrical condition. Another set of cards used 7 asymmetrical letters (B, G, J, K, N, R, S); this is called the Asymmetrical condition. The mean percentages of occurrence in English of the symmetrical and asymmetrical letters were 3.33 and 3.36, respectively.[7] Each stimulus-pattern included all 7 letters, with one letter appearing twice. Each letter appeared in each possible position 8 times in the group of 56 patterns. The 112 cards were arranged in a haphazard order of presentation, with neither symmetrical nor asymmetrical patterns appearing more than three times in succession.

The *O*s, college students with normal vision, were told only that eight letters would be exposed. No *O* volunteered any information indicating awareness of the critical difference in the stimulus-cards. There were 17 *O*s in Experiment I, and 47 different *O*s in Experiment II.

Each letter of the pattern was to be reproduced on a prepared record-sheet. One error was scored for failure to reproduce correctly a letter in its correct position within the pattern, allowing displacement by one position either way. This positional allowance was used to emphasize the aspect of letter-recognition, rather than stimulus-localization. Some requirement for localization was necessary because each letter appeared in each pattern.

*Results.* Of the 17 *O*s in Experiment I, 13 made fewer errors for letters in the left field, confirming Hypothesis 1 ($p = 0.025$, by the binomial test). The data for the *O*s who exhibited fewer errors on the right (the Right-superior group), and the data of those *O*s who produced fewer errors on the left (the Left-superior group), are shown separately in Fig. 1. The means of errors as a function of letter-position for all *O*s are plotted for both symmetrical and asymmetrical conditions. In Fig. 2 the data of Experiment II are given separately for the 11 'Right-superior' and for the 36 'Left-superior' *O*s. The hypothesis of left superiority is again confirmed by the binomial test ($p < 0.0003$).

Each of the 17 *O*s in Experiment I is categorized by a two-way classification in Table I, according to whether he exhibited more over-all errors for symmetrical or asymmetrical patterns, and whether he exhibited a greater difference between hemifields with symmetrical or asymmetrical patterns. Thirteen of the 17 *O*s exhibited a greater difference between left and right

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[7] Godfrey Dewey, Relative frequency of English speech sounds, *Harvard Studies in Educ*, 4, 1923, 184.

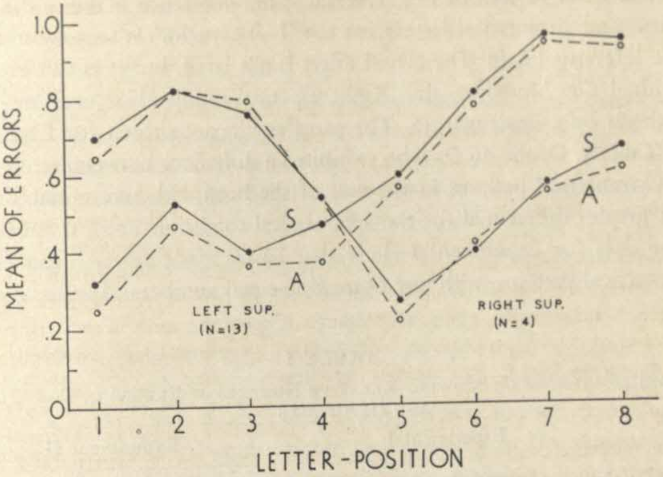

Fig. 1. Means of Errors Per Letter-Position in Experiment I
A = asymmetrical; S = symmetrical letters

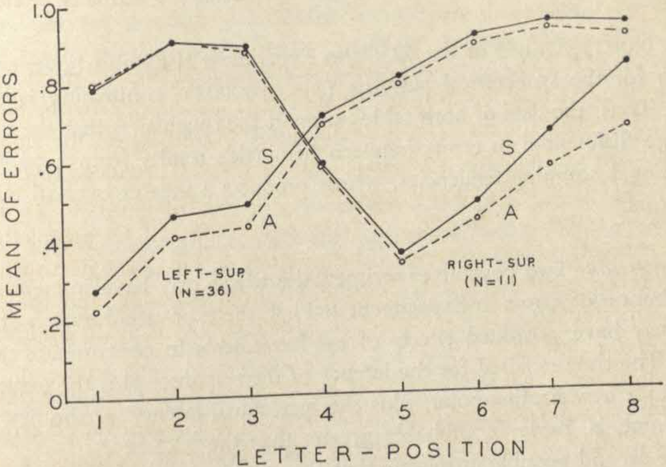

Fig. 2. Means of Errors Per Letter-Position in Experiment II
A = asymmetrical; S = symmetrical letters

fields with the asymmetrical letters ($p = 0.025$), supporting Hypothesis 2. This effect can be seen in Fig. 1, because the difference between the asymmetrical and symmetrical letters for the 'Left-superior' is somewhat greater in the left visual field. The actual effect is not large, but it is consistent for individual *O*s. Moreover, the 'Right-superior' group also conforms to the hypothesis by a small margin. The same results are shown for Experiment II in Table I. Of the 46 *O*s who exhibited a difference between symmetrical and asymmetrical patterns in the size of the hemifield differential, only 13 had a greater differential for the symmetrical condition ($p < 0.003$).

The data for Experiment I show that 14 *O*s made fewer errors for the asymmetrical patterns, with one *O* making equal numbers of errors for both ($p = 0.002$). Only 5 of the 47 *O*s in Experiment II exhibit fewer over-all errors for the symmetrical patterns ($p < 0.00003$), confirming Hypothesis 3. Thus, the data of both tables support the conclusion that the lack of a large differential in errors between hemifields results from a conflict in directional scanning tendencies, which produces a greater over-all number of errors.

TABLE I

CLASSIFICATION OF *O*S SHOWING RELATIVE NUMBERS OF ERRORS IN CONDITIONS AND HEMIFIELDS

| | Experiment I | | | Experiment II | | |
|---|---|---|---|---|---|---|
| Greater R-L difference | Fewer over-all errors | | | Fewer over-all errors | | |
| | asym. | sym. | Total | asym. | sym. | Total |
| asymmetrical | 12.5 | 0.5 | 13.0 | 31.75 | 1.75 | 33.50 |
| symmetrical | 2.0 | 2.0 | 4.0 | 10.25 | 3.25 | 13.50 |
| Total | 14.5 | 2.5 | 17.0 | 42.00 | 5.00 | 47.00 |

*Discussion*. Two possible experimental artifacts may have influenced the distribution of errors in Experiment I. First, the exposure-duration of 0.25 sec. may have permitted effects of eye-movements to contaminate the results. The time required for the latency of the response, plus the excursion-time to a new fixation-point, plus the 'perceptual latency' at the new fixation-point, is, however, probably greater than the 0.25 sec.[8]

The second possible artifact is that the curves of errors for symmetrical and asymmetrical conditions converged on the high-error side of fixation because they both approached the ceiling on possible errors. This does not seem to be the determining factor, however, since 8 of the 13 *O*s who showed a greater right-left differential for asymmetrical letters actually exhibited a crossover to more errors for asymmetrical letters at one or more letter-positions on the high-error side of fixation. Also, in Experiment II, 22 of the 36 *O*s showing a greater hemifield differential for the asymmetrical patterns exhibited crossovers to more errors for asymmetrical letters at one or more of the letter-positions on the high-error side.

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[8] Hubertus Strughold, The human time factor in flight: II. Chains of latencies in vision, *J. Aviat, Med.*, 22, 1951, 100-108.

There is some additional evidence that the greater right-left difference for asymmetrical letters is not an artifact. In Table I the greater degree of left-right differential seems to be related to a smaller number of over-all errors. Of the 13 *O*s showing a greater left-right differential for asymmetrical letters, only one exhibited equal over-all errors for the two patterns. On the other hand, of the 4 *O*s exhibiting a greater difference for symmetrical letters, two of them also showed fewer over-all errors for the symmetrical patterns. A similar result was obtained in Experiment II.

The superior performance for Asymmetrical letters may have resulted from the fact that they are more easily recognized. For example, the top edges of V, X, Y, and M are very similar, and, thus, serve to produce confusion among these letters. But the relationship noted above in Table I—that the greater number of errors are related to smaller left-right differences—indicates an effect of a conflict of scanning directions.

The results of the present study, therefore, provide evidence for a sequential reading of persisting traces after tachistoscopic exposures in the manner of the 'post-exposure process' proposed by Heron.[9] Since there was no chance of establishing a differential set to perceive the two classes of stimuli before their exposure in this study, the direction of the process must be established in part by a direction inherent in the stimuli themselves. This confirms and extends the earlier conclusion of Terrace, who found that form-stimuli produced equal numbers of errors in different exposures to the right and left of fixation, and that letter-stimuli produced fewer errors on the right of fixation, even though the *O*s did not beforehand know the type of stimulus-pattern.[10] The present results are also consistent with the suggestion of Bryden that the process of perceiving tachistoscopically presented groups of letters is 'polarized' in that it exhibits a directional character.[11] Thus, the temporal-spatial characteristics of the process are determined by variables among the *O*s, as shown by the existence of both Right- and Left-superior *O*s, and by directional attributes of the stimulus-elements themselves. The influence of variables among the *O*s in the distribution of attention and cognition, as well as the effect of stimulus-determinants, has previously been demonstrated by Dallenbach and his co-workers.[12]

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[9] Heron, *op. cit.*, 38-48.

[10] Terrace, *op. cit.*, 382-385.

[11] Bryden, Tachistoscopic recognition of non-alphabetical material, *Canad. J. Psychol.*, 14, 1960, 78-86.

## SUMMARY

The basic assumption of this study was that perception of letter-patterns flashed across the visual field requires a temporal as well as a spatial analysis of the visual traces. Therefore, patterns composed of asymmetrical letters, which have greater intrinsic directional capacity than symmetrical letters, should be more consistently analyzed in one direction. Primacy-effects favoring the elements analyzed first would produce, it was predicted, a larger differential in errors of reproduction between hemifields for asymmetrical-letter patterns. It was also predicted that conflicts between directional-scanning tendencies would produce a greater over-all number of errors for the symmetrical patterns. In two experiments both predicted outcomes were obtained.

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[12] Dallenbach, *op. cit.*, 183-230; Position vs. intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286; R. S. Burke and K. M. Dallenbach, Position vs. intensity as a determinant of attention of left-handed observers, this JOURNAL, 35, 1924, 267-269; Glanville and Dallenbach, *op. cit.*, 233-236.

# FIGURAL AFTER-EFFECTS: DISPLACEMENT OR CONTRAST?

By ROBERT B. FREEMAN, Jr., The Pennsylvania State University

Two physiological theories have been offered to account for figural after-effects (*FAE*s). The satiation-theory derives from the Gestalt field-theory of the cortical processes held to underlie figural perception.[1] The statistical theory is based on an analysis by Marshall and Talbot of visual contour-resolution based on vertical and lateral summation of excitation.[2] Both theories treat *FAE*s as phenomena of contour-displacement, the magnitude of any effect being assumed to depend, among other things, on the angular distance between inspection- (*I*-) and test- (*T*-) contours. In fact, a major theoretical problem has been raised by the observation of the so-called *distance-paradox*, the quadratic form which describes the relationship between inter-contour distance and magnitude of *FAE*.

There is, however, some reason to doubt that distance between contours is a critical determinant of the magnitude of the *FAE*. In a series of experiments with circular *I*- and *T*-figures, Oyama obtained two results which are inconsistent with the displacement-hypothesis.[3] First, he found that, regardless of the angular size of the *I*- and *T*-figures, the maximal size-effect was produced when the diameters of concentric *I*- and *T*-circles were in a ratio of 1:2 or 2:1. That is, the magnitude of the size-effect was found to be a function of the size-ratio of the stimulus-circles rather than of the absolute distance between their contours. Secondly, the size-effect was also obtained under conditions in which the *I*- and *T*-circles were not concentric; the magnitude of the size-effect was found to be a linear, decreasing function of the eccentricity of the *I*-circle from the *T*-circle. The simi-

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[1] Wolfgang Köhler, *Dynamics in Psychology*, 1940, 73-82; Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

[2] C. E. Osgood and A. W. Heyer, Jr., A new interpretation of figural after-effects, *Psychol. Rev.*, 59, 1952, 98-118; W. H. Marshall and S. A. Talbot, Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity, in Heinrich Klüver (ed.), Visual mechanisms, *Biol. Sympos.*, 7, 1942, 117-164.

[3] Morigi Sagara and Tadasu Oyama, Experimental studies on figural after-effects, *Psychol. Bull.*, 54, 1957, 327-338; Tadasu Oyama, Experimental studies of figural after-effects: (II) Spatial factors, *Jap. J. Psychol.*, 25, 1954, 195-206.

larity between Oyama's results and those obtained by Morinaga and by Ogasawara in experiments on simultaneous and successive size-contrast suggests that the size-effect may be a phenomenon of contrast rather than of contour-displacement.[4] The present experiment is designed to test the contrast-hypothesis.

*Method: (1) Stimulus-conditions.* Four characteristics of the stimulus were varied: (1) the size of the *I*-figure; (2) the ratio of the diameters of the standard *T*-figure and the *I*-figure; (3) the degree of eccentricity of the center of the *I*-figure from that of the *T*-figure; and (4) the direction of eccentricity, either upward (E+) or downward (E−). The stimulus-conditions are shown in Fig. 1. The diameter of the *I*-circle took one of three angular values (0.5°, 1.0°, and 1.5°), of which only $I = 1°$ is shown. The diameter of the *T*-figures varied as ratios of the *I*-figure, from $T = I$ to $T = 2.4I$. The eccentricity of the center of the *I*-circle from the center of the *T*-circle varied in 0.5°-steps from $\mathrm{E} = 0°$ to $\mathrm{E} = 2.0°$. The center of the standard *T*-circle ($T_S$) was 3.5° to the right of the fixation-dot, and the center of the variable *T*-circle ($T_V$) 3.5° to the left.

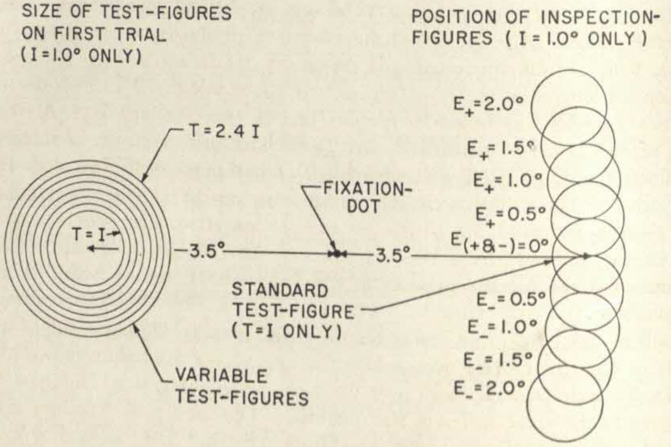

FIG. 1. ANGULAR RELATIONSHIPS AMONG THE STIMULUS-CONDITIONS
(Stimulus-relationships are shown only for the size-condition in which the diameter of the *I*-circle was 1°.)

The center of the *I*-figure was varied along the circumference of an arc whose radius was 7° of visual angle and whose center coincided with the center of $T_V$ on the left. The height of the *T*-figures in the visual field did not vary.

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[4] S. Morinaga, Conditions of size-assimilation and size-contrast, *Masuda Hakushi Shaon Saikin Shinrigaku Ronbunshu*, 1935, 24-48; J. Ogasawara, Displacement-effect of concentric circles, *Jap. J. Psychol.*, 22, 1952, 224-234; both these papers are cited in Sagara and Oyama, *op. cit.*, 330.

*(2) Observers.* The *Os* were 12 university undergraduates, each of whom served for five 1-hr. periods and was paid for his participation in the experiment. All *Os* were men, right-handed and right-eyed, and all had normal vision.

*(3) Apparatus.* The test-figures ($T_S$ and $T_V$) consisted of circular apertures cut out of cardboard sheets and illuminated from the rear by a lightbox whose front surface was a large sheet of flashed opal glass. The circular cutouts were positioned in the fronto-parallel plane at an optical distance of 250 cm. The *I*-aperture was illuminated by a second lightbox behind it, and its image was so reflected from a 45° first-surface, half-silvered mirror that it coincided laterally with the image of $T_S$. The luminance of all the figures was 0.15 ft.-L.

The distance of $T_V$ from *O* was variable along a track, permitting variation in the projected size of $T_V$ in constant units of retinal angle. The size of $T_V$ during testing was varied in steps equal to 1/60 the diameter of $T_S$. The staircase method was used, with the first setting of $T_V$ always at physical equality with $T_S$.

*(4) Procedure.* All *Os* viewed the stimuli with the right eye alone through a 4-mm. hole. *O* was instructed, and repeatedly reminded, to keep his eye on the fixation-dot whenever the *I*-or *T*-figure was illuminated, and to indicate which of the two *T*-circles was larger by pressing one of two telegraph keys. At the beginning of each hour, *O* matched $T_V$ and $T_S$ without prior exposure of *I* for the test-condition $T = I$, giving five determinations of half-meridional differences (*HMDs*) for each *O*.[5] Then there were six presentations of the *I*-circle alone, after which the *T*-circles followed the *I*-circle on each presentation. The *I*-circle was exposed for 8.5 sec., followed by a 1-sec. exposure of the *T*-circles, followed by 3.5 sec. of darkness (except for the presence of the fixation-dot), during which *O* made his response.

Each of the 12 *Os* was tested for five 1-hr. periods. During a single hour, the angle of eccentricity (E) was constant at one of the five values indicated. Eccentricity for each *O* was varied over the 5-hr. period of testing. Different *Os* were assigned to the three different size-conditions. The groups were further subdivided into the two directional control conditions ($E_+$ and $E_-$). In a 3 × 2 factorial design with two *Os* in each cell, a total of 12 *Os* was required. Within each hour, *O* was tested on each of eight values of *T*:*I* ratio, as indicated at the left in Fig. 1. The assignment of conditions to *Os*, the order of presentation of displacement-conditions in successive hours, and the order of presentation of *T*-figure sizes within each hour all were randomly determined.

*Results: (1) Analysis of the data.* A point of subjective equality (*PSE*) was computed for each experimental condition in terms of distance of $T_V$ from *O*. The *PSE*s were then subjected to analysis in four stages: (1) The *PSE*s in distance of $T_V$ from *O* were transformed to *PSE*s in angular terms (minutes of arc). (2) The *PSE*s then were corrected for half-meridional differences (*HMD*s) as determined prior to the experiment. The *HMD*-correction affected only the over-all level of all curves for any given *O*, and had no effect on the slope or curvilinearity of the *FAE*-functions. Mean *PSE*s, corrected for *HMD*s, are plotted in Fig. 2. (3) Three aspects

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[5] K. T. Brown, Factors affecting differences in apparent size between opposite halves of a visual meridian, *J. opt. Soc. Amer.*, 43, 1953, 464-472; Methodology for studying figural after-effects and practice effects in the Müller-Lyer illusion, this JOURNAL, 66, 1953, 629-634.

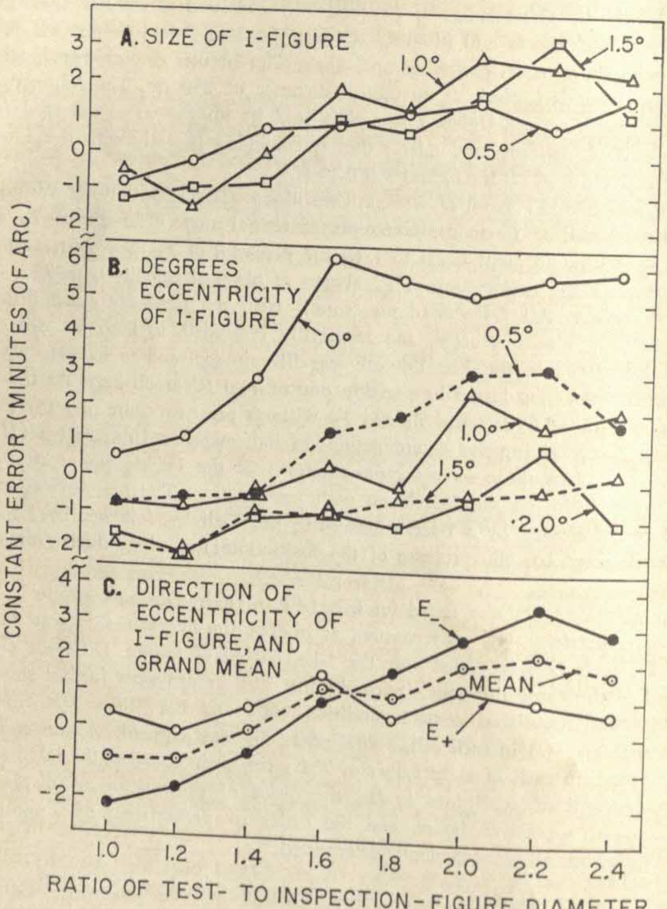

FIG. 2. FIGURAL AFTER-EFFECTS AS A FUNCTION (A) OF SIZE, (B) OF DEGREES ECCENTRICITY, AND (C) OF DIRECTION OF ECCENTRICITY OF THE INSPECTION-FIGURE

The three sets of curves are based on data obtained from the same 12 *O*s. Each point in (A) is based on five measures for each of 4 *O*s; in (B) on one measure for each of 12 *O*s; and in (C) on five measures for each of 6 *O*s.

of the functions relating *PSE* to *T:I* ratio were analyzed statistically: level, slope, and curvilinearity. An index was computed for each of the three sources of variance. An index of *level* was obtained by computing the mean of all eight scores for each individual curve. An index of *slope* was obtained for each curve by multiplying the scores by the coefficients of orthogonals of the first degree and summing the cross products for all eight measures. An index of *curvilinearity* was obtained in the same way, except that the coefficients for the second-degree polynomial were used. These computations yielded a single score representing the responses of each *O* to all eight *T:I* ratios. (4) *Each* of the three matrices of indices then was subjected to analysis of trend for repeated measures,[6] in which the trend of the indices over the five conditions of eccentricity experienced by each *O* was analyzed for the six experimental groups (three sizes and two directions). Since the only significant effects were amount and direction of eccentricity, summaries of the three trend-analyses are omitted.

*(2) Size of inspection-figures (I).* Variation in *I* did not contribute significantly to the variance of the results (Fig. 2A), nor did it have a significant effect either on the slope or the curvilinearity of the errors of judgment as a function of *T:I* ratio. As can be seen in Fig. 2 A, the slopes and the maxima of the mean error were comparable for all three sizes of *I*-figure.

*(3) Eccentricity.* As eccentricity increased, the magnitude of the *FAE* decreased. The over-all level of the curves in Fig. 2 B decreases as eccentricity increases. The over-all trend of the indices of *level* with variation of eccentricity was significant at the 1% level ($F = 14.54$, $df = 1,6$). The effect of eccentricity on the level of the curves is primarily linear; variance attributable to quadratic, cubic, and quartic components of the curve relating eccentricity to level does not achieve significance.

An analysis of the indices of *slope* also shows a significant over-all trend with variation in eccentricity ($F = 15.35$, $df = 1,6$). The slope of the curves of Fig. 2 B decreases with increase in eccentricity. Again the three size-conditions do not contribute significantly to the variance of the indices.

*(4) Direction of eccentricity.* The direction in which the *I*-circle was eccentric to the *T*-circle also had a significant effect on judgments. The comparison of mean slope of the curves shown in Fig. 2 C for $E_-$ and $E_+$ shows that the slope is, in general, significantly greater for the $E_-$ condition than for the $E_+$ condition ($F = 6.01$, $df = 1,6$). Since there is *not* an interaction between amount of eccentricity and direction, it may be assumed that the effect of direction of eccentricity is a general one, in the sence that it is independent of the amount of eccentricity.

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[6] D. A. Grant, Analysis-of-variance tests in the analysis and comparison of curves, *Psychol. Bull.*, 53, 1956, 141-154.

*(5) Distance-paradox.* Indices representing the quadratic form of the curves in Fig. 2 were obtained and a trend analysis was performed on the indices thus obtained. None of the experimental variables (size, direction of eccentricity, or amount of eccentricity) contributed significantly to the variance of the indices of curvilinearity. The over-all mean of the curvilinearity for all *O*s and all conditions of eccentricity is, however, significantly different from zero beyond the 5% level as determined by a *t*-test. There is, then, statistical evidence that curvilinearity of a quadratic form exists but that it does not vary among experimental conditions. One may therefore assume that the major contributor to the variation of *PSE* with $T_S$ is the ratio of the size of $T_S$ to *I*.

*Discussion.* It is the thesis of this paper that the stimulus-variable which produces *FA*Es of size is the size-ratio of the *T*- and *I*-figures and not the angular distance between their contours. If the thesis is correct, then the perceptual effect of one figure on another must be attributable to contrast rather than to displacement. The contrast-explanation of the size-effect is consistent with findings reported by Oyama, which were replicated and extended in the present experiment. Oyama obtained the maximal size-effect with a size-ratio of 2:1 when concentric *I*- and *T*-figures were used.[7] The results of the present experiment support Oyama's findings for non-concentric as well as for concentric circles. Oyama found the *FAE* to be a nearly linear decreasing function of the distance between *I*- and *T*-circles of constant size-ratio.[8] This experiment replicated Oyama's findings for a range of eight different size-ratios. The close correspondence between the data of this experiment and those of Oyama under markedly different conditions of observation provide strong support for the contrast-interpretation.

Oyama also reported that, for a given *I*:*T* ratio, the magnitude of the size-effect increases with *I*.[9] This finding was not replicated in the present experiment; *FAE* was uninfluenced by variation in *I*. Furthermore, most of the variance attributable to variation in *I*:*T* ratio was found in the $E_-$ condition of eccentricity (Fig. 2 C). A possible interpretation of this result is that height in the visual field functions as a cue to apparent distance and hence to apparent size, but only when the object is below the visual horizon. For the $E_+$ condition, then, distance, and hence size, would be indeterminate, with the result that the *I*-figure would have little or no effect on *T* through contrast.

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[7] Oyama, *op. cit.*, 196; Sagara and Oyama, *op. cit.*, 329.

[8] Oyama, *op. cit.*, 202; Sagara and Oyama, *op. cit.*, 334.

[9] Oyama, *op. cit.*, 199.

There is little information in the experimental literature which makes possible a resolution between the displacement- and contrast-hypotheses in *FAE*s not involving the size-effect. One experiment, by Fox,[10] was, however, so designed as to provide conditions which could elicit either displacement or contrast. Some of the results of the experiment can be explained only by contrast resulting from adaptation, while others are interpretable either in terms of displacement or of contrast. Other experiments are needed to provide tests of the two hypotheses for various stimulus-conditions.

## SUMMARY

An experiment was conducted to measure variation in *FAE* as a function of the eight ratios of the size of the *I*- to the *T*-figure. In addition, size of the *I*-figure (in retinal angle) was varied over three values; the *I*-figure was presented at five values of eccentricity in relation to the *T*-figure and in two different directions (up and down). The results of the experiment support the hypothesis that the major determinant of the size-effect is successive size-contrast, in which the critical stimulus-variable is the ratio of the size of the *I*- and *T*-figures. The typical inflection of the *FAE* (distance-paradox) was obtained for various conditions of eccentricity. Variation in the absolute size of the *I*-figure did not affect the shape or slope of the *FAE*-function. *FAE*s were, however, obtained only when the *I*-figure was either at the same level as or lower than the *T*-figure in an otherwise impoverished visual field.

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[10] B. H. Fox, Figural after-effects: "satiation" and adaptation, *J. exp. Psychol.*, 42, 1951, 317-326.

# VIGILANCE, AGE, AND RESPONSE-TIME

By WALTER W. SURWILLO and REGINALD E. QUILTER,
Baltimore, Maryland

The last decade has seen a revival of interest in the topic of attention,[1] particularly as it is concerned with the study of the efficiency in detecting small changes in the external environment. Interest centers on the failure of an observer to notice a perceivable signal and on the relation of such failure to various environmental and organismic conditions.[2] In discussions of theory arising from this work, the word 'attention' has in most cases been replaced by the term 'vigilance.' Mackworth adopted the latter from Henry Head, who used the term to describe a physiological and psychological readiness to react.[3] This approach ignores the classical problem of the description of conscious states in attention, but is firmly based on measures of performance. The use of an objective estimate of attention makes possible an examination of its role in various aspects of behavior.

One application of this approach to attention lies in the possible identification of the basis for certain performance-differences between people. Thus, for example, we may inquire whether vigilance is a factor or plays a role in the slowing of responses which has been reported as characteristic of older subjects.[4] The purpose of the present research was to investigate the

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* Received for publication October 22, 1963. From Gerontology Branch, Baltimore City Hospital. The writers gratefully acknowledge Mr. Richard Mathias' assistance with the measurements and with the analysis of the data.

[1] N. H. Mackworth, The breakdown of vigilance during prolonged visual search, *Quart. J. exp. Psychol.*, 1, 1948, 6-21; Researches on the measurement of human performance, *Med. Res. Council, Special Rep. No. 268*, 1950.

[2] Paul Bakan, Discrimination decrement as a function of time in a prolonged vigil, *J. exp. Psychol.*, 50, 1955, 387-390; C. H. Baker, Attention to visual displays during a vigilance task: II. Maintaining the level of vigilance, *Brit. J. Psychol.*, 50, 1959, 30-36; D. E. Broadbent, Noise, paced performance and vigilance tasks, *Brit. J. Psychol.*, 44, 1953, 295-303; James Deese, Some problems in the theory of vigilance *Psychol. Rev.*, 62, 1955, 359-368; H. M. Jenkins, The effect of signal-rate on performance in visual monitoring, this JOURNAL, 71, 1958, 647-661; Mackworth, Vigilance, *Nature (Lond.)*, 178, 1956, 1375-1377; P. D. McCormack, Performance in a vigilance task as a function of inter-stimulus interval and interpolated rest, *Canad. J. Psychol.*, 12, 1958, 242-246.

[3] Mackworth, The breakdown of vigilance during prolonged visual search, *Quart. J. exp. Psychol.*, 1, 1948, 6.

[4] See for example, Gilbert Forbes, The effect of certain variables on visual and auditory reaction-times, *J. exp. Psychol.*, 35, 1945, 153-162; Helene Gavini, Les temps de reaction simple chez les hommes et les femmes de 55 à 85 ans, in *Le vieillissement de fonctions psychologiques et psychophysiologiques*, Centre National

relation of vigilance to age, and to determine whether lowered vigilance is associated with age-related slowing in reaction-time.

### METHOD AND PROCEDURE

*Subjects.* The *S*s were 106 men, aged 22–82 yr. All were volunteers who were living in the community and had agreed to serve in a 3-day testing program at the Gerontology Branch of Baltimore City Hospitals. By and large, the *S*s were from the professions, principally scientists, engineers, educators, and administrators from the Baltimore-Washington area. Persons with visual disturbances or with impairment or disease associated with muscles involved in the reaction-task were not included in the group tested. Fifty-three of the *S*s were under 60 yr. of age, and an equal number were 60 yr. or older. Mean age of the young group was 43.7 yr., of the old group 71.0 yr.

*Apparatus.* Mackworth's Clock-Test was used.[5] The 'clock' was a metal box with a plain white face, 12 in. in diameter, and a single black pointer, 6 in. in length, mounted from its center. This pointer moved in discrete steps like the second hand of a large clock. The full circle contained 100 steps, each of which occurred once every second. No reference-points or scale-markings of any kind appeared on the white background behind the pointer. At long and irregular intervals, the pointer traveled through twice the usual distance in the same amount of time. These movements were referred to as 'double jumps' and, in the course of the 1-hr. test, 23 were presented. Since this corresponded to 23/3,600 or 0.64% of all the pointer-movements, a double jump was a rare event indeed.

The clock was identical in construction and operation to the one used by Mackworth,[6] with the exception of a few physical dimensions. Together with the fact that subject-to-clock distance in our case was different, these variations in construction produced differences in angle subtended at the eye by the movements of the pointer. In Mackworth's experiments, the ordinary movements of the pointer and the double jumps subtended angles of 12′ and 24′ respectively, while in our case they were 28′ and 56′. It was reasoned that larger angles would reduce the effects of small differences in visual acuity on the detection of double jumps. The clock was set upright on a shelf 4 ft. in front of *S* and was illuminated by a 40-w. lamp placed behind his head.

*Procedure.* The procedure consisted of: (1) an instructional and demonstrational session; (2) a practice session; and (3) the test-session. During the entire period, *S* sat comfortably in a bucket-type seat with his legs supported in a horizontal position. While he was performing, this area was surrounded by a white canopy which

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de la Recherche Scientifique, Paris, 1961, 97-105; W. R. Miles, Correlation of reaction and coördination speed with age in adults, this JOURNAL, 43, 1931, 377-391; W. D. Obrist, Simple auditory reaction time in aged adults, *J. Psychol.*, 35, 1953, 259-266; W. W. Surwillo, The relation of simple response time to brain-wave frequency and the effects of age, *Electroenceph. clin. Neurophysiol.*, 15, 1963, 105-114.

[5] Mackworth, The breakdown of vigilance during prolonged visual search, *Quart. J. exp. Psychol.*, 1, 1948, 7-11; Researches on the measurement of human performance, *Med. Res. Council, Special Rep. No. 268,* 1950, 12-13.

[6] Mackworth, The breakdown of vigilance during prolonged visual search, *Quart. J. exp. Psychol.*, 1, 1948, 7-11; Researches on the measurement of human performance, *Med. Res. Council, Special Rep. No. 268,* 1950, 12-13.

isolated him (and the clock) from the rest of the room. As nearly as possible, all extraneous noises were excluded from the room. The only sound emitted by the clock was a steady, faint drone of its motor. This sound mingled with the noise produced by another small motor that drove the oscillograph-chart upon which S's performance was recorded.

*(1) Instructional and demonstrational session.* The Ss were told they would be working at a 'watch-keeping' task and that it was a demanding one requiring their fullest attention and coöperation. After S had been comfortably settled in front of the clock, E started the mechanism and, together with S, observed the ordinary movement of the pointer. E told S that every now and then, at long and irregular intervals, the pointer moved twice the usual distance and that this movement was termed a double jump. After S learned to recognize the difference between the two kinds of signals, at least 10 double jumps were presented at different positions along the face of the clock and interspersed with ordinary movements. E watched the clock with S and prompted him whenever necessary to make sure that he understood the task. Persons who could not at this point clearly identify a double jump were excluded from the study.

*(2) Practice session.* The practice session was a short sample of the actual test, which lasted 9 min. and was designed to give S some idea of what to expect from the clock. S was informed that four double jumps would appear, and that his task was to press a response-key as quickly as possible when he recognized them. During this time he was left entirely by himself. Whenever S failed to press the response-key within 6 sec. after presentation of a double jump, E called out that he had missed one. E also informed him if he pressed in error. The response-key was a modified microswitch, held in the hand and actuated by pressing on a button with the thumb. Prior to the start of the practice session, all the Ss were instructed in its use.

*(3) Test-session.* A rest-period of at least 5 min. followed the practice session, at which time any small adjustments contributing to S's comfort were made. He was told that the test-session entailed watching the clock-pointer for 1 hr., in the course of which an unspecified number of double jumps would appear in random order. E asked him to press the response-key *as fast as he could,* but not to feel that it was ever too late to press, since we were interested in finding out how many double jumps he could detect during the session. False responses, it was pointed out, would not affect his score on the test. S was not permitted the use of a timepiece, and was told that E would neither communicate with him in any way nor answer any query, unless some emergency arose. After he completed the test, S was asked in an informal manner to comment on it and to relate any 'method' that was used.

The 23 double jumps presented in the hour-long session did not, in reality, occur at random. As was the case in Mackworth's experiments, they appeared according to a fixed program which repeated itself after ½ hr. Thus, the spacing of Signals 1-12 was the same as for Signals 12-23. The Ss were apparently unaware of this; after the test was over, many expressed the belief that the double jumps had indeed appeared in random order. Of those few who suspected a fixed program or pattern of double jumps, only one (S–36, age 49 yr.) was able to name correctly any part of it.

Whenever a double jump occurred, a switch automatically closed and fed a standard signal into an integrator which was, in turn, coupled to an ink-writing galvanometer. This signal ceased when *S* pressed the response-key, or after 6 sec. had elapsed. The galvanometer traced out spikes, 40 mm. in maximal height, on a moving chart, at the rate of four spikes per sec. As the cumulative amplitude of these spikes was proportional to the duration of the standard signal, response-time was determined by measuring them and converting the result into units of time. Accuracy of measurement was approximately ± 2 m.sec. If no response came from *S* within 6 sec. of the appearance of a double jump, it was assumed that he had missed that particular signal.

## RESULTS

*Relation of vigilance to age.* The number of double jumps detected by each *S* in the course of the 1-hr. watch was determined, and each divided by 23 to give the percentage detected. Averages were obtained for young ($< 60$ yr.) and old ($\geqq 60$ yr.) *S*s. The mean percentage detected by the young group was 72.9%, while the corresponding value for the old group was 64.4%. The difference of 8.5% was statistically significant ($t = 2.21$; $P < 0.05$), and we may conclude that, under the conditions of this experiment, old people were less vigilant than young people.[7]

To determine whether this difference was due to an over-all lower level of vigilance, or was the result of a more rapid decrease on the part of the older *S*s than the young during the hour-long vigil, the data were fractionated into ¼-hr. periods. Fig. 1 shows the average percentage of double jumps detected by the old and young groups in each ¼-hr. period. The points plotted reveal the characteristic drop in vigilance with time which has been reported on numerous occasions.[8] Since, in the first 15 min. of the test, the mean value for the old *S*s (78.3%) was not significantly smaller than that for the young group (82.3%), there is no evidence that in the early stages of the watch old *S*s were less vigilant than young. This is substantiated by the finding that scores for both groups on the practice session were identical (see Fig. 1). On the other hand, mean difference between first and fourth ¼-hr. periods was significantly greater for the old group than for the young (21.3% and 11.2% respectively; $t = 2.23$; $P < 0.02$). It appears therefore that, over a period of 1 hr., vigilance falls more rapidly in old persons than in young.

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[7] Although the distributions of the percentages of signals detected proved to be skewed, no transformations were made of the data. Use of the *t*-test was justified on the basis of recent evidence showing that, with sample sizes of the order encountered here, the *t*-test is "functionally nonparametric or distribution-free" (C. A. Boneau, The effects of violations of assumptions underlying the *t*-test, *Psychol. Bull.*, 57, 1960, 63).

[8] See Baker, Attention to visual displays during a vigilance task: II. Maintaining the level of vigilance, *Brit. J. Psychol.*, 50, 1959, 30.

*Relation of response-speed to vigilance and age.* Average reaction-time during the hour-long vigil was determined for each of the 106 *S*s. A Pearson product-moment correlation between reaction-time and the percentage of double jumps detected was computed and proved to be highly significant ($r = -0.45$; $P < 0.001$). The coefficient of determination, however, was only 0.20. Hence vigilance-level is of little practical value in prediction of *S*'s speed of response.

Examination of the reaction-times of the two groups yielded an unexpected result. Mean values for the whole session showed no reliable difference between young and old (*M*, young = 511 m.sec.; *M*, old = 530 m.sec.; $t_{\text{diff.}} = 0.61$). Comparison of the difference in reaction-times from the first to the fourth ¼-hr. periods also revealed that young *S*s were not superior to old *S*s. There was an increase of 137 m.sec. for the old group and an increase of 134 m.sec. for the young; the difference of 3 m.sec. was not statistically significant.

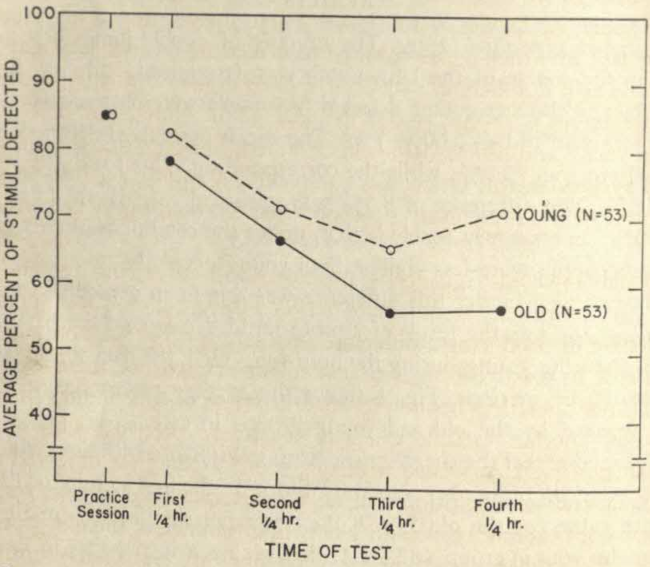

FIG. 1. DECLINE OF VIGILANCE DURING PROLONGED VISUAL SEARCH
From left to right, the *SD*s of the five points are as follows: Young *S*s, 16.2, 18.7, 28.4, 28.5, 24.7; Old *S*s, 17.0, 21.8, 26.3, 28.0, 27.1.

*Discussion.* In the initial stages of 'watchkeeping,' old people appear to be as vigilant as young. After working at the task for 45 min., however, vigilance declines to a lower level in the older group than in the younger. This finding suggests that there are age-differences in vigilance, and that these differences are probably referable to changes in the over-all capacity of the organism to respond rather than to differences in ability. In practical terms, this means that older individuals cannot be expected to keep watch for as long a period of time as younger *Ss*. Whether interpolated rest is capable of reducing this difference in the same way it dispels fatigue, remains to be seen.[9]

Our findings are in contrast to those reported by Griew and Davies and, more recently, by Davies and Griew.[10] These investigators made use of an auditory task in which *S* was required to detect the occurrence of any three consecutive odd numbers in a series of spoken digits. With 40-min. and (in the latter report) 75-min. tasks, they found no differences in vigilance between young and old *Ss*. It should be noted, however, that the old group studied by Davies and Griew was composed only of *Ss* aged 41-58 yr. In the context of the present investigation, their *Ss* could hardly be considered 'old.'

Although lowered vigilance is associated with advancing age and with longer reaction-times, the difference between the age-groups was apparently not effective in producing a difference in response-time. Since only 20% of the variance in reaction-time is accounted for by vigilance, there are clearly other factors operating beside vigilance. In view of this finding, it is easy to see how another variable which is negatively correlated with reaction-time, but positively correlated with age, could produce the observed result.

It is, of course, surprising that an age-associated slowing in response-time was not observed, especially since the mean age of the old group was 71.0 yr. We should note, however, that our task was clearly unlike the traditional reaction-time procedure. In the usual reaction-time experiment, *S* works only for short periods of time. A preparatory signal precedes the stimulus, which appears against a neutral background. In our study, there was no warning signal or foreperiod. *S* must continually search the field for the signal which appears against a background of 'noise,' and he must do this for 1 hr. without interruption. As might be suspected, this difference is reflected in the magnitude of the average reaction-times which, as reported here, are about 2.5 times larger than the usually-reported values. For this reason, our findings are, perhaps, not directly comparable with those in which the typical reaction-time procedure was studied.

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[9] In a watchkeeping task, Jenkins reports a significant improvement in percentage of signals detected from interrupting the task for 30 sec. every 5 min. (H. M. Jenkins, The effect of signal-rate on performance in visual monitoring, this JOURNAL, 71, 1958, 654-656.)

[10] Stephen Griew and D. R. Davies, The effect of aging on auditory vigilance performance, *J. Geront.*, 17, 1962, 88-90; Davies and Griew, A further note on the effect of aging on auditory vigilance performance: The effect of low signal frequency, *J. Geront.*, 18, 1963, 370-371.

It is worth noting, finally, that failure of the 19-m.sec. difference to reach significance is not attributable to an inordinately large variance on the part of the old group. Although variability tends to be greater in a group composed of older *S*s, *SD*s of reaction-time in our own case did not differ (167 m.sec. for the young *S*s, and 153 m.sec. for the old).[11]

## SUMMARY

Current research on the topic of attention has concerned itself largely with the matter of *S*'s readiness to react to a stimulus. This readiness has been named 'vigilance,' and is defined in terms of the efficiency with which small but perceivable changes in the external environment are detected. The present experiment studied the relation of vigilance to age, and investigated the question of whether differences in vigilance could account for the frequently-reported finding that old people are slower responders than young persons.

Fifty-three young ($M = 43.7$ yr.) and 53 old *S*s ($M = 71.0$ yr.), were tested for a period of 1 hr. on Mackworth's Clock-Test. It was found that the old *S*s were as vigilant as the young in the initial stages of 'watchkeeping' but, after 45 min. on the task, vigilance declined to a significantly lower level in the older group. Reaction-time and vigilance were significantly correlated, but age-differences in the latter variable were apparently not associated with corresponding differences in speed of response.

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[11] A. T. Welford, Psychomotor performance, in J. E. Birren (ed.), *Handbook of Aging and the Individual,* 1959, 576.

# THE VERBAL-LOOP HYPOTHESIS: CONVENTIONAL FIGURES

By MURRAY GLANZER, New York University, and WILLIAM H. CLARK, Walter Reed Army Institute of Research

A question of continuing practical and theoretical interest is the following: "What makes one stimulus simple, well-organized, easy to handle, while another stimulus is complex, poorly-organized, difficult to handle?" The verbal-loop hypothesis answers this question by making a specific and simple assertion concerning the *S*'s use of language and then by spelling out the implications of that assertion.[1] The answer differs markedly from the answers given by gestalt and information-theory approaches to perceptual organization.

According to the hypothesis, an *S* carrying out a perceptual task translates the input-information into words, stores these words, and then uses them as the basis for his final response. The hypothesis is schematized in Fig. 1. The hypothesis implies that the extent of the *S*'s covert verbalization (or translation) for a given stimulus-object is critical in determining the efficiency of his performance. For example, if the *S* uses a long verbalization for a given object, then a short exposure of the object will not permit the *S* to complete the translation. His final response is therefore likely to be incorrect. Since accuracy of performance with respect to an object often is used to define its 'organization' or 'simplicity,' the verbal-loop hypothesis offers a basis for reinterpreting and reanalyzing 'perceptual organization.'

In the preceding studies, the *Ss*' overt descriptions of the stimulus-objects were used to estimate the length of covert verbalizations to the objects; behavioral measures of their difficulty were used as indices of the organization or complexity of the objects. A strong relation between length of verbalization and stimulus-difficulty was shown for sets of binary numbers and for a systematically generated set of stimulus-objects of the type used in studies inspired by information-theory.

Two points should be established before generalizing broadly from the hypothesis: (1) that the relation between length of verbalization and stimulus-difficulty holds for more conventional types of figure—it might be argued that the stimulus-objects used in the preceding studies were rather

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[1] Murray Glanzer and W. H. Clark, Accuracy of perceptual recall: An analysis of organization, *J. verb. Learn. verb. Beh.*, 1, 1963, 289-299; Glanzer and Clark, The verbal loop hypothesis. Binary numbers, *ibid.*, 2, 1963, 301-309.

special and peculiar; (2) that the relation holds for some other index of stimulus-complexity than behavioral measures of difficulty—it might be argued that behavioral measures (latency, accuracy of reproduction) do not capture the meaning of the term 'complexity' as fully as the direct judgment of complexity by the *Ss*, expressed in ratings. The purpose of this investigation was, therefore, to determine whether the verbal-loop hypothesis furnishes a basis for predicting behavioral measures of complexity of a new set of stimulus-objects—conventional outline figures—and to determine whether the hypothesis furnishes the basis for predicting ratings of complexity.

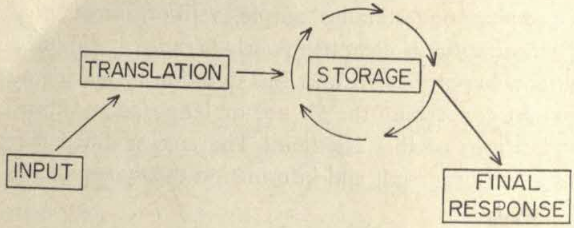

FIG. 1. SCHEMATIC OUTLINE OF THE VERBAL-LOOP HYPOTHESIS

## EXPERIMENT I

The first step was to determine the relation between length of verbalization and behavioral measures of complexity for a set of conventional line drawings. The figures and their behavioral measures were obtained from an earlier study.

*Materials.* The stimulus-objects were the 26 line drawings shown in Fig. 2, black against a light gray background. The figures are ranked according to behavioral measures obtained from a study by Fehrer.[2] In that study, 128 figures, including the 26 in Fig. 2, were presented at 8 different exposure-times, ranging from 40-680 m.sec. The 128 figures consisted of 112 outline drawings and 16 dot-patterns. Eight *Ss* viewed these figures repeatedly under the various conditions of exposure and attempted to reproduce them after each presentation. Each figure was assigned a composite score based on the number of repetitions needed for accurate reproduction at each exposure-time.

The set of 26 figures was obtained by starting with the easiest figure of Fehrer's set of 128 and selecting every fifth figure in order of difficulty. In four instances in which this procedure selected a dot-pattern, the line figure with the nearest difficulty-score was selected instead.[3] The rank of the figures from 1-26 is used in Fig. 2 and in the subsequent analyses as the index of figural complexity. In addition to the 26 figures, a set of five practice-figures also was chosen to cover the entire range of scores from very easy to very difficult.

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[2] E. V. Fehrer, An investigation of the learning of visually perceived forms, this JOURNAL, 47, 1935, 187-221.

*Equipment.* The figures were shown on a 4.5 × 6 ft. screen with a Keystone 511 35 mm. slide-projector in a semi-darkened room. The timing of the slide-exposure and the intervals between slides was controlled by a closed loop of tape on a Gerbrands tape-programmer.

*Procedure.* The *Ss* were told that the purpose of the task was to discover how well people use words or sounds to describe things they see. They were told to write descriptions of the figures shown that were brief but accurate enough to reproduce the figures on the basis of the description alone.

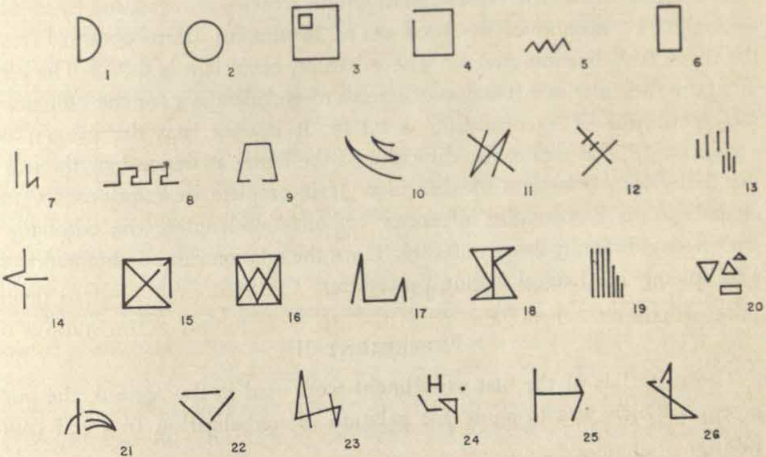

FIG. 2. THE TWENTY-SIX OUTLINE FIGURES
Ordered according to difficulty of reproduction.

First, the five practice-figures were shown in a random order for 1 sec. each. After the preview, the five figures, in a different random order, were each shown for 55 sec. during which the *Ss* wrote descriptions of the figure in a booklet containing lettered blanks. After all five practice-figures had been described, the *Ss* were asked to reproduce them using their verbal descriptions. The same general procedure was used for the main series of 26 figures—a 1-sec. preview of each figure in one order, a 55-sec. viewing and description period for each figure in another order. Solely to bear out the initial instructions, *E* then designated nine of the descriptions and instructed the *Ss* to reproduce the corresponding figures.

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[3] The dot-patterns were excluded beforehand because they seemed intuitively to belong to a different set than the line-figures. Private communication with Dr. Fehrer indicated that the dot-patterns had, indeed, presented special scoring problems.

*Subjects.* The *S*s were 100 college students, tested in groups of 37, 32, and 31. Each group viewed the figures in a different random order.

*Results.* Each of the 26 items was scored simply by counting the number of words used by *S* in describing the figure. Abbreviations or symbols were counted as single words. The mean number of words used to describe each figure was defined as the figure's verbalization-length. There were 26 instances in which *S* either made no response or made an obviously incomplete response. In those cases, the mean length of verbalization was computed on the basis of the number of *S*s making complete responses.

The reliability of the verbalization-length scores was measured by means of analysis of variance of the three sets of 26 stimulus-means obtained from the three verbalization-groups.[4] The reliability coefficient is 0.983. The plot of figure-difficulty as a function of length of verbalization for the 26 figures was rectilinear. The correlation is 0.810. It is clear that the longer the verbalization, the greater the difficulty of the figure as defined by the original behavioral measures of difficulty. If incomplete descriptions are included in the computation of mean verbalization-lengths, the correlation with figure-difficulty drops to 0.804. Thus, the relation can be obtained with a completely mechanical scoring procedure.

## EXPERIMENT II

The materials of the first experiment were used in the second, the purpose of which was to study the relation of verbalization to rated complexity.

*Procedure.* The *S*s were told that they were to judge the 'complexity' of a series of designs and were instructed in the use of seven-point rating forms that had the following headings: *extremely simple, very simple, simple, medium, complex, very complex, extremely complex.* No definition of the term 'complexity' was given. Following the instructions, there was a preview of the three practice-figures, which were shown in a random order for 1 sec. each. Then the three figures were each shown in a different random order for 15 sec. during which the *S*s rated each one. The same general procedure was used for the main series of 26 figures—a 1-sec. preview of each figure, a 15-sec. period for viewing and rating each figure in another random order. After the 26 figures had been rated once, they were again shown for 15 sec. each, in a different random order, and rated a second time.

*Subjects.* The *S*s were 216 college students, tested in two groups—one group of 103 and one of 113. The stimuli were randomized independently for each group.

*Results.* The mean of all the *S*s' ratings was assigned to each of the 26 figures as its complexity-score. The reliability of these ratings, measured by correlating the 26 mean ratings of complexity obtained from one group against the 26 means obtained from the other group, is 0.990. The plot of the mean ratings of complexity as a function of mean length of verbalization is rectilinear. The correlation is 0.865, showing that length of verbalization is an efficient predictor of rating of complexity. The correlation of rating of complexity and difficulty of figure also was computed. The correlation of 0.743, although high, is not as high as the correlation of either of these variables with length of verbalization.

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[4] B. J. Winer, *Statistical Principles in Experimental Design,* 1962, 124-132.

## DISCUSSION

The results extend the generality of the verbal-loop hypothesis. They show that the hypothesis can account for differences between conventional figures and that, with these stimulus-objects, it can account both for behavioral measures of complexity and for ratings of complexity. The hypothesis affords a more general and powerful approach to perceptual organization than does either gestalt- or information-theory. Gestalt theory has not yet yielded a set of well-defined measures that can account systematically and parsimoniously for the differences in such figures as those studied here. Information-theory, although it has made use of a set of well-defined measures, has been able to deal only with specially constructed stimulus-objects. Even with those, its success has been slight. Recently, investigators have found it necessary either to bring in additional concepts from outside the theory, *e.g.* Miller's 'chunking,'[5] or to reinterpret and restructure the approach as originally set fourth, *e.g.* Garner's redefinition of redundancy.[6] The verbal-loop hypothesis leads to a simple, general, easily obtained measure of 'organization' that is derived from a specific statement about the psychological mechanism involved in perception. It can be argued further that length of verbalization measure exhausts the meaning of the term 'organization,' and that the perceptual process can be considered more clearly without using the term.

Recent studies by other investigators, concerned not with form-perception but with memory for visual stimulus-figures, give independent support to the hypothesis. Conrad has shown that visually presented letters which are confused in short-term recall are also letters that are confused during noisy auditory presentation.[7] He deduces from this finding that the

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[5] G. A. Miller, The magical number seven, plus or minus two: Some limits on our capacity for processing information, *Psychol. Rev.*, 63, 1956, 89-97.

[6] W. R. Garner, *Uncertainty and Structure as Psychological Concepts*, 1962, 138-212.

[7] R. Conrad, An association between memory errors and errors due to acoustic masking of speech, *Nature*, 193, 1962, 1314-1315.

*S*s "repeated the letters sub-vocally." Sperling has been led by his work on decay of memory for visually presented letters to postulate an "auditory information storage-rehearsal loop" that closely resembles the verbal loop discussed above.[8]

The relation between the ratings of complexity and length of verbalization suggests an interesting and specific mechanism for rating. According to the verbal-loop hypothesis, *S* views the figure and then translates it into words. If he is carrying out a perceptual task, *e.g.* recognition or reproduction of the figure, *S* stores these words and then uses them to make his response. If the final response required is a rating, then it may be assumed that *S* responds to the length of his stored translation in making his judgment of complexity. One implication of this statement is that training *S* to make different verbal responses to a given stimulus-object will change his judgment of its complexity.

In this paper, the terms 'complexity' and 'simplicity' are used as equivalent to the terms 'poorly-organized' and 'well-organized.' A case can, of course, be made for distinguishing these two sets of terms. There is little reason to believe, however, that the ratings of the figures would have been much different if the instructions had been phrased in terms of 'well-organized,' *vs.* 'poorly-organized.' The latter terms were not used because it was thought that they would be less clear to the *S*s, *i.e.* would generate more questions during the instructional period. The behavioral measure of difficulty is, of course, neutral with respect to these two terms.

## SUMMARY

The verbal-loop hypothesis states that perceptual processing includes covert verbalization and that the length of verbalization determines the 'complexity' of the stimulus-object. Using overt descriptions to estimate the covert length of verbalization, it was possible to account for two measures of the complexity of conventional outline figures. One measure was a behavioral one—the accuracy of reproduction after brief exposure. The other measure was based on ratings of complexity. The data support the hypothesis as an alternative to Gestalt- and information-theory approaches to perceptual organization.

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[8] George Sperling, A model for visual memory tasks, *Human Factors*, 5, 1963, 19-31.

# THE EFFECT OF BURSTS OF NOISE ON AN ARITHMETIC TASK

By Muriel M. Woodhead, Cambridge, England

As modern technology advances, more people are being subjected to loud noise during the course of their work. The detailed effects of auditory distraction remain unsettled, although the balance of evidence suggests that loud sound distracts attention from complex tasks such as calculating. Previous researches have shown that noise can impair performance in arithmetic,[1] although in two studies arithmetical tests were unaffected by noise.[2] The conflicting results are presumably due to differences in experimental conditions and in techniques of analysis, and can nearly be resolved by considering the intensities of the noises used. Below 90 db. there were no reported effects; above this level only one experiment by Wilbanks, Webb, and Tolhurst did not show any significant effect.[3]

The present study was intended to investigate changes in the performance of arithmetic following a 1-sec. burst of noise at a sound-pressure level of 100 db. re 0.0002 microbar. The task required a number to be memorized from a visual display; this number was succeeded by a smaller one, which remained in view until it had been subtracted from the number committed to memory. When noise occurred while a number was being memorized, it would certainly be loud enough to compete for the man's attention. If it did so successfully, during a limited but normally adequate time available for memorizing, the number might not be learned correctly, in which case the subsequent calculation would be wrong. The time for the actual calculation could not be limited; otherwise too many omissions might occur.

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* Received for publication June 3, 1963. This study, from the Applied Psychology Research Unit of the British Medical Research Council, was directed by Mr. Donald E. Broadbent, and the subjects and equipment were provided by the Royal Navy.

[1] M. S. Viteles and K. R. Smith, An experimental investigation of change in atmospheric conditions and noise upon performance, *Trans. Amer. Soc. Heat and Vent. Engineers*, 52, 1946, 1291; T. D. Hanley and R. J. Williamson, The effect of various noise levels on performance of three mental tasks, *U.S.O.N.R.*, Tech. Rept. S.D.C., 104-2-21, 1950; D. E. Broadbent, Effect of noise on an intellectual task, *J. acoust. Soc. Amer.*, 30, 1958, 824-827.

[2] E. V. Mech, Factors influencing routine performance under noise, *J. Psychol.*, 35, 1953, 283-298; W. A. Wilbanks, W. B. Webb, and G. C. Tolhurst, A study of intellectual activity in a noisy environment, *U.S.N. Sch. Av. Med.*, NM .001 .104 .100 Rept. 1, 1956.

[3] Wilbanks, Webb and Tolhurst, *op. cit.*, 1956.

It seemed probable that when noise occurred during this calculating period it would affect the time taken for the calculation rather than its accuracy. More time would be taken if a brief period of inactivity followed the burst of noise, as previously found with a decision-making task,[4] or less time would be taken if the noise helped to maintain alertness or aroused some feeling of urgency.

These predictions were tested by two experiments. In the first, a single burst of noise occurred during each memorizing period, and the results were compared with a quiet control condition, to determine whether divisions of attention between the noise and the visual display of numbers reduced the accuracy of the subsequent calculation. In the second experiment memorizing was always carried out in quiet, followed by a single burst in each calculating period, and the results compared with the control to investigate any effect on the time needed for calculation. Noise was thus presented in an activity which was paced and a different activity which was unpaced, and therefore the comparisons of results were always between an N condition and quiet.

## METHOD

*Subjects.* The *Ss* were 84 young Royal Naval enlisted men, all with normal hearing.

*Task.* The task was adapted from an Arithmetic Test devised by Broadbent.[5] A six-digit number was projected onto a screen in front of *S;* he was instructed to memorize this number. The average memorizing time required by Broadbent's *Ss* was approximately 9 sec.; with this in mind, 10 sec. were allocated as viewing time. The six digits were then replaced by a four-digit number which stayed on the screen until *S* had mentally subtracted it from its predecessor, written down the remainder, and signalled completion by pressing a bell-push. He was not allowed to write anything except the answer. The time taken was recorded and after a 15-sec. interval the next six-digit number appeared. The whole series employed only the integers 1, 2, 3, 4; as for example a visible 2443 to be subtracted from a memorized 324412. This digital limitation actually makes the task more confusing, not easier as might be supposed. To make the problems about equally difficult, 'borrowing' was always required in three columns of a subtraction.

*Procedure.* Within each experiment there were two groups, consisting of 21 *Ss* each. Group N had the noise; its control, Group C, worked in quiet. In the AH.4 Intelligence Test there was no significant difference between any of the groups.[6] Every *S* of Group N was paired with an *S* of Group C who had the same order of problems. For the *Ss* of Group $N_1$, the burst of noise began 4 sec. after the first number appeared, during the memorizing time. For those of Group $N_3$, the burst of noise began 5 sec. after the display had changed, while the answer was being calculated. For the *S*s of Groups $C_1$ and $C_2$ no noise was presented. (Although it was not essential to have two identical control groups, the readily available *S*s made the additional precaution seem worthwhile.)

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[4] M. M. Woodhead, Effect of brief loud noise on decision-making, *J. acoust. Soc. Amer.,* 31, 1959, 1329-1331.

[5] Broadbent, *op. cit.,* 825.

[6] A. W. Heim, Manual for the Group Test of General Intelligence AH.4, London: *Nat. Found. Educ. Res.,* 1955.

*S* sat alone in a small cubicle fitted with two loudspeakers, one overhead and the other in front at knee-level, for the presentation of sound in the N conditions. He was told "in this experiment we are interested in accuracy, memory, and speed." He practiced until achieving three correct answers, and was told the time taken for each one of his practice calculations. In the test, 40 different subtractions were given, and this was repeated on the two following days. The same 120 questions were used for all the *S*s, in counterbalanced orders within each experiment. The *S*s of Group N were informed that every question would be accompanied by a burst of sound and two sample bursts were presented. This procedure meant that on the first day the N-groups heard 42 identical bursts, and on each of the second and third days 40 bursts. By the third day, it was believed that the noise would not attract attention simply on account of its unfamiliarity (which would have required a different hypothesis), although this might have been so for the first day. It was in the results from the third day, when the *S*s were most at ease, that the interest lay.

*Noise.* The noise, which lasted approximately 1 sec., was a recorded rocket firing consisting mainly of low frequencies. The spectrum has been described previously,[7] and was measured in the reverberant cubicle used to test the *S*s. The peak intensity level was 100 db.

*Treatment of data.* To calculate the over-all differences between a noise condition and its control, the performance of each *S* of Group N was compared with that for the particular *S* of Group C who had the same order of test-problems. The reliability of the difference in the trend of calculating speed between a noise and quiet condition was determined by taking the mean difference in performance between each N and C pair for each quarter (10 problems) of the test, and computing the value of $3(Q_1 - Q_4) + (Q_2 - Q_3)$ where $Q$ is the relevant quarter part.

## RESULTS

The results of the last test-day are given in Table I. It was possible that intermittent noise might increase variability in time.[8] Thus the mean standard deviation for the times to calculate answers shows the average variability in time of individual *S*s; the standard error shows the differences between subjects. The standard error of the mean, $SD_M$, indicates the size of the individual differences in variability. The changes in rates of calculating during the test sessions of each condition on the last day are shown graphically in Fig. 1.

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[7] Woodhead, Value of ear defenders for mental work during intermittent noise, *J. acoust. Soc. Amer.*, 32, 1960, 682. See Fig. 2.

[8] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 29, 1918, 129-143.

*Effects of noise during paced memorizing.* As indicated in Table I, Group $C_1$ was more accurate than $N_1$ ($P < 0.05$). This was not due simply to the very good performance of $C_1$, since $C_1$ and $C_2$ combined were also more accurate than $N_1$ ($P = 0.03$, 1-tailed test). The difference in accuracy was general, and not related to particular sections of the test. Within the $C_1$ group the more accurate *Ss* tended to take shorter times for their calculations than those who were less accurate, Kendall's tau = 0.26 ($P = 0.05$, 1-tailed test; see also the results for $C_2$ below). The corresponding tau for $N_1$ was approximately 0.07 which was quite insignificant.

There were no reliable differences between Groups $N_1$ and $C_1$ either in the mean time taken for the calculations, or in the average variablility of time taken. The mean *SD* of $N_1$ did exceed that of $C_1$, but it was less than that of control Group $C_2$. The slopes of the functions in Fig. 1 did not differ between the $N_1$ and $C_1$ conditions.

TABLE I

RESULTS OF NOISE OCCURRENCE DURING MEMORIZING ($N_1$) AND CALCULATING ($N_2$) AND RESULTS FOR THE SAME TASK IN QUIET

| Groups of *Ss* | % correct | | Calculating times (sec.)<br>Average time taken | | Average variability in time taken | |
|---|---|---|---|---|---|---|
| | Mean | *SE* | Mean | $SE_M$ | $SD_M$ | *SE* of $SD_M$ |
| $N_1$ | 68* | 4.48 | 27.0 | 1.79 | 7.6 | 0.61 |
| $C_1$ | 81 | 2.83 | 26.8 | 1.98 | 6.69 | 0.69 |
| $N_2$ | 75 | 4.08 | 28.7 | 1.96 | 8.46 | 0.97 |
| $C_2$ | 72 | 4.78 | 29.6 | 2.42 | 8.51 | 0.98 |

* Control more accurate, $P<0.05$.

*Effects of noise during calculating.* The over-all time required for calculating and the average variability in time, were not appreciably different for Groups $N_2$ and $C_2$ (Table I). Fig. 1, however, shows that the *Ss* of Group $N_2$ had the larger reduction in calculating times during the progress of the test ($P < 0.05$). The difference in slope was still reliable when $N_2$ was compared with the pooled results of $C_1$ and $C_2$. Further analysis revealed that this improvement applied only to those problems which were answered correctly ($P < 0.01$).

Accuracy was not different in noise compared with quiet nor was there any reliably different change in accuracy as the run proceeded. Time and accuracy, however, were correlated in $C_2$, the higher scorers again answering with greater speed. The correlation for $C_2$ showed a Kendall tau of

0.43 ($P = 0.007$). Within $N_2$ the correlation was insignificantly in the opposite direction. Thus speed and accuracy were related in both control groups, but not in either noise group.

## DISCUSSION

The main results are reasonably compatible with the pretest predictions.

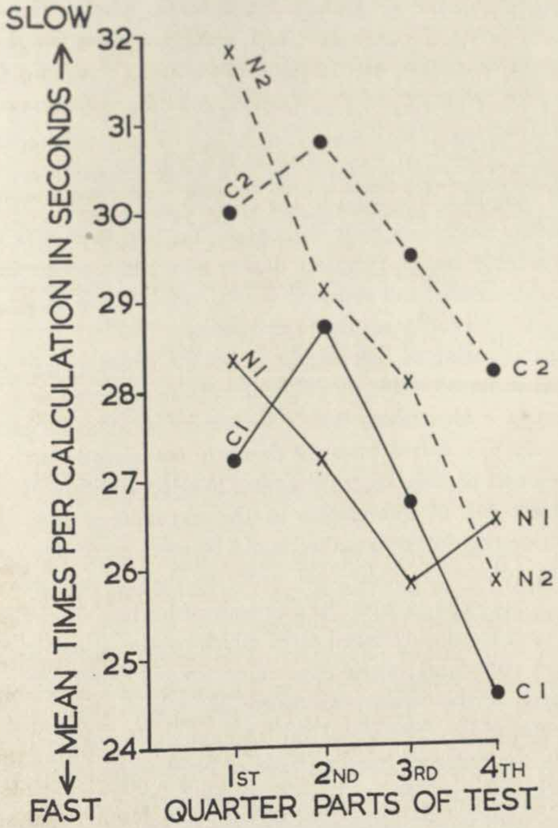

FIG. 1. CHANGES IN THE AVERAGE RATE OF CALCULATION DURING THE LAST TEST

The results of Group $N_1$ show that if the burst of noise occurred while *S* was attempting to memorize the six-digit number visible for a limited period, his subsequent calculation was more likely to be wrong. By the third day the number was probably inadequately learned rather than completely forgotten, since less than 1% of responses were omitted. It seems reasonable to infer that performance suffered because the strong auditory stimulus caused a division of *S*'s attention which would have otherwise been on the display.

The performance of Group $N_2$ indicates that noise during the calculations produced an initial slowness, but an increasingly fast rate of work as the test proceeded, without affecting the variability of the *Ss*' calculating times or their accuracy. It was not unexpected that this increasing speed applied only to the calculations which were correct; in many activities wrong answers frequently fail to show as consistent a trend as right ones. A different effect on speed of performance in noise compared with quiet was reported by Broadbent, who found, in a shorter version of the subtraction-task performed by Naval *Ss*, that work slowed down in noise.[9] This trend may have been due to the type of auditory stimulation, which was continuous machinery noise throughout the test. Even a familiar burst of noise probably carries a greater quality of urgency or dominance than steady, machine noise, and repeated bursts may continue to act as a series of arousing stimuli for longer than steady noise.

The general nature of these findings fits the sensory input model suggested by Broadbent.[10] This model proposes the existence within the nervous system of a filter which selects sensory information necessary for the task, but which is biased towards channels not recently active. For example, the onset of noise causes the filter to select auditory information at the temporary cost of information of the non-auditory task. Any consequent deterioration in performance could be more marked when working time is not adjusted to individual needs so that an operator is already making a compromise to meet the demands of his task. If the filter model is a valid one, then many paced tasks which require visual learning in an environment containing intermittent noise, should demonstrate the interference effect. In the current experiment $N_1$, a burst of intense noise did compete perceptually. The visual information was temporarily rejected or confused in the restricted learning time of $N_1$, causing errors in the calculation which followed. The auditory stimulus in $N_2$ was not in perceptual competition with visual learning; it appears to have served as an arouser, resulting in increasing speed of calculation, but not in more errors. The scope of these experiments is limited, but as far as they go it seems that, compared with quiet, the nature of the effect of a brief loud noise on mental subtraction depends upon whether the noise occurs while the man is taking in information displayed for a limited time, or whether it occurs while he is calculating.

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[9] Broadbent, *op. cit.*, 826.

[10] Broadbent, *Perception and Communication*, 1958, 85-86.

## SUMMARY

Bursts of 100 db. noise were found to affect performance in arithmetic. In each of 40 problems the *Ss* were required to memorize a 6-digit number from a visual display and then to subtract from it a visible 4-digit number. In comparison with quiet conditions, the occurrence of a brief noise while the numbers were being learned produced a tendency to get the subsequent calculation wrong. When the noise occurred during the calculating period, the rate of work increased throughout the session from a rather slow start. Thus the effects of a burst of noise depend upon its relationship with different parts of the task. An occurrence during perception and learning does not have the same effect as one during calculation.

# EFFECTS OF PAIRING-TIME AND TEST-TIME ON PERFORMANCE DURING AND AFTER PAIRED-ASSOCIATE TRAINING

By SLATER E. NEWMAN, North Carolina State College

Rate of paired-associate learning and rate of presentation during training have been shown to be inversely related.[1] In these experiments pairing-time and test-time were varied together. Thus, it was not possible to determine whether the obtained facilitation derived from lengthening the pairing-interval, the test-interval or both. Recently, Bugelski has shown that as pairing-time is increased (with test-time held constant), the number of trials to learn a paired-associate list decreases.[2]

The present experiment studied the effect of pairing-time and of test-time on performance during and immediately after paired-associate training. The length of the pairing-interval and of the test-interval were independently varied. A pairing-test procedure was used, each of the 12 pairing-trials being followed by a test-trial. The main dependent variables were the number of correct responses during the 12 test-trials and during a Test 13 administered immediately after paired-associate training.

*Method: (1) Subjects.* Eighty undergraduates at North Carolina State College served as *Ss*. The first 16 were women; the next 64 were men. A balanced Latin square was used in assigning *Ss* to treatments.

*Materials.* Twelve trigrams of pronounceability value less than 3.50 were used to construct a six-pair list.[3] No two terms began with the same letter. The pairs are TUD-VER, FID-ZUN, HOV-SUL, REL-JAD, LAR-BOT and MOG-DIR.

*Procedure.* A pairing-test procedure was used during paired-associate training.

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* Received for publication June 11, 1963. Preparation of this report was supported by Office of Naval Research Contract Nonr 486(08). My thanks are due Mrs. Linda Marsh Green, who collected the data, Mr. Alfred Forsyth who assisted in the analyses of them, and Dr. I. W. Drewes, who suggested certain of the analyses.

[1] A. E. Goss, C. H. Morgan and S. J. Golin, Paired-associates learning as a function of percentage of occurrence of response members (reinforcement), *J. exp. Psychol.*, 57, 1959, 96-104; L. W. Gregg, A. P. Chenzoff, and K. R. Laughery, The effect of rate of presentation, substitution and mode of response in paired-associate learning, this JOURNAL, 76, 1963, 110-115; C. I. Hovland, Experimental studies in rote-learning theory: VIII. Distributed practice of paired-associates with varying rates of presentation, *J. exp. Psychol.*, 39, 1949, 714-718.

[2] B. R. Bugelski, Presentation time, total time, and mediation in paired-associate learning, *J. exp. Psychol.*, 63, 1962, 409-412.

[3] B. J. Underwood and R. W. Schulz, *Meaningfulness and Verbal Learning*, 1960, 371-372.

There were 12 pairing-trials, each followed by a test-trial. On the pairing-trials each of the pairs was presented; on the test-trials each of the stimulus-terms was presented alone, *S* being required to spell its appropriate response-term. For half the *Ss* a 2-sec. rate was used during the pairing-trials, for the other half a 4-sec. rate. One-half the *Ss* under each of these conditions had a 2-sec. test-rate, and the other half a 4-sec. test-rate. The intertrial interval was 4 sec. for all *Ss*. (Each group is named by a two-digit number; the first stands for length of the pairing-interval, the second for length of the test-interval.) Three different pairing-orders and three different test-orders were used to reduce the likelihood that serial position would serve as a cue.

After the twelfth test-trial, all of the *Ss* were given one more test. For half the *Ss* under each of the four conditions, the thirteenth test was at a 2-sec. rate; for the other half the rate was 4 sec.

*Results: (1) Trials 1-12.* The means for correct responses during each three-trial block are presented in Table I. Analysis of variance (2 × 2) of these data gave significant *F*s for Pairing-Time ($F = 8.31$, $df = 1, 76$, $p < 0.01$); Test-Time ($F = 7.58$, $df = 1, 76$, $p < 0.01$); Trial-Blocks ($F = 254.49$, $df = 3, 228$, $p < 0.01$); and Pairing-Time × Trial-Blocks ($F = 4.41$; $df = 3, 228$, $p < 0.01$). None of the other *F*s was significant ($p > 0.05$). The significant interaction of Pairing-Time × Trial-Blocks appears to be due to the fact that Pairing-Time was not significant during the first block of trials but was for the remaining blocks.

TABLE I

MEAN CORRECT FOR EACH THREE-TRIAL BLOCK AND FOR TEST 13

| | Three-trial block | | | | | |
|---|---|---|---|---|---|---|
| Group* | 1–3 | 4–6 | 7–9 | 10–12 | Total | Test 13 |
| 22 | 1.30 | 4.45 | 7.30 | 8.55 | 21.60 | 3.20 |
| 24 | 1.65 | 5.25 | 8.55 | 11.20 | 26.65 | 3.30 |
| 42 | 1.25 | 5.50 | 8.75 | 11.55 | 27.05 | 3.90 |
| 44 | 3.15 | 8.45 | 12.75 | 14.70 | 39.05 | 4.65 |

* The first figure of the group-numbers indicates the length of the pairing-interval; the second, the length of the test-interval.

For each *S* the proportion of errors that were omissions, substitutions, and inventions was determined. The means for each group are presented in Table II. An angular transformation was applied to each of these distributions, and analyses of variance applied to the transformed scores. The *F*s for Test-Time on omissions and inventions were both significant ($p <$ 0.01), but on substitutions the *F* for Test-Time was not significant ($p >$ 0.05). For the groups given a 4-sec. test-rate the inventions comprised a higher proportion of errors and omissions a lower proportion than for those given a 2-sec. test-rate. Neither the *F* for Pairing-Time nor for the interaction of Pairing-Time and Test-Time was significant ($p > 0.05$).

The trial by which each of the six response-terms was given at least once was determined for each *S*. The medians for the four groups are: 22-12+, 24-12+, 42-10.5 and 44-8.5.[4] A median test for higher-order designs was applied to these data. Only the effect of Pairing-Time was significant (Chi square = 9.8; $df = 1$; $p < 0.01$).

For the pairs that were given correctly, the percentage of those for which the first occurrence of a response-term was to its own stimulus-term was determined for each group. For Group 22, the percentage was 75.9; for Group 24, 85.1; for Group 42, 84.4; and for Group 44, 91.4. The number of pairs for each group on which these percentages are based was 87, 101, 96, and 105 for Groups 22, 24, 42, 44, respectively.

TABLE II

PROPORTION OF TOTAL ERRORS DURING TRAINING FOR THREE TYPES OF ERROR

| Group | Errors |  |  |
|---|---|---|---|
|  | omissions | substitutions | inventions |
| 22 | .872 | .057 | .071 |
| 24 | .764 | .091 | .145 |
| 42 | .864 | .065 | .071 |
| 44 | .802 | .068 | .130 |

*(2) Test 13.* An analysis of the data for number correct during the first 12 trials showed: (1) that in Group 22, the *S*s who had the 2-sec. rate on Test 13 had done significantly ($p < 0.01$) better during training than the *S*s who had had the 4-sec. rate on Test 13; and (2) that in Group 44, the *S*s who had had the 4-sec. rate on Test 13 had done significantly ($p < 0.05$) better during training than the 2-sec. *S*s. Therefore, a 2 × 2 analysis of variance (Pairing-Time × Test-Time) was applied to the data for Test 13. Only the *F* of 6.00 for Pairing-Time was found to be significant ($df = 1, 76$, $p < 0.05$). The means for number correct during Test 13 are presented in Table I.

*Discussion.* Increasing the pairing-interval from 2 to 4 sec. led to earlier emission of response-terms from the list and a concomitant increase in the number of correct responses during paired-associate training. Type of error was not affected.

When the test-interval was lengthened from 2 to 4 sec., response-terms from the list were again likely to be given sooner and to be given correctly sooner. Omissions decreased and inventions increased. With a 4-sec. test-interval, *S* was more likely (than with a 2-sec. interval) to respond either correctly with a response-term from the list, or incorrectly with a response-term from outside the list.

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[4] The '+' for Groups 22 and 24 signifies that more than half the *S*s in these groups did not give each of the six response-terms at least once during training.

Of particular interest are the data from the Test 13. Combined with the data from the first 12 trials, they show that increasing the pairing-interval facilitated both learning and performance, whereas increasing the test-interval facilitated only performance. Thus, the *Ss* with a 4-sec. test-interval did not learn more pairs than those with a 2-sec. test-interval even though they responded correctly more often during training than the *Ss* with the shorter test-interval.

Bugelski has hypothesized that "in at least some areas of memorization, and under some conditions of presentation, the degree of learning will be a function of total time, regardless of the duration of the individual trials or inter-item times."[5] He has presented some data showing that total time (Presentation-Time × Trials) to learn a paired-associate list was constant, whether the pairing-interval was 2, 4, 6, 8, or 15 sec.

Bugelski has hypothesized that "in at least some areas of memorization, the performance of groups having equal test-times during paired-associate training. Group 22 had been in training for 336 sec. at the beginning of Test 11, and Group 42 at the beginning of Test 8. The mean for Group 22 on Test 11 was 3.00, and for Group 42 on Test 8 it was 3.05. These data are strikingly in accord with Bugelski's hypothesis.

The hypothesis is less strongly supported by the comparison of Groups 24 and 44. At the beginning of Test 9, Group 24 had been in training 368 sec; Group 44 had been in training 364 sec. at the beginning of Test 7. Despite the slightly shorter time during training for Group 44 its mean on Test 7 was 4.10, whereas the Group 24 mean on Test 9 was 3.25. This difference, in means, however, was not significant ($F = 2.67$, $df = 1, 38$, $0.20 > p > 0.10$).

## SUMMARY

All *Ss* were given 12 pairing-trials, each followed by a test-trial. Pairing-time and test-time were independently varied at 2 and 4 sec. An increase in pairing-time and test-time were each found to facilitate performance during paired-associate training. Only the increase in pairing-time facilitated performance on a test immediately following paired-associate training.

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[5] Bugelski, *op. cit.*, 409.

# APPARATUS

## A DELAYED FEEDBACK SELF-TRACKING AND RECORDING INSTRUMENT

By B. J. Baldridge, University of Cincinnati

In 1955 a brief report was published by Adelson on a method of self-tracking with delayed feedback.[1] Adelson pointed out some of the advantages of using this technique in analyzing the tracking characteristics of human *Ss*. Essentially, the method consists of combining *S* with some inert components to form a closed, self-exciting, feedback system.[2] In this arrangement, *S* is engaged in tracking a target while his own output is fed back, after a fixed time-delay, to become the subsequent target.

If external forces are prevented from entering, this closed system provides both the input signal and the output motion and any resulting action must be a function of the system itself. In psychological terms, *S* provides both the stimulus and the response; hence, the response must be a function of *S* alone and represents a natural or preferred pattern of responding.[3]

Theoretically, if such a closed feedback system were stable, it would remain indefinitely in motionless equilibrium. To prevent this, an external disturbing force is introduced to set the system into motion and then it is withdrawn. Adelson reported, and subsequent study has shown, that the nature of the disturbing force has little effect on the final results and could be eliminated entirely by having *S* provide his own initial disturbance.

In his report, Adelson stated the instrument which he used was crude, and it is likely that limitations of this kind had some effect on his results. The instrument to be described here was developed to be a simple, yet refined, delayed feedback self-tracking device to provide information about the natural or preferred tracking characteristics of the human *S*. The mass and friction of the inert components were kept to a minimum to reduce their influence on the performance of the over-all system. The apparatus was designed to be compact, easy to operate, and to use inexpensive unlined paper rolls.[4] Its construction was simple enough to be duplicated in any small machine shop.

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[1] This instrument was developed for use in a Doctoral study directed by Professor George L. Kreezer at Washington University.

[2] Melvin Adelson, Some observations on a delayed feedback self-tracking task, in Henry Quastrel, (ed.) *Information Theory in Psychology*, 1955, 365-367.

[3] A concise presentation of feed-back concepts and servotheory is given by G. L. Kreezer, Servo theory, in I. A. Greenwood, J. V. Holdam, and D. Macrae, (eds.), *Electronic Instruments*, 1948, 225-355.

The instrument, essentially, is a specially designed combined tracking and recording device. Its purpose is to provide a single dimensional, tracking-task, with the initial target (the pattern to be followed) provided by *E*, and with arrangements for *S*'s response to be fed back in such a way that he would subsequently track his own sequence of responses. The initial target can be any arbitrary function with a fundamental period of 8 sec. Feeding back *S*'s response, in effect, serves to modify the initial pattern by the amount of the tracking error. This may be stated clearly in the following equation: Initial target + Error = Response = New target. The tracking portion of the instrument is similar to many in use. A continuous paper tape, 5-in. wide,[5] is pulled at the rate of 6 in. per second past a narrow rectangular opening which is perpendicular to the direction of the movement of the tape. A heavy ink-line drawn on this tape gives the appearance of a short moving line segment or spot within the narrow opening. Tracking is done by means of a small, movable carriage mounted directly behind the rectangular opening. This carriage is fitted with a small knob for gripping and a black tipped plastic pointer used in following the moving spot. The carriage is so constructed that it moves freely and yet is rugged enough to support the weight of *S*'s arm while tracking. The weight of the carriage is less than one and one-half ounces and the coefficient of friction small enough for the marriage to roll freely down an incline of one degree. The top of the recorder is a smooth, flat surface on which *S* may rest his arm at the elbow. It also supports the hand on the carriage itself.

*S*'s task is simply to follow every movement of the line as precisely as he can.

The recording portion of the instrument is extremely simple. A Leroy type lettering pen is attached directly to the tracking carriage and follows every movement the carriage makes. In this way, the exact movement of the carriage is recorded by the pen. The pen itself was attached to the lower portion of the tracking carriage and concealed beneath the top of the recording instrument. Fig. 1 shows a view of the recording instrument with part of the top removed exposing the recording pen, while Fig. 2 shows the carriage and its track removed from the recorder. The ballbearing suspension system of the carriage along with the attached pointer and the recording pen may be seen in Fig. 2. Aside from the paper drive, this is the only moving component of the instrument.

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[4] The small effect produced by the passive physical components can be determined and compensated for but it is generally so small as to be negligible.

[5] The National Cash Register Company produces an unlined tape, 5-in wide, which is identified as E5.

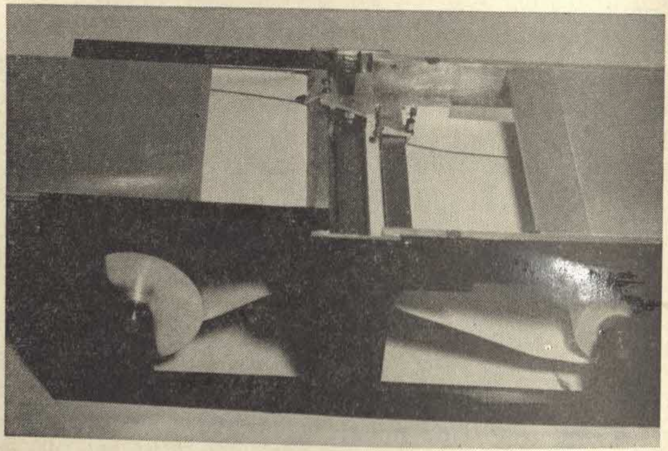

FIG. 1. DELAYED FEEDBACK SELF-TRACKING INSTRUMENT
Part of top removed to show tracking-pen and moveable carriage. Bar between pen and carriage was weighted to supply constant tension.

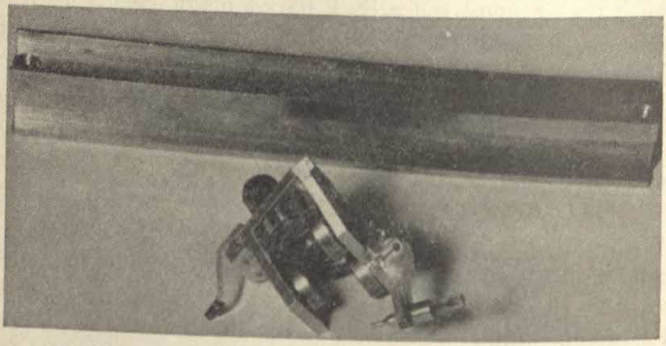

FIG. 2. DETAILS OF CONSTRUCTION OF MOVEABLE CARRIAGE AND TRACK

The delayed feedback self-tracking aspect of the instrument is provided by recording the response on the same 5-in. paper tape on which the moving line is being tracked. The delay consists simply in the fact that the tape moves past the recording station first, and then after an 8-sec. interval reaches the tracking station.

The schematic drawing of the instrument shown in Fig. 3 will make this clear. Beginning with the supply roll of tape, the paper proceeds around Idler 1, beneath weight W, which supplied the proper tension, and past the recording position. It then proceeds around Idlers 2, 3, 4, and 5 to the tracking position immediately in front of the moveable carriage. From here, it turns sharply around Idler #6, between the drive rollers and to the take-up reel. Power is supplied to the drive roller from a small constant-speed electric motor through a chain and sprockets and to the take-up reel by a loosely fitting belt to allow for slipping.

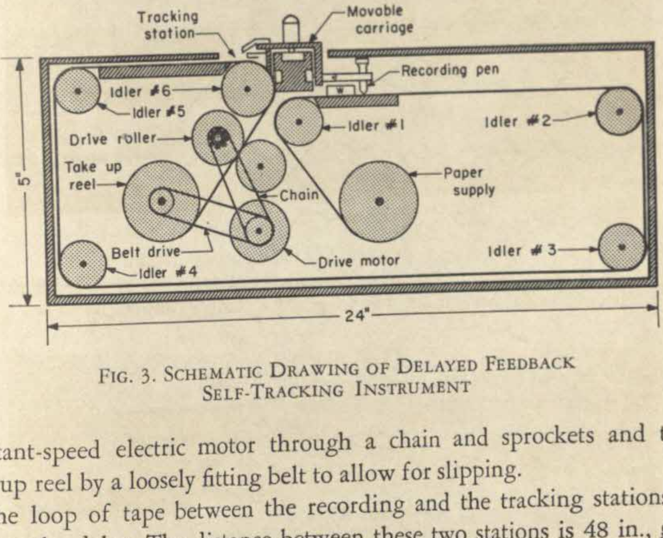

FIG. 3. SCHEMATIC DRAWING OF DELAYED FEEDBACK SELF-TRACKING INSTRUMENT

The loop of tape between the recording and the tracking stations constitutes the delay. The distance between these two stations is 48 in., giving a delay of 8 sec. at a tape-speed of 6 in. per sec. As was stated previously, the initial input-curves are of 8 sec. duration, thus they extend just from the tracking station to the recording pen. Since these curves are also defined as having an initial value equal to the final value, when the pointer is placed at the very beginning of the input curve, the pen is exactly over the other end.

In practice, each tracking session begins with a stationary tape. *S* is instructed to align the pointer with the beginning part of the curve. A pen lift, not shown in any of the photographs, prevents the pen from recording extraneous excursions prior to the starting of the tape. When *S* is set, the pen is lowered to the paper, and the motor started. As *S* tracks the on-coming curve, his responses are recorded as a continuation of the initial input-curve. Thus, after the initial 8 sec. of each trial, he is tracking a curve which he has drawn himself.

Fig. 4 shows a part of an *S*'s record. It was cut at each successive cycle and the sections mounted one below the other. Every segment (except the first which was provided by *E*) is the response to the target above and becomes the target for the response below. It may be seen how *S* smoothed parts of the curve which changed too rapidly and introduced more rapid changes where they were too slow. Analysis of these curves may be a Fourier transformation or other appropriate method to determine each *S*'s preferred response pattern.

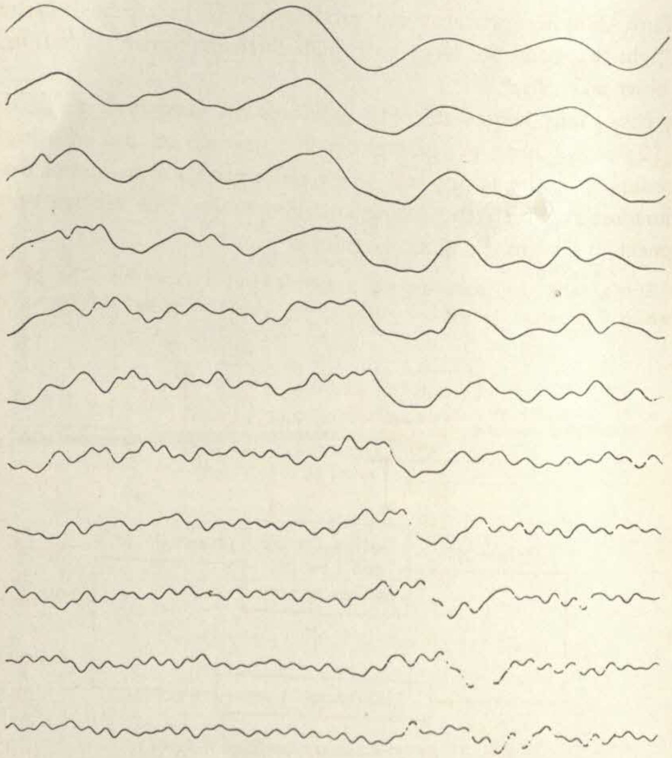

FIG. 4. A DELAYED FEEDBACK SELF-TRACKING RECORD
The record has been cut into 8-sec. segments. Each segment, except the first, is the response to the target above and becomes the target for the response below.

# A HIGH-INTENSITY FLASH-SOURCE

By Ralph Gerbrands and Joseph C. Stevens, Harvard University

A fairly simple apparatus can produce single or repeated flashes of white light that combine large area, high intensity, variable duration, and rapid onset and offset.

The basic components are: (1) a fluorescent lamp with a fast decay-time; (2) a switching or 'gating' circuit to switch on and off a high DC voltage-supply to the lamp; and (3) a pulse-generator to control the rate and duration at which the gating circuit operates. The relation of these components is seen in the block-diagram of Fig. 1.

The lamps used are commercially available and inexpensive. The phosphors with which fluorescent lamps are coated vary widely in color and rate of decay, but Sylvania lamps, designated as Deluxe Cool White (formerly as Super Deluxe Cool White) have been found by our laboratory, and earlier by Sperling,[1] to give remarkably faster decay-rates than other white fluorescent lamps on the market. (It is probably advisable, however, to test each lamp individually, for one cannot take for granted that the coating of the lamp is identical for all lamps designated in the same way by the manufacturer.)

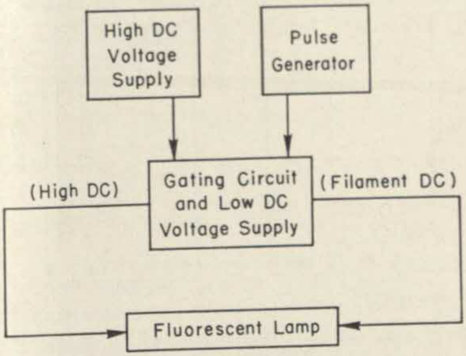

Fig. 1. Arrangement of the Primary Components

The switching action of the gating circuit (described in detail below) is very

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* This apparatus was developed with the support of Grant G-10716 from the National Science Foundation (Laboratory of Psychophysics Report PPR-283).

[1] G. Sperling, Personal communication, April, 1963.

fast and depends upon the limits imposed by the lamp-phosphors. We have been able to produce reasonably square, intense light-pulses with durations of less than a millisecond (see Fig. 2). The peak-intensity, measured by photometry on longer pulses, lies in the neighborhood of 5 or 6 lamberts (about 108 db. above threshold). The capacity of the switching circuit is such that lamps ranging from 4 to 40 w. can be illuminated to full intensity, and the smaller lamps (4 to 20 w.) can easily be illuminated at intensities in excess of their actual wattage-ratings.

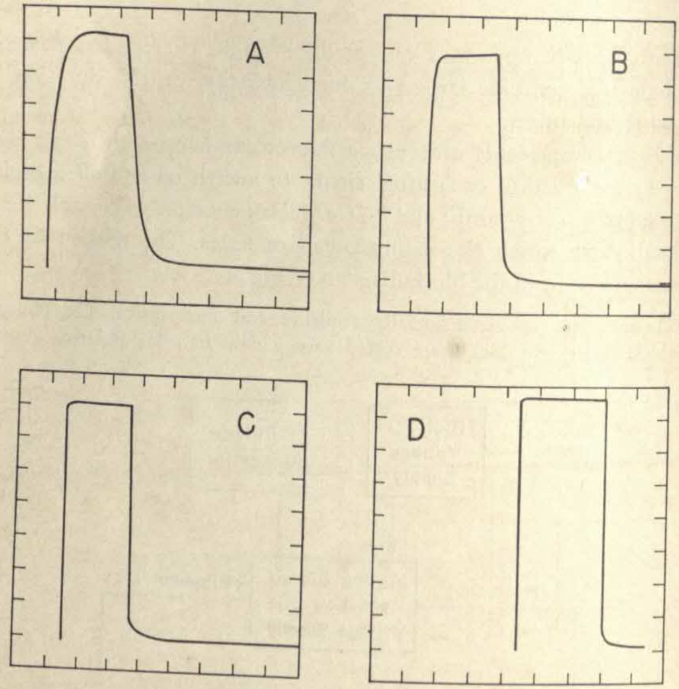

FIG. 2. TRACINGS OF OSCILLOSCOPE-PHOTOGRAPHS OF PULSES
Approximate durations: (A) 0.5 m.sec.; (B) 1 m.sec.; (C) 10 m. sec.; and (D) 50 m.sec. Horizontal sweep times 0.2, 0.5, 5, and 20 m.sec. per scale division, respectively.

The gating circuit shown in Fig. 3 makes use of a simple, basic method of lighting fluorescent lamps with DC voltage. One of the filaments of the lamp is heated to incandescence and a high DC voltage is then applied between the heated filament at one end of the lamp and the unheated filament at the other end of the lamp. The negative terminal of the DC power-supply is connected to the heated filament, and the positive terminal to the unheated filament. If the power-supply does not have a high internal resistance, a high resistance should be inserted between the unheated filament and the positive terminal of the power-supply. When the high DC voltage is applied to the lamp, the lamp is 'struck,' *i.e.* the arc is formed, current flows through the lamp, and the phosphors are excited. The lamp-current then falls to the value determined by the series resistance of the power-supply.

The gating circuit, which is used to turn on and off the high DC voltage, is similar in some respects to the one employed successfully by Koletsky and Kolers to operate a number of cold cathode gas-discharge tubes that serve as the light-sources for a multi-field tachistoscope.[2] Our gating circuit uses a 6DQ5 power-amplifier tube connected as a conventional pentode amplifier. It is biased to cut-off by a 90-v. battery connected between the cathode and the grid-circuit. The screen-voltage, approximately 200 v., is tapped from the midpoint of a 40-kΩ resistor connected between the B supply and ground. The B supply-voltage, about 400 v., is obtained from a regulated DC voltage-supply that is external to the gate. The plate-load resistor is 10 kΩ.

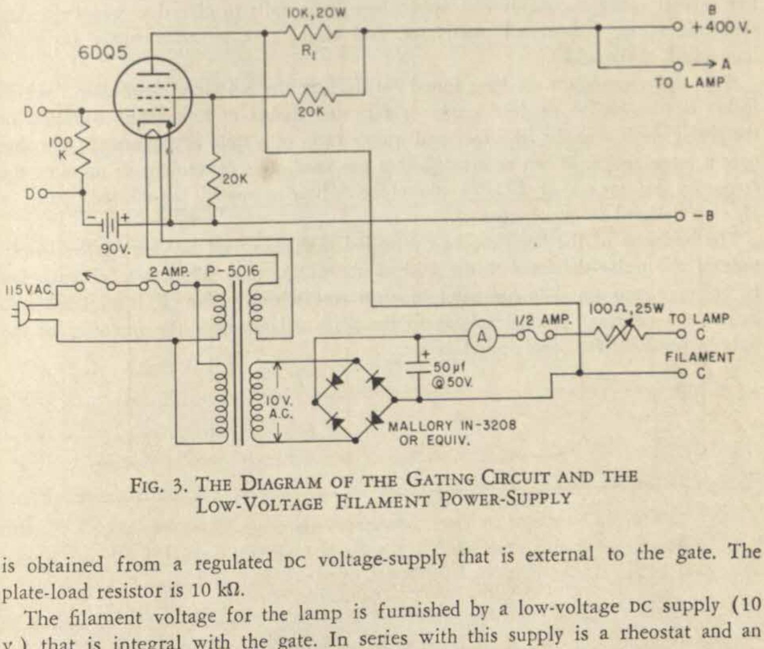

FIG. 3. THE DIAGRAM OF THE GATING CIRCUIT AND THE LOW-VOLTAGE FILAMENT POWER-SUPPLY

The filament voltage for the lamp is furnished by a low-voltage DC supply (10 v.) that is integral with the gate. In series with this supply is a rheostat and an ammeter with which to adjust the filament current to an optimal value.

As the circuit diagram of Fig. 3 shows, one filament of the lamp is connected to the terminals CC, and the unheated filament at the other end of the lamp is connected to the terminal A. These terminals connect the lamp in parallel with the plate-resistor $R_1$. With no signal applied to the grid of the 6DQ5 tube and with the tube biased to cut-off by the battery, no current flows through $R_1$, and hence no voltage-difference appears across $R_1$. When a postive signal of sufficient amplitude (optimally 70 to 90 v.) is applied to the grid-circuit DD, current flows in the plate-circuit and causes a voltage across $R_1$ that is large enough to strike the lamp. Once the current-flow has been established through the lamp, the lamp becomes the effective load on the tube. Since the amplifier-tube is relatively insensitive to the plate-voltage, the current through the lamp and hence the intensity of the light varies with the amplitude of the grid-signal produced by the pulse-generator.

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[2] H. S. Koletsky and P. A. Kolers, A multi-field electronic tachistoscope, this JOURNAL, 72, 1959, 456-459.

The light-pulses, shown in Fig. 2, were measured with a 929 phototube, but the light-input to the phototube was first passed through a No. 106 Wratten filter. The spectral sensitivity of the 929 tube and the absorption-spectrum of the 106 filter are such that, when they are used in combination, the spectral sensitivity of the system resembles closely the photopic sensitivity-function. Thus, a relatively unbiased measurement of the form of the pulse is achieved. Used by itself the 929, like several other commonly employed phototubes, fails to give due weight to the slower-decaying, red-colored phosphors, and hence give an erroneously favorable impression of the offset.

As a pulse-generator we have found satisfactory the S-4 Grass Stimulator, which makes it possible to produce single or repeated flashes of light. The duration of the single flash and the frequency and on-off ratio of a train of flashes are variable over a large range. When repeated flashes are used, it is convenient to monitor the frequency by means of a 1P14 phototube whose output is connected across a Hewlett-Packard frequency-counter.

The intensity of the lamp can be controlled to some extent by varying the amplitude of the pulse delivered to the grid of the 6DQ5 tube. The range of variation is, however, too small to be useful in most research on vision. It is preferable to maintain a constant optimal voltage to the grid and attenuate the intensity of the light by some conventional optical device.

# A SIMPLE AND INEXPENSIVE CHRONIC CANNULA FOR INTRACEREBRAL INJECTIONS INTO SMALL ANIMALS

By JOHN R. VOGEL, The Squibb Institute for Medical Research, and
PETER L. CARLTON, Rutgers, The State University

This paper presents a detailed description of a cannula that is easily constructed of inexpensive, readily available components.[1] It can be used very satisfactorily for both acute and chronic intracerebral injections and can also serve as a monopolar electrode for recording, for electrical stimulation,[2] and for lesioning. All the necessary components for construction of the cannula can be purchased for less than $1.

Parts for the unit are illustrated in Fig. 1 A. A ½-in., 6-32 round head brass machine screw is cut in half. The head of the screw is ground flat. Each of the pieces is center-drilled to a depth of 3⁄16 in. with a number 60 twist drill. The holes are completed with a number 73 twist drill. To make the outer cannula (chronic implant), a 1¼-in. length of 23 gauge hypodermic needle tubing is force fitted through the piece having the screw head.[3] The tubing is ground flush with the end of the screw stock and 1 in. of tubing is left protruding from the screw head (Fig. 1A-3).

The filler-wire for 23 gauge tubing is passed through the tip of the tubing and the entire unit is dipped in epoxy resin varnish,[4] and hung in an oven for baking (160° C. for 60 min.).

The removable inner cannula is made by forcing a short length of 23 gauge tubing through the hole in the headless screw. The tubing is ground flush to the screw stock at both ends. A 1¾-in. length of 30 gauge hypodermic needle tubing is then so fitted through the 23 gauge tubing that 1¼ in. of tubing extend from one end and ¼ in. extends from the other. The 30 gauge tubing is then soldered to the screw stock at its short end (Fig. 1 A-5). (Stainless steel solder-flux should be used.)

The inner and outer cannulas are fitted together and locked with a threaded teflon collar (Fig. 1 A-4).[5] The length of the cannula is determined by the depth of the target-structure. The inner and outer cannulas are fitted together with the collar, and a filler wire is placed in the inner cannula. The unit is trimmed with a fine grinding wheel. (The filler wire insures a clear inner cannula.)

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[1] We are grateful to Dr. J. C. Burke for his advice during the design and construction of this cannula.

[2] For long-term stimulation (*e.g.* self-stimulation), platinum-iridium tubing (J. Bishop Company, Malvern, Pennsylvania) is recommended.

[3] Becton, Dickinson and Company, Rutherford, New Jersey.

[4] BC-341 Dolphon Epoxy Resin Varnish, John C. Dolph Company, Monmouth Junction, New Jersey.

[5] Available from Plastics Products Company, Roanoke, Virginia.

For simple connection to stimulating and recording apparatus, one lead of a twisted-pair, bipolar electrode (MS-303-0.014 -0.500 in. electrode)[6] is soldered to the outer cannula before it is dipped in varnish.

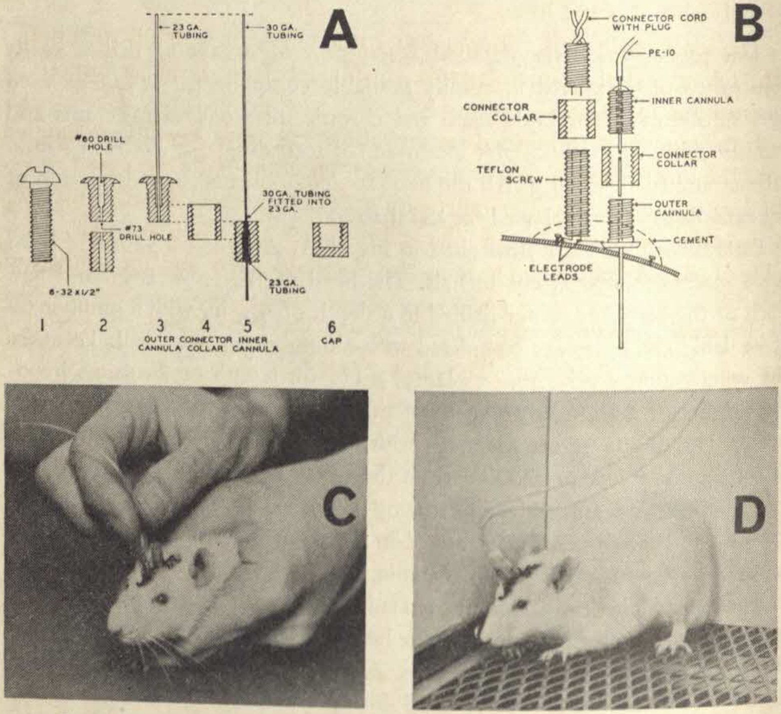

FIG. 1. CANNULA FOR INTRACEREBRAL INJECTIONS IN SMALL ANIMALS
A—Components for cannula; B—Implanted outer cannula and connectors; C—Preparation: Sutures not removed; note cap on teflon electrode screw; D—Completed preparation; inner cannula and connector cord attached.

A long-lasting implant is obtained by placing three 0-80 × ⅛-in. screws in the calvarium.[7] The outer cannula is then positioned stereotaxically. The other lead of the electrode is soldered to one of the 0-80 screws. The teflon screw of the electrode is positioned next to the implanted outer cannula, with enough clearance left for the inner cannula to be attached. The entire area is covered with dental cement with about 1/8 in. of the cannula and the teflon exposed. Fig. 1 B and C, show the completed preparation. The threaded teflon cap shown in Fig. 1 A-6, is used to cover the cannula when it is not in use.

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[6] Available from Plastics Products Company, Roanoke, Virginia.

[7] E. S. Valenstein, William Hodos, and Larry Stein, A simplified electrode-assembly for implanting chronic electrodes in the brains of small animals, this JOURNAL, 74, 1961, 125.

The inner cannula is connected to a microinjector with PE-10 tubing.[8] The tubing and the inner cannula are filled before the inner cannula is inserted for injection.

If the cannula is to be used for crystalline injections, crystals are tamped into the inner cannula before insertion.[9] The cannula is connected to recording or stimulating apparatus with a flexible hearing aid cord that has a two-prong molded plug on one end to match the teflon electrode screw (Fig. 1 B). A teflon collar joins the electrode screw and the connector.

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[8] Intramedic Polyethylene Tubing, Caly Adams, Inc., New York.

[9] S. P. Grossman, Direct adrenergic and cholinergic stimulation of hypothalamic mechanisms, *Amer.J. Physiol.*, 202, 1962, 872-882.

# APPARATUS NOTES

## USE OF THE GLOW MODULATOR TUBE FOR VISUAL RESEARCH

The Sylvania glow modulator tube, particularly model R1131C, has been used frequently to provide stimuli for visual experimentation, and indeed at present appears to be the best commercially available light source for most problems requiring rectangular-wave flashes of controllable duration from a "point" source. Readily available electronic units make precision timing of flashes possible, and with optics giving a Maxwellian view an intensity range of at least six log units above, the dark-adapted threshold is easily obtained. A number of colleagues (in at least seven different laboratories) have reported, however, that the glow modulator performs erratically—*i.e.* sometimes it does not fire at all to an imposed voltage pulse, and when it does fire, the time between the onset of the voltage pulse and the beginning of the output of light is found to be extremely variable. This note will describe the source of the difficulty and an easily applied remedy.

The time between the moment of imposition of a voltage above breakdown potential and the moment when a steady value of light output is reached from the glow tube is made up of two components: (1) statistical time-lag, and (2) formative time-lag.[1] The duration of the statistical time-lag is the time from application of a voltage until a small self-sustained current arises from the cathode as a result of an initial electron appearing in the gap between cathode and anode. (Between them the $n$ positive ions of the $n$ ion-pairs formed during movement of the electron toward the anode must liberate at least one electron at the cathode and this process must be sustained until the regenerative process can become independent of the initial small current.) The formative time-lag refers to the time taken for the initial small current to build up to a value limited by circuit parameters such as values of series resistance and voltage.

The formative time-lag has a fairly stable value, and as measured with a 931A photomultiplier tube and 531 Tektronix oscilloscope its value for unfiltered light ouput is about 25μsec.

The erratic behavior referred to above is trial-to-trial fluctuation of the duration of statistical time-lag. Under the usual conditions maintained during experimentation with visual processes, the glow tube is kept in complete darkness and enclosed in a metal container. These conditions minimize the opportunity for a stray electron (such as a photoelectron produced at the cathode or one due to gas ionization by external agents) to enter the cathode-anode gap; as a result the average value (as well as variability) of statistical time-lag is lengthened.

Any means of irradiating the cathode with energy at wavelengths which are appropriate to produce photoelectrons will remedy the situation. The present writer has employed a General Electric argon glow lamp, AR-3 or AR-4, as a source of continuous ultraviolet radiation (300-400 mμ); this is placed over the glow

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* This work was supported by a contract between Columbia University and the Office of Naval Research, and by Grant G18120 from the National Science Foundation.

[1] M. J. Druyvesteyn and F. M. Penning. The mechanism of electrical discharges in gases of low pressure, *Rev. mod Physics*, 12, 1940, 87-174.

modulator tube and operated on D.C. voltage. A Corning 5860 glass filter placed between the G.E. lamp and the glow modulator tube absorbs wavelengths emanating from the G.E. lamp above 390 mμ and prevents them from reaching the *S*. Ultraviolet radiation from the G.E. lamp is prevented from reaching the *S* by a filter (*e.g.* Kodak #2A, 4, or 8) placed between the glow modulator tube and the *S*; when achromatic lenses are used in the system, they too will absorb the ultraviolet if, as is usual, they contain a high flint component. With this system the glow modulator has worked with excellent reliability (*i.e.* statistical time-lag never greater than 2 μsec. with variability undetectable by present methods of monitoring) in experiments in which a single 2 m.sec. temporally rectangular flash was produced by each of two brass-enclosed glow modulator tubes at intervals of about 30 sec.[2] Although the ultraviolet output of the G.E. lamps decreases with time, after about 800 hr. of use no lengthening of statistical time-lag has been noticeable.

It must also be noted that even without irradiation from external sources the durations of statistical time-lag will sometimes be shortened immediately after a preceding flash. (This effect is probably due to autoelectronic emission resulting from cathodal impurities).[3] There seems to be considerable variability from tube to tube in the magnitude of this shortening effect, however. For some tubes sufficient stabilization of statistical time-lag for studying steady state visual flicker may be obtained without external irradiation since flashes follow each other at short time intervals. Nevertheless, since autoelectronic emission is notoriously variable, the present writer finds it simpler to provide a margin of safety by always using the G.E. lamps to irradiate the cathode.

A second problem should be mentioned in connection with the use of the glow modulator tube. When a tube is fired after having been quiescent for some time, the light output increases with a slow quasi-exponential rise for a period of several minutes to values which may be nearly twice the initial value. Further, the effects of successive flashes are cumulative for sufficiently long flash-durations and for sufficiently short interflash-intervals. This variation in the relation between input current and light output appears to follow a complex time course in which effects can be observed to decay over periods which can range from several minutes to several days depending on conditions of stimulation during prior use.

Several means of handling this problem are possible: (1) If sufficiently short flashes and sufficiently long interflash-intervals are used, no cumulative effects are found. Thus, for example, no change in light output could be detected over periods of several hours when 2 m.sec. flashes were repeated regularly at 10 sec. intervals. (2) The output of a photocell which itself samples the output of the glow modulator tube can be fed back so as to stabilize the glow tube's output. Since the spectral energy-distribution of the glow modulator tube varies with lamp current, it is, however, important to note that unless the photocell's spectral absorption distribution is filtered to match the luminosity function of the *S*, luminous output of the lamp will be less than completely stabilized.

LEONARD MATIN

New York University

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[2] Leonard Matin, Binocular summation at the absolute threshold of peripheral vision, *J. opt. Soc. Amer.*, 52, 1962, 1276-1286.

[3] M. J. Druyvesteyn and F. M. Penning, *op. cit.*, 117-118.

# A SIMPLIFIED APPARATUS FOR THE RELIABLE DELIVERY OF SHOCK IN ANIMAL EXPERIMENTS

In experimental work involving aversive stimulation, shock is frequently used as the stimulus. The shock is often delivered to the feet of the experimental animal through an electrified grid. With alternate bars of the grid charge (+) and grounded (−) it is possible for the animal to avoid or escape the shock by standing on bars of a like charge. One solution to this problem is a grid-scrambler which is a rotating switch which changes the relative polarity of the bars in short time-intervals. With the apparatus and wiring technique described below, it is possible to obtain about the same effect in a simple and inexpensive way.

The basic instrument, a current distributor, consists of a rotating cylinder divided into two parts (Fig. 1A) insulated from each other and connected to a transformer via two wire brush contacts (+ and − in Fig. 1A). From the cross-section in Fig. 1B, the charge from the rotating cylinder will be distributed successively to two of three bars of the grid. Relative polarity vs. angle of rotation is shown at the top of Fig. 1B for the case where the two distributing notches occupy 120 and 240°. To increase the distance between grid bars of like charge, alternating current may be applied to some bars directly from the transformer. Thus we obtain a series of five independent bar-connections with an actual shock-duration varying between 30 and 100% of the setting. In a commercial grid-scrambler we have examined,

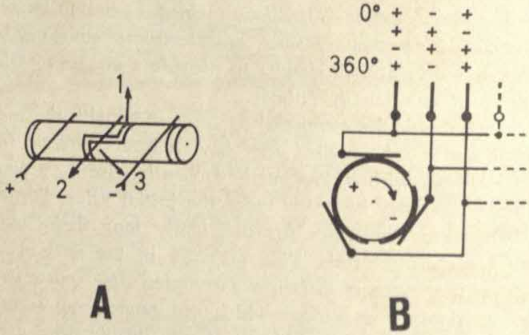

FIG. 1. SCHEMATIC DRAWINGS OF THE DISTRIBUTOR
'A' shows the pickups leading to the grid-bars; 'B', the connections to the grid-bars and above them the changes in relative polarity of each grid as the cylinder revolves through 360°

this was found to vary between 5.6 and 97% of the setting, depending on the contacting pair. The dual construction in Fig. 2 is intended for the two compartments of a shuttle box. The cylinder is made of Perspex and covered with three thin-walled brass tube sections. It is driven by a synchronous motor (40 r.p.m.). Due to wide insulating gaps and a low self-capacitance, this apparatus is suited for work with potential in excess of 1 kV.

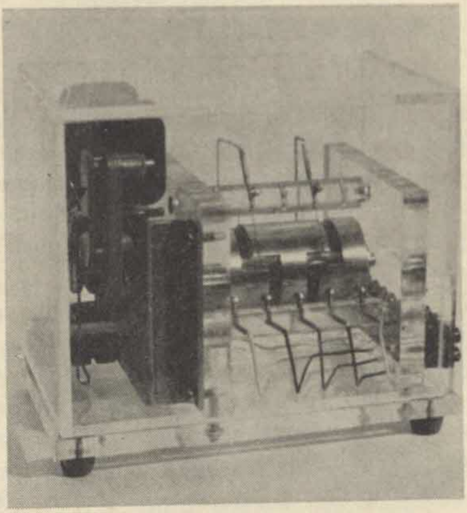

FIG. 2. PHOTOGRAPH OF THE APPARATUS

University of Göteborg
Department of Pharmacology
Sweden

L. S. SEIDEN
GEORG THIEME
IVAN ANDERSON

# NOTES AND DISCUSSIONS

## "STABILIZED" DOUBLE IMAGES

With the renewal of interest in problems of depth-perception, it might be useful to report some, informal as yet, continued observations of double images carried out for a number of years. This phenomenon was first reported more than a hundred years ago and has since then been considered as a possible cue in depth-perception, perhaps most elaborately by Woodworth and Schlosberg.[1]

The essential facts of double images can be briefly summarized. When an object is binocularly in focus—perceived as 'one'—the light falls on "corresponding" areas of the respective retinas. Objects nearer or farther away (*i.e.* not on the given empirical horopter), if attended to, are seen as double, since for the most part they stimulate non-corresponding retinal areas. A simple, traditional, demonstration is made with the use of a near and far pencil, focusing first on one and then on the other—each, alternately seen as 'single' or 'double.' With naïve subjects who attend only to that which is in focus, an effective method is to place the first two fingers of one hand some inches in front of the eyes focusing at distant objects through a window. By appropriately adjusting the distance of the fingers, one readily perceives the doubling (*four* fingers) or the reduction to three fingers (one of each double image superimposing).

It is usually easier to focus on some near object and thus obtain doubling of a more distant object. This is a simple way of experiencing depth with a pair of ('left' and 'right' eye) stereoscopic pictures without employing a stereoscope. Since the 'right eye picture' stimulates the left eye and the 'left eye picture' reaches the right eye, the depth will be reversed, at least for line-drawings. This method of observing stereoscopic cards without a stereoscope, has been used by the present writer for years and is the origin of the observations constituting the core of this report.

Within a relatively short period after this practice was initiated, there was no need to use a near focusing object to obtain the appropriate crossing of the stereoscopic pictures and converging control of the eyes soon became entirely voluntary. It was also possible to diverge the eyes to obtain an horopter beyond the pictures and get the same results, this time resulting in 'true' depth, but this required considerable effort and was not a regular practice. Some years later, during college and graduate studies, the "wallpaper experiment" was carried out frequently, with tile walls, windows, trees, automobile headlights, and heads of people. At a later time, heads of students were superimposed with no difficulty whatsoever during lectures. More important, the superimposition of similar objects could be readily held even as the distance varied, such as moving about during the lecture or fixating the headlights of a moving automobile while standing still. There was no conscious strain in holding the appropriate 'doubling' unless the head was tilted considerably.

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[1] R. S. Woodworth, *Experimental Psychology*, 1938, 662 ff.; R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 1954, 458 f.

When the background was a similar or repeated pattern, with appropriate focus on a near object, the doubling of this background was such that given units exactly superimposed and were seen as 'one.' Here, the far (double) images fell on corresponding areas of the respective retinas. As one attended to the pattern it was in sharp clear focus; the *focal* distance—lens accommodation—was different from that for which convergence of the two eyes (*i.e.* functional horopter) was maintained. In scanning a wallpaper pattern, it was possible to vary convergence (a sort of 'distant-scanning') and at a given instance hold a particular superimposition. Under this circumstance, it was extremely difficult to determine whether the horopter was nearer or farther than the repeated pattern and the only way to regain 'control' was to close the eyes. Obviously, the kinesthetic pattern from the external (striated) muscles controlling (in this case, degree of convergence of) the eyes was becoming strongly habituated and the activity was discontinued as a regular practice. The independent functioning of convergence and accommodation was readily obtained and did not require superimposition of two similar objects or components of a repeated pattern. The desired amount of 'doubling' of any given single object, within the physical limits of convergence, was and still is, readily obtained.

One additional, although somewhat difficult, observation at the perceptual level may be of interest in clearly demonstrating the role of attentive set with respect to double images. A reversible figure such as the "Peter-Paul goblet" can be perceived as a goblet or the heads of two men vis a vis.[2] With appropriate 'doubling,' the figure is seen as two overlapping or distinctly separate images, depending upon the distance of the figure from the face and the amount of convergence. With sufficient practice, it has been possible to perceive one of these double images as one figure-ground (goblet) and the other in the reverse form (two heads). The experience is one of considerable strain and still cannot be maintained indefinitely. The effect has been reproduced with some other "reversibles" and was more readily obtained with physically smaller and simpler figures which were not composed of a repeated pattern.[3] Others, such as the reversible staircase, completely resisted such treatment. Difficulty was also encountered with those figures so large that the doubling displaced the images laterally; sufficiently from foveal vision so as to impair form discrimination.

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[2] F. A. Geldard, *Fundamentals of Psychology,* 1962, 248.

It is clear that Woodworth in speaking of double images refers to those obtained in the obvious changes produced in focusing on nearer and farther distances and not to the slight changes which may occur in maintaining a given focal distance. On this point he writes as follows:

> When attention shifts from one object to another at a different distance, the double images which are already present are perfect cues for the readjustment of convergence to the new object. The probability is that these excellent cues of relative distance are actually used. . . . Because we ordinarily are unaware of most of the double images which are demonstrably present, it might be argued that we cannot use them as cues. But need a cue be noticed to be used? On the contrary, it seems that a sign need not be known except in terms of its meaning. Binaural time difference is an example. . . . Binocular disparity, constantly in use as a cue, is seldom observable for what it is—it is observed only as depth and relief of the object.[4]

It has been questioned whether the exact role of double images can be determined, since "there is no way of eliminating them from binocular vision so as to find out how much depth perception would be left."[5] This statement requires qualification.

Double-images of course will occur while obtaining the appropriate convergence-distance. This, under experimental conditions, could be minimized by use of a fine light-point at the required distance at which *S* fixates—before the test-stimulus is presented. To the argument that in the very process of fixating this point in the normally illuminated situation, double images would occur is for all practical purposes unanswerable. It would be a rare person indeed who could maintain the required horopter without variation until the test-stimulus was presented. This, however, would not appear to be a major issue. If it is to be tested experimentally, the question would have to be the role of double images *during* a given period of focal fixation.

A second possibility is a process of 'hunting' while the stimulus is held in focus such that slight changes in convergence occur, with consequent slight doubling of the focused object. It is hard to see how one could test the role of such a subliminal factor, if in fact it does occur. Electrical recording from the external eye-muscles under a variety of focal distances might shed some light on this question. The blurring of the image not in accommodative focus is clearly observable and might be more important. This variable is susceptible to manipulation by the appropriate use of pinhole apertures in front of the eyes, or similarly devised contact lenses.[6] However, granting the possible role of subliminal doubling during the "hunting" process, larger detectable degrees of doubling (*e.g.* flag-pole and pencil) also ought to play a role, and more so. And this condition is testable.

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[3] Woodworth and Schlosberg, *op. cit.*, 404 f., Figs. 14.2 and 14.3.
[4] Woodworth, *op. cit.*, 663 f.
[5] Woodworth and Schlosberg, *op. cit.*, 459.

'Simple' double images as ordinarily perceived, can readily be eliminated. Consider a visual field which is limited to a flag-pole about 200 ft. away. If one adjusts the distance of a vertical pencil in front of one's face and focuses on the flag-pole, the pencil is seen as two. (With such a far focal distance, the *right* image of the nearer double image of the pencil is seen by the left eye and the left image is seen by the right eye.) Another vertical pencil now placed close to the left eye can be adjusted laterally so as to completely block out the *right* double image of the pencil. One sees one flag-pole (both eyes) and only one pencil (by the right eye). With appropriate modifications, an equivalent demonstration can be made by focusing on the pencil and blocking out one of the double images of the flag-pole. In the latter case, the flag-pole image on the same side of the eye which is masked will disappear. Of course, other objects in the field will mar the demonstration because all objects not in focus will be, more or less, doubled depending upon their distance from the given horopter. But this simple demonstration is readily replicated under exact experimental conditions with appropriate lighting so as to limit vision to the two objects representing farther and nearer stimuli.

An additional question is concerned with what is meant by double images. In the first place, even when *attending* to the stimuli off (nearer or farther than) the horopter, doubling as such can be perceived only within narrow limits of the line of direct regard. Suppose one 'converges' on a near (real or imaginary) horopter and attends to windows in a building across campus which are in direct line of regard. These windows are seen as double; but, while maintaining this line of regard, the windows in the periphery appear blurred. One reason is the fact that the periphery of an extended plane surface at right angles to the eyes has a different focal distance to that on direct line of regard. The only case in which double images would remain exactly doubled and in focus would be a wallpaper pattern which was so curved as to have all points at equidistance from the functional horopter. In the normal perceptual situation, even the double images on the line of direct regard beyond the horopter are blurred, being out of accommodative focus. In addition, discrimination of form is progressively impaired as distance from foveal stimulation is increased. It is probable therefore that if 'double images' are a depth-cue, it is not so much the doubling as much as serving as a non-focused background.

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[6] Cf. E. G. Heinemann, Endel Tulving, and Jacob Nachmias, The effect of oculomotor adjustments on apparent size, this JOURNAL, 72, 1959, 32-45.

A more pertinent consideration than double images *per se* would appear to be the effect of a form of motion parallax. In recent years, while motion parallax, monocular as well as binocular, has been given more serious attention as a depth-cue, very little attention has been given to the role of visual phi which is evoked as double images are modified with changing horopters. This effect is intrinsically and inseparably related to the phenomenon of double images. In changing binocular convergence, or divergence, for a different horopter, there is a definite experience of motion as the objects 'move.' This is true whether the visual field, as in the everyday situation, is composed of many objects or is confined to a single near and far object. The same experience is obtained in a sufficiently darkened room so as to confine the experience to a single near object a few inches in front of the face. The stimulus is first seen as one and when the eyes diverge, rapidly to some more distant imaginary horopter, motion is experienced as the stimulus, *e.g.* a finger, becomes doubled. This is no less an example of motion parallax than those usually described in the literature. Here, the changing angle of the eyes during convergence or divergence displaces the retinal positions of the visual images. In a sample of recent writers, no mention is made of this phenomenon.[7]

The present observations would also appear to be relevant to the problem of the "fusional process," concerning which considerable discussion is to be found in the literature. With the use of an appropriate ophthalmic prism before one eye, two objects are now seen where, previously at a given fixation-point, one had been seen. With moderate 'doubling,' fusion occurs as the result of movement of the eye behind the prism, which again locates the image on a retinal area corresponding to that of the other eye. This is attributed to a very strong motor reflex of 'compulsion for fusion' which normally operates to obtain fusion by stimulation of corresponding retinal areas and the instigation of these reflexes is noted to depend upon attention.[8] For another treatment of the "compulsion for fusion," see Linksz.[9]

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[7] C. H. Graham, Visual perception, in S. S. Stevens, *Handbook of Experimental Psychology*, 1951, 870 ff.; W. H. Ittelson, *Visual Space Perception*, 1960, 84 ff.; Arthur Linksz, *Vision*, Vol. 2, 1952, 581; K. N. Ogle, *Researches in Binocular Vision*, 1950, 134, 139 f.; Woodworth, *op. cit.*, 652 ff.; Woodworth and Schlosberg, *op. cit.*, 461 f., 474.

The nature of the fusional process has remained largely speculative, some having assumed that there *is* a fusion process uniquely characteristic of the stereoscopic experience. This mechanism, rather than reflexive, is more probably overlaid with learning originating from the initial eye-movement reflex to center light foveally. Sherrington suggested years ago that "The convergence of nerve-paths from the right and left retinae respectively toward one central region is significant of union for co-ordination of motor reaction rather than for synthesis of sensation."[10] He further proposed, "that during binocular regard of an objective image each uniocular mechanism develops independently a sensual image of considerable completeness. The singleness of the binocular perception results from union of these elaborated uniocular sensations. The singleness is therefore the product of a synthesis that works with already elaborated sensations contemporaneously proceeding."[11] The observations reported here would appear to support this position. One may reasonably assume that the genetic basis is a monocular reflex such as to orient the eye foveally to a visual stimulus. Each eye operating individually upon this basis would establish coördinated binocular patterns of convergence (or divergence) appropriate to the given stimuli. An important consideration in the organization of this pattern would appear to be the role of attending. Although its precise role is still to be determined, the data demonstrate the functional independence of attending under the appropriate conditions. Accommodative focus and binocular convergence are separable and appropriate attentive sets can be established for the individual monocular images.

Orienting reflexes are not peculiar to vision, but rather operate in several senses, suggesting "a directionally structured space of action, and discriminative locating responses within that space."[12] Without forcing a "motor" theory of behavior, Drever's approach may very well make sense in understanding the neonate's development of three dimensional as well as two dimensional space. The intimate, although not entirely understood, relationship between depth perceptual-constancy and depth-perception is well recognized: *e.g.* apparent size is a function of perceived distance.[13] It has also been suggested that perceptual shape, orientation in space, and possibly brightness are dependent on apparent distance, and also that relative apparent distance is very strongly dependent on parallax indications.[14]

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[8] Ogle, *op. cit.*, 60 f.

[9] Linksz, *op. cit.*, 828 ff.

[10] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1947, 353.

[11] Sherrington, *op. cit.*, 380.

[12] James Drever, Perception and action, *Bull. Brit. Psychol. Soc.*, September, 1961 No. 45, p. 5.

[13] E. G. Boring, On the moon illusion, *Science*, 137, 1962, 902-906.

Brooklyn College EDWARD GIRDEN

## ON THE PRESENTATION OF STIMULUS-OBJECTS IN THE METHOD OF PAIRED COMPARISONS

Textbooks of psychology usually point out that in any experiment making use of the method of paired comparisons it is obviously desirable that pairs involving a particular object should be as far apart as possible, and that no particular object should appear preponderantly in one position (first, top, left). Surprisingly, no completely satisfactory way of achieving this appears to have been presented. The latest publication on the subject the writer has been able to trace is a quarter of a century old, and it gives an incomplete solution for the case of an odd number of objects and quotes an obviously incorrect solution for the case of an even number of objects. The present note is an attempt to correct this situation, and to provide a solution which will be of practical value to investigators using the method of paired comparisons.

*Odd number of stimulus-objects.* Ross has presented tables (whose general form is evident from Tables I A and B) for generating lists governing the presentation of an odd number of stimulus-objects in the method of paired comparisons which yield (1) the greatest possible spacing between pairs involving the same stimulus-object; and which ensure (2) that every stimulus-object occurs an equal number of times in the first and the second positions.[1] Wherry pointed out that in Ross's lists there were a number of stimulus-objects which occurred either first in a pair, or second in a pair, twice running, *e.g.* Ross's list for five stimulus-objects is 1-2, 5-3, 4-1, 3-2, 4-5, 1-3, 2-4, 5-1, 3-4, 2-5, and this 'space-error' is made four times.[2] His list for seven stimulus-objects is 1-2, 7-3, 6-4, 5-1, 3-2, 4-7, 5-6, 1-3, 2-4, 7-5, 6-1, 4-3, 5-2, 6-7, 1-4, 3-5, 2-6, 7-1, 4-5, 3-6, 2-7, and this 'space-error' is made eight times. Wherry presented empirically derived lists for five stimulus-objects (1-2, 3-4, 5-1, 2-3, 4-5, 3-1, 4-2, 5-3, 1-4, 2-5) and for seven stimulus-objects (4-2, 6-7, 3-1, 2-5, 7-4, 1-6, 5-3, 2-7, 4-1, 6-3, 7-5, 1-2, 3-4, 5-6, 7-1, 2-3, 4-6, 1-5, 3-7, 6-2, 5-4) in which the 'space-error' occurs less often. Now these empirically derived lists turn out to be, apart from the order of the members within each pair, formally identical with those of Ross. (Thus Wherry's list for five stimulus-objects is Ross's written backwards, with the substitutions 1 for 5 and vice versa, and 3 for 4 and vice versa; similarly for the list for seven stimulus-objects, but with the substitutions 1 for 5 and vice versa, and 4 for 2, 3 for 4, 6 for 3, 7 for 6 and 2 for 7.) The superiority of Wherry's lists therefore lies in the fact that he has (apparently inadvertently) found a method of ordering the members within each pair which is better than Ross's. It is not explicitly stated by Ross, but is evident from Table I that, for the above lists, he attains the property given in (2) above by reversing the order of the members of the pairs in the first column of the right half-table and, to the right of this column, in all rows except the last, and this in fact is true of all the lists he gives. If Wherry's lists are translated back into Ross's by reversal of each list and of the substitutions given above, producing: 2-1, 5-3, 1-4, 3-2, 4-5, 3-1, 2-4, 1-5, 4-3, 5-2 and 1-2, 3-7, 4-6, 5-1, 2-3, 7-4, 6-5, 1-3, 4-2, 5-7, 6-1, 3-4, 2-5, 7-6, 1-4, 5-3, 6-2, 7-1, 4-5, 3-6, 2-7, it is evident that, apparently without realizing it, he is reversing the order of the members of the second and all following columns in Ross's left half-table and in the first column of Ross's right half-table (or, in the list of five stimulus-objects, reversing all columns except these). Application of this method of reversal produces in general lists with $n$-3 space-errors (whilst Ross's method produces lists with twice as many). It can, moreover, be shown that no list for an odd number of stimulus-objects with the property described in (1) above can have less than $n$-3 space errors. It is therefore ironic that Wherry should have written "it seems to the writer that the question of optimum lists is still an open one": had he but known it, he had himself completed Ross's solution.

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[14] Cf. Ittelson, *op. cit.*, 64 ff., 84 ff.

[1] R. T. Ross, Optimum order for the presentation of pairs in the method of paired comparisons, *J. educ. Psychol.*, 25, 1934, 375-382.

[2] R. J. Wherry, Orders for the presentation of pairs in the method of paired comparisons, *J. exp. Psychol.*, 23, 1938, 651-660.

TABLE I

ROSS'S TABLES (A) FOR FIVE AND (B) FOR SEVEN STIMULUS-PAIRS

For five stimulus-pairs, read across the first row of Table A, then across the second row; for seven stimulus-pairs read across the first row of Table B, then the second and the third. For the order of the members within each pair, see the text.

| A. Five stimulus-pairs | | | | |
|---|---|---|---|---|
| 1–2 | 5–3 | 1–4 | 2–3 | 5–4 |
| 1–3 | 2–4 | 1–5 | 3–4 | 2–5 |

| B. Seven stimulus-pairs | | | | | | |
|---|---|---|---|---|---|---|
| 1–2 | 7–3 | 6–4 | 1–5 | 2–3 | 7–4 | 6–5 |
| 1–3 | 2–4 | 7–5 | 1–6 | 3–4 | 2–5 | 7–6 |
| 1–4 | 3–5 | 2–6 | 1–7 | 4–5 | 3–6 | 2–7 |

*Even number of stimulus-objects.* Ross's tables are appropriate only for an odd number of stimulus-objects. He claims, however, that for an even number of stimulus-objects lists with property (1) mentioned above, with a minimal spacing of $(n\text{-}4)/2$ and a maximal spacing of $(n\text{-}2)/2$, may be generated by omitting from the lists for the next largest number all pairs involving that number. This claim is not justified. Consider, for example the list so generated for six stimulus-objects (which is derived from that given above for seven stimulus-objects): 1-2, 6-4, 5-1, 3-2, 5-6, 1-3, 2-4, 6-1, 4-3, 5-2, 1-4, 3-5, 2-6, 4-5, 3-6. The spacings between pairs involving various stimuli are given in Table II, from which it can be seen that spacings of not only $(n-4)/2$ and $(n-2)/2$, but also of $n-2$, occur. The number of space-errors, moreover, does not bear thinking on. A superior list is given by: 1-2, 6-3, 5-4, 3-1, 4-2, 6-5, 1-4, 5-3, 2-6, 5-1, 6-4, 3-2, 1-6, 2-5, 4-3. The corresponding spacings are given in Table III, from which it can be seen that they are (more) symmetrically distributed, and with smaller variance; that the total spacings for each stimulus-object have a smaller variance, and the grand total of spacings attains the theoretical maximum of $n\ (n-2)^2/2$.

TABLE II

SPACINGS BETWEEN SUCCESSIVE OCCURRENCES OF EACH STIMULUS FOR ROSS'S LIST FOR SIX STIMULI

| Stimulus | 1st & 2nd | 2nd & 3rd | 3rd & 4th | 4th & 5th | Total |
|---|---|---|---|---|---|
| 1 | 1 | 2 | 1 | 2 | 6 |
| 2 | 2 | 2 | 2 | 2 | 8 |
| 3 | 1 | 2 | 2 | 2 | 7 |
| 4 | 4 | 1 | 1 | 2 | 8 |
| 5 | 1 | 4 | 1 | 1 | 7 |
| 6 | 2 | 2 | 4 | 1 | 9 |
| | | | | | 45 |

TABLE III

SPACINGS BETWEEN SUCCESSIVE OCCURRENCES OF EACH STIMULUS FOR THE SUGGESTED LIST FOR SIX STIMULI

| Stimulus | 1st & 2nd | 2nd & 3rd | 3rd & 4th | 4th & 5th | Total |
|---|---|---|---|---|---|
| 1 | 2 | 2 | 2 | 2 | 8 |
| 2 | 3 | 3 | 2 | 1 | 9 |
| 3 | 1 | 3 | 3 | 2 | 9 |
| 4 | 1 | 1 | 3 | 3 | 8 |
| 5 | 2 | 1 | 1 | 3 | 7 |
| 6 | 3 | 2 | 1 | 1 | 7 |
| | | | | | 48 |

TABLE IV

SUGGESTED METHOD FOR GENERATING LISTS FOR AN EVEN NUMBER OF STIMULUS-OBJECTS

The pairs are read across the successive rows. For the order of the members within each pair, see text.

| | | | | |
|---|---|---|---|---|
| $1-2$ | $3-n$ | $4-(n-1)$ | . . . | $(n+2)/2-(n+4)/2$ |
| $1-3$ | $4-2$ | $5-n$ | . . . | $(n+4)/2-(n+6)/2$ |
| $1-4$ | $5-3$ | $6-2$ | . . . | $(n+6)/2-(n+8)/2$ |
| . | . | . | . . . | . |
| . | . | . | . . . | . |
| . | . | . | . . . | . |
| $1-n$ | $2-(n-1)$ | $3-(n-2)$ | | $n/2-(n+2)/2$ |

Such lists for an even number of stimulus-objects may be generated from Table IV, as has been done in the list given above. Space-errors may be kept to a prov-

TABLE V

BALANCED ORDERS FOR THREE TO FIFTEEN STIMULUS-PAIRS

$n=3$

1-2
3-1
2-3

$n=4$

1-2
4-3
3-1
4-2
1-4
2-3

$n=5$

1-2
3-5
4-1
2-3
5-4
1-3
4-2
5-1
3-4
2-5

$n=6$

1-2
6-3
5-4
3-1
4-2
6-5
1-4
5-3
2-6
5-1
6-4
3-2
1-6
2-5
4-3

$n=7$

1-2
3-7
4-6
5-1
2-3
7-4
6-5
1-3
4-2
5-7
6-1
3-4
2-5
7-6
1-4
5-3
6-2
7-1
4-5
3-6
2-7

$n=8$

1-2
8-3
7-4
5-6
3-1
4-2
8-5
6-7
1-4
5-3
2-6
7-8
5-1
6-4
3-7
8-2
1-6
7-5
4-8
2-3
7-1
8-6
5-2
3-4
1-8
2-7
6-3
4-5

$n=9$

1-2
3-9
4-8
5-7
6-1
2-3
9-4
8-5
7-6
1-3
4-2
5-9
6-8
7-1
3-4
2-5
9-6
8-7
1-4
5-3
6-2
7-9
8-1
4-5
3-6
2-7
9-8
1-5
6-4
7-3
8-2
9-1
5-6
4-7
3-8
2-9

$n=10$

1-2
10-3
9-4
5-8
7-6
3-1
4-2
10-5
6-9
8-7
1-4
5-3
2-6
7-10
9-8
5-1
6-4
3-7
8-2
10-9
1-6
7-5
4-8
9-3
2-10
7-1
8-6
5-9
10-4
3-2
1-8
9-7
6-10
2-5
4-3
9-1
10-8
7-2
3-6
5-4
1-10
2-9
8-3
4-7
6-5

$n=11$

1-2
3-11
4-10
5-9
6-8
7-1
2-3
11-4
10-5
9-6
8-7
1-3
4-2
5-11
6-10
7-9
8-1
3-4
2-5
11-6
10-7
9-8
1-4
5-3
6-2
7-11
8-10
9-1
4-5
3-6
2-7
11-8
10-9
1-5
6-4
7-3
8-2
9-11
10-1
5-6
4-7
3-8
2-9
11-10
1-6
7-5
8-4
9-3
10-2
11-1
6-7
5-8
4-9
3-10
2-11

$n=12$

1-2
12-3
11-4
5-10
9-6
7-8
3-1
4-2
12-5
6-11
10-7
8-9
1-4
5-3
2-6
7-12
11-8
9-10
5-1
6-4
3-7
8-2
12-9
10-11
1-6
7-5
4-8
9-3
2-10
11-12
7-1
8-6
5-9
10-4
3-11
12-2
1-8
9-7
6-10
11-5
4-12
2-3
9-1
10-8
7-11
12-6
5-2
3-4
1-10
11-9
8-12
2-7
6-3
4-5
11-1
12-10
9-2
3-8
7-4
5-6
1-12
2-11
10-3
4-9
8-5
6-7

$n=13$

1-2
3-13
4-12
5-11
6-10
7-9
8-1
2-3
13-4
12-5
11-6
10-7
9-8
1-3
4-2
5-13
6-12
7-11
8-10
9-1
3-4
2-5
13-6
12-7
11-8
10-9
1-4
5-3
6-2
7-13
8-12
9-11
10-1
9-11
10-1
4-5
3-6
2-7
13-8
12-9
11-10
1-5
6-4
7-3
8-2
9-13
10-12
11-1
5-6
4-7
3-8
2-9
13-10
12-11
1-6
7-5
8-4
9-3
10-2
11-13
12-1
6-7
5-8
4-9
3-10
2-11
13-12
1-7
8-6
9-5
10-4
11-3
12-2
13-1
7-8
6-9
5-10
4-11
3-12
2-13

$n=14$

1-2
14-3
13-4
5-12
11-6
7-10
9-8
3-1
4-2
14-5
6-13
12-7
8-11
10-9
1-4
5-3
2-6
7-14
13-8
9-12
11-10
5-1
6-4
3-7
8-2
14-9
10-13
12-11
1-6
7-5
4-8
9-3
2-10
11-14
13-12
7-1
8-6
5-9
10-4
3-11
12-2
14-13
1-8
9-7
6-10
11-5
4-12
13-3
2-14
9-1
10-8
7-11
12-6
5-13
14-4
3-2
1-10
11-9
8-12
13-7
6-14
2-5
4-3
11-1
12-10
9-13
14-8
7-2
3-6
5-4
1-12
13-11
10-14
2-9
8-3
4-7
6-5
13-1
14-12
11-2
3-10
9-4
5-8
7-6
1-14
2-13
12-3
4-11
10-5
6-9
8-7

$n=15$

1-2
3-15
4-14
5-13
6-12
7-11
8-10
9-1
2-3
15-4
14-5
13-6
12-7
11-8
10-9
1-3
4-2
5-15
6-14
7-13
8-12
9-11
10-1
3-4
2-5
15-6
14-7
13-8
12-9
11-10
1-4
5-3
6-2
7-15
8-14
9-13
10-12
11-1
4-5
3-6
2-7
15-8
14-9
13-10
12-11
1-5
6-4
7-3
8-2
9-15
10-14
11-13
12-1
5-6
4-7
3-8
2-9
15-10
14-11
13-12
1-6
7-5
8-4
9-3
10-2
11-15
12-14
13-1
6-7
5-8
4-9
3-10
2-11
15-12
14-13
1-7
8-6
9-5
10-4
11-3
12-2
13-15
14-1
7-8
6-9
5-10
4-11
3-12
2-13
15-14
1-8
9-7
10-6
11-5
12-4
13-3
14-2
15-1
8-9
7-10
6-11
5-12
4-13
3-14
2-15

be seen that spacings of not only $(n-4)/2$ and $(n-2)/2$, but also of $n-2$, occur. The number of space-errors, moreover, does not bear thinking on. A superior list is given by: 1-2, 6-3, 5-4, 3-1, 4-2, 6-5, 1-4, 5-3, 2-6, 5-1, 6-4, 3-2, 1-6, 2-5, 4-3. The corresponding spacings are given in Table III, from which it can be seen that

TABLE II

Spacings between Successive Occurrences of Each Stimulus for Ross's List for Six Stimuli

| Stimulus | 1st & 2nd | 2nd & 3rd | 3rd & 4th | 4th & 5th | Total |
|---|---|---|---|---|---|
| 1 | 1 | 2 | 1 | 2 | 6 |
| 2 | 2 | 2 | 2 | 2 | 8 |
| 3 | 1 | 2 | 2 | 2 | 7 |
| 4 | 4 | 1 | 1 | 2 | 8 |
| 5 | 1 | 4 | 1 | 1 | 7 |
| 6 | 2 | 2 | 4 | 1 | 9 |
| | | | | | 45 |

TABLE III

Spacings between Successive Occurrences of Each Stimulus for the Suggested List for Six Stimuli

| Stimulus | 1st & 2nd | 2nd & 3rd | 3rd & 4th | 4th & 5th | Total |
|---|---|---|---|---|---|
| 1 | 2 | 2 | 2 | 2 | 8 |
| 2 | 3 | 3 | 2 | 1 | 9 |
| 3 | 1 | 3 | 3 | 2 | 9 |
| 4 | 1 | 1 | 3 | 3 | 8 |
| 5 | 2 | 1 | 1 | 3 | 7 |
| 6 | 3 | 2 | 1 | 1 | 7 |
| | | | | | 48 |

TABLE IV

Suggested Method for Generating Lists for an Even Number of Stimulus-Objects

The pairs are read across the successive rows. For the order of the members within each pair, see text.

| | | | | |
|---|---|---|---|---|
| $1-2$ | $3-n$ | $4-(n-1)$ | . . . | $(n+2)/2-(n+4)/2$ |
| $1-3$ | $4-2$ | $5-n$ | . . . | $(n+4)/2-(n+6)/2$ |
| $1-4$ | $5-3$ | $6-2$ | . . . | $(n+6)/2-(n+8)/2$ |
| . | . | . | . . . | . |
| . | . | . | . . . | . |
| . | . | . | . . . | . |
| $1-n$ | $2-(n-1)$ | $3-(n-2)$ | | $n/2-(n+2)/2$ |

they are (more) symmetrically distributed, and with smaller variance; that the total spacings for each stimulus-object have a smaller variance, and the grand total of spacings attains the theoretical maximum of $n\ (n-2)^2/2$.

Such lists for an even number of stimulus-objects may be generated from Table IV, as has been done in the list given above. Space-errors may be kept to a prov-

## TABLE V

### BALANCED ORDERS FOR THREE TO FIFTEEN STIMULUS-PAIRS

*n*=3

1-2
3-1
2-3

*n*=4

1-2
4-3
3-1
4-2
1-4
2-3

*n*=5

1-2
3-5
4-1
2-3
5-4
1-3
4-2
5-1
3-4
2-5

*n*=6

1-2
6-3
5-4
3-1
4-2
6-5
1-4
5-3
2-6
5-1
6-4
3-2
1-6
2-5
4-3

*n*=7

1-2
3-7
4-6
5-1
2-3
7-4
6-5
1-3
4-2
5-7
6-1
3-4
2-5
7-6
1-4
5-3
6-2
7-1
4-5
3-6
2-7

*n*=8

1-2
8-3
7-4
5-6
3-1
4-2
8-5
6-7
1-4
5-3
2-6
7-8
5-1
6-4
3-7
8-2
1-6
7-5
4-8
2-3
7-1
8-6
5-2
3-4
1-8
2-7
6-3
4-5

*n*=9

1-2
3-9
4-8
5-7
6-1
2-3
9-4
8-5
7-6
1-3
4-2
5-9
6-8
7-1
3-4
2-5
9-6
8-7
1-4
5-3
6-2
7-9
8-1
4-5
3-6
2-7
9-8
1-5
6-4
7-3
8-2
9-1
5-6
4-7
3-8
2-9

*n*=10

1-2
10-3
9-4
5-8
7-6
3-1
4-2
10-5
6-9
8-7
1-4
5-3
2-6
7-10
9-8
5-1
6-4
3-7
8-2
10-9
1-6
7-5
4-8
9-3
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7-1
8-6
5-9
10-4
3-2
1-8
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2-5
4-3
9-1
10-8
7-2
3-6
5-4
1-10
2-9
8-3
4-7
6-5

*n*=11

1-2
3-11
4-10
5-9
6-8
7-1
2-3
11-4
10-5
9-6
8-7
1-3
4-2
5-11
6-10
7-9
8-1
3-4
2-5
11-6
10-7
9-8
1-4
5-3
6-2
7-11
8-10
9-1
4-5
3-6
2-7
11-8
10-9
1-5
6-4
7-3
8-2
9-11
10-1
5-6
4-7
3-8
2-9
11-10
1-6
7-5
8-4
9-3
10-2
11-1
6-7
5-8
4-9
3-10
2-11

*n*=12

1-2
12-3
11-4
5-10
9-6
7-8
3-1
4-2
12-5
6-11
10-7
8-9
1-4
5-3
2-6
7-12
11-8
9-10
5-1
6-4
3-7
8-2
12-9
10-11
1-6
7-5
4-8
9-3
2-10
11-12
7-1
8-6
5-9
10-4
3-11
12-2
1-8
9-7
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11-5
4-12
2-3
9-1
10-8
7-11
12-6
5-2
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8-12
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11-1
12-10
9-2
3-8
7-4
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1-12
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8-5
6-7

*n*=13

1-2
3-13
4-12
5-11
6-10
7-9
8-1
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13-4
12-5
11-6
10-7
9-8
1-3
4-2
5-13
6-12
7-11
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9-1
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11-13
12-1
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4-9
3-10
2-11
13-12
1-7
8-6
9-5
10-4
11-3
12-2
13-1
7-8
6-9
5-10
4-11
3-12
2-13

*n*=14

1-2
14-3
13-4
5-12
11-6
7-10
9-8
3-1
4-2
14-5
6-13
12-7
8-11
10-9
1-4
5-3
2-6
7-14
13-8
9-12
11-10
5-1
6-4
3-7
8-2
14-9
10-13
12-11
1-6
7-5
4-8
9-3
2-10
11-14
13-12
7-1
8-6
5-9
10-4
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12-2
14-13
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9-1
10-8
7-11
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14-4
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14-8
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10-14
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13-1
14-12
11-2
3-10
9-4
5-8
7-6
1-14
2-13
12-3
4-11
10-5
6-9
8-7

*n*=15

1-2
3-15
4-14
5-13
6-12
7-11
8-10
9-1
2-3
15-4
14-5
13-6
12-7
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10-4
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12-2
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14-1
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2-13
15-14
1-8
9-7
10-6
11-5
12-4
13-3
14-2
15-1
8-9
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6-11
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3-14
2-15

able minimum of $n$-2 by reversing the order of the members within each pair for all pairs in even numbered rows in the first column, all pairs in the third column, and all odd numbered columns thereafter, and the first pair in the second column. (This last reversal does not decrease the total number of space-errors, but increases the number of pairs between the two components of the first space-error).

For the investigator's convenience in routine use of the method of paired comparisons, Table V gives lists constructed on the above principles for $n = 3$ to $n = 15$. A certain degree of randomization can and should still be achieved (1) by assigning the numbers 1, 2 . . . . $n$ to the stimulus-objects at random, and (2) by deciding randomly whether to use a list forwards or backwards.

The University,
Hull, England

J. P. N. Phillips

## *A POSTERIORI* CORRECTION FOR GUESSING IN RECOGNITIVE TASKS

Many psychological and psychophysical procedures incorporate a series of 'tasks' (test items, pure tones, flashes of light, etc.) and require every subject ($S$) to respond to each task whether or not he can distinguish the correct response. Most prominent among these are the auditory, visual, and tactile psychophysical procedures for obtaining $S$'s difference-limen (DL).

For the purpose of this note, all procedures of interest will have the following properties: (a) They require a simple, categorized response; (b) they require a response to each task; and (c) the $S$s can respond correctly by guessing. An example of a procedure to which the methods of this note apply is the common task of pitch-discrimination, in which the $S$s respond 'yes' or 'no' as they perceive, or think they perceive, the first of two tones as pitched higher than the second. A response equivalent to "I don't know" is usually not permitted. The $DL$ for $S$ is established on the basis of the proportion of $S$'s responses correct at various levels of difficulty of discrimination. Originally, the $DL$ was the point at which 50% of the responses were correct; but common usage shifted to 75%, apparently in an attempt to correct for guessing.

A $DL$ may be very sensitive to guessing if the guessing pattern is non-random. Non-random guessing can be crucial in situations in which $DL$s are found separately for the right ear and left ear, or right hand and left hand, etc. Often, one assumes random guessing and thus applies an *a priori* correction for guessing. Evidence indicates that this assumption is often grossly violated. Many *S*s exhibit marked "response-sets," *i.e.* tendencies to respond in specific non-random patterns when confronted by difficult tasks.[1] Some evidence of extreme response-sets is presented later.

There have been serious attempts to arrive at something other than an *a priori* correction for guessing, some of them very sophisticated as well as very complicated. A simple method is given below for estimating from *S*'s responses the number of correct responses which did not arise from guessing.

Assume that a series of $T_1$ tasks, on which Alternative 1 is correct, is presented to each *S*. Denote *S*'s probability of choosing *1* when he does not know the correct alternative by $P_{S1}$. *S* responds correctly to $R_{S1}$ of the $T_1$ tasks. $R_{S1}$ can be thought of as being composed of $K_{S1}$, the number of tasks in which *S* knows the correct response, and $C_{S1}$, the number of times he guesses the correct alternative, *i.e.* $R_{S1} = K_{S1} + C_{S1}$. A logical estimator of $C_{S1}$ is given below:

$$\text{Est } (C_{S1}) = p_{S1}(T_1 - K_{S1}). \qquad [1]$$

If we assume that *S* always chooses the correct alternative when he knows which is correct, then the number of wrong responses by *S* on the $T_1$ tasks, $W_{S1}$, is estimated by $(1 - p_{S1})(T_1 - K_{S1})$. This assumption fails when *S* is confused or uncoöperative, for example. The following estimators are defined in order to find an estimator of $K_{S1}$:

$$\text{Est } (W_{S1}) = (1 - p_{S1})(T_1 - K_{S1}), \qquad [2]$$

$$\text{Est } (C_{S1}) = (p_{S1}W_{S1})/(1 - p_{S1}). \qquad [3]$$

An estimate of the number of tasks on which *S* knows that the correct alternative is *1* is given by Formula [4]:

$$\text{Est } (K_{S1}) = R_{S1} - [(p_{S1}W_{S1})/(1 - p_{S1})]. \qquad [4]$$

Similarly, if *T* denotes the total number of tasks, regardless of which alternative is correct, an estimate of the number of tasks to which *S* knows the correct response is given by

$$\text{Est } (K_S) = R_S - \sum_{i=1}^{k} [(p_{Si}W_{Si})/(1 - p_{S1})], \qquad [5]$$

where $R_S$ is the total number of correct responses in *T* tasks, $P_{Si}$ is an estimate of S's probability of choosing Alternative *i* when ignorant of the correct alternative, $W_{Si}$ is the number of wrong responses by *S* on all those tasks to which Alternative *i* is correct.

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[1] L. J. Cronbach, Response sets and test validity, *Educ. Psychol. Measmt.*, 6, 1946, 475-494; Further evidence on response sets and test design, *ibid.*, 10, 1950, 3-31; J. P. Guilford, *Psychometric Methods*, 2nd ed., 1954, 451-456.

The important point to be made is the $P_{Si}$ may often be estimated from certain of *S*'s responses. For example, the above example of pitch-discrimination, the proportion of times an *S* responded "yes" when two tones of *equal* pitch were presented to him can be an estimate of $p_{S1}$. If one wishes an estimate of the number of tasks to which *S* knew that the correct response was "yes," one need only substitute the proper values into Formula [4].

The following example is intended only to illustrate the application of the *a posteriori* correction.

Two pure tones were sounded simultaneously in *S*'s earphones. *S* responded "right" or "left" accordingly as he perceived the louder of the tones to be in the right or left ear. (A midline balance was first achieved for each *S*, *i.e.* adjustments made for differential perceptions of tones measured by the apparatus as equal.) The tones then differed by either 4.0, 3.2, 2.4, 1.6, 1.2, 0.8, or 0.4 db. Twenty presentations at each difference-level for both "right louder" and "left louder" were made. In addition, 20 presentations were made in which the tones were of equal loudness ("equal" as described by each *S* originally). *S* was thus exposed to 300 presentations, randomly ordered.

TABLE I

COMPARISON OF *A Posteriori* AND *A Priori* CORRECTED SCORES FOR A SINGLE *S*

| Difference in tones (db.) | Observed no. correct responses | | *A posteriori* corrected score* | | *A priori* corrected score* | |
|---|---|---|---|---|---|---|
| | *R* | *L* | *R* | *L* | *R* | *L* |
| 4.0 | 20 | 16 | 20 | 14 | 20 | 12 |
| 3.2 | 20 | 15 | 20 | 13 | 20 | 10 |
| 2.4 | 20 | 13 | 20 | 10 | 20 | 6 |
| 1.6 | 19 | 10 | 17 | 6 | 18 | 0 |
| 1.2 | 18 | 5 | 13 | −1 | 16 | −10 |
| 0.8 | 17 | 4 | 10 | −3 | 14 | −12 |
| 0.4 | 18 | 7 | 13 | 1 | 16 | −6 |
| 0 | 14 | 6 | — | — | — | — |

* To the nearest integer.

The responses of a single *S* were chosen and appear in both *a posteriori* and *a priori* corrected form in Table I.[2]

The *a priori* correction assumes any *S* responds "right" or "left" with equal probability when ignorant of the correct response. The *a posteriori* correction, however, uses an estimate of *S*'s probability of guessing each alternative from certain of *S*'s own responses. Note that the *S* whose responses are tabulated chose "right" 14 times in 20 when the two tones did not differ in loudness. Consequently, the estimates of this *S*'s probability of responsing "right" when ignorant of the correct response was taken as 0.7. Formula [5] was applied to the data in Columns 1 and 2 to yield the *a posteriori* corrected number correct in columns 3 and 4. It is clear that the *a posteriori* correction compensates for *S*'s bias to respond "right" when ignorant of the correct response, while *a priori* correction does not. The entries in Columns 3 through 6, the "corrected scores," are estimates of the number of correct responses *S* would have made had he not guessed. Obviously then, a negative score has no meaning; negative scores in Column 4 arise from errors made in estimating the true probability of *S*s always guessing a certain alternative from *S*'s responses when no "correct" alternative exists; *e.g.* when the two sounds do not differ in intensity. The large negative scores in Column 6 are a result of assuming *S* guesses "right" or "left" with equal probability.

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[2] The authors wish to thank Dr. Irving Silverman for permission to use his data.

The *S* chosen to illustrate the method was not at all extreme. One individual exhibited a bias of 20-0 on two separate occasions, and "zero responses" of 17-3 were not uncommon (64 *S*s participated). *DL*s obtained from the corrected scores were quite different from those using uncorrected scores.

Application of the method is clearly limited to situations in which some measure of $p_{Si}$, *S*'s bias, is available. This restriction is at present more severe than necessary, however, since presentations from which $p_{Si}$ can be estimated may be inserted in many procedures. The restriction of importance is then the necessity that each *S* answer every item.

University of Wisconsin

GENE V. GLASS
LESLIE D. MCLEAN

## ON THE PERCEPTION OF WORDS

In a recent note in this JOURNAL,[1] Anisfeld criticized the conclusion of Gibson, Pick, Osser, and Hammond that invariant grapheme-phoneme correspondence is a determinant of perceptibility of visually presented verbal material.[2] His criticism rested on the argument that summed bigram and trigram letter-frequencies could also predict recognition under the conditions of the experiment. In the experiment, pseudo-words, which had a regular pronunciation according to rules of English (pronounceable words), were more often recognized correctly with a tachistoscopic presentation than pseudo-words which did not (unpronounceable words). Every pronounceable word had an unpronounceable counterpart constructed of the same letters, so that summed letter-frequency was automatically controlled.

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[1] Moshe Anisfeld, A comment on "The role of grapheme-phoneme correspondence in the perception of words," this JOURNAL, 77, 1964, 320-321.

Anisfeld pointed out that summed bigram and trigram frequencies were not controlled and that these counts were higher for the pronounceable pseudo-words, in many cases, than for their unpronounceable counterparts. He also pointed out that pronunciability and bigram and trigram frequencies tend to covary. In answer to his argument, we wish to present the following correlations, which were calculated from the data published and might well have been obtained by Anisfeld himself.

TABLE I

CORRELATIONS OF POSSIBLE PREDICTORS WITH THE NUMBER OF PSEUDO-WORDS CORRECTLY RECOGNIZED

| | |
|---|---|
| Mean trigram frequency, all items | .03 |
| Mean bigram frequency, all items | −.01 |
| Pronunciability, all items | −.65 |
| Trigram frequency, 5-letter items | .54 |
| Bigram frequency, 5-letter items | .74 |
| Pronunciability, 5-letter items | −.83 |
| Trigram frequency, 5-letter items (pronunciability constant) | .34 |
| Bigram frequency, 5-letter items (pronunciability constant) | .37 |

($r$ must be $=0.48$ with 15 degrees of freedom for $p<0.05$.)

Do summed bigram frequency and trigram frequency actually predict the perceptibility of words? It is possible to sum the bigrams or trigrams sequentially across each word and then to correlate its summed frequency with the number of times it was correctly recognized. Longer words, however, necessarily have higher sums than shorter ones and longer words are less often perceived correctly. Since the lengths of the pseudo-words tested varied, mere sums would give a bias toward a negative correlation. The sums for each word were therefore divided by the number of component bigrams or trigrams in each case, and the mean summed bigram and trigram frequencies for each of the 50 pseudo-words was correlated with the number of times it was perceived correctly. In Table I, it can be seen that the correlations are −0.01 and 0.03, respectively. Therefore, summed bigram and trigram frequency do *not* predict recognizability of pseudo-words.

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[2] E. J. Gibson, Anne Pick, Harry Osser, and Marcia Hammond, The role of grapheme-phoneme correspondence in the perception of words, this JOURNAL, 75, 1962, 554-570.

On the other hand, pronunciability of a pseudo-word *does* predict its recognizability under conditions of tachistoscopic viewing. The pseudo-words were all rated for pronunciability.[3] These ratings were correlated with the number of correct recognitions, yielding a correlation of −0.65 for all 50 words (significant at <0.01). (The sign is negative because high pronunciability was rated 1.)

It is also possible to run correlations without a correction for length if only the 16 five-letter pseudo-words are considered. When summed bigram and trigram frequencies are correlated with the number of correct recognitions for these items, the correlations are 0.74 and 0.54 respectively. These correlations are significant at <0.05. But, as Anisfeld remarked, these frequency counts are correlated with pronunciability ($r =$ −0.72 and −0.46). When the effect of pronunciability is held constant, the correlations between the frequency counts and successful recognition are reduced to 0.37 and 0.34, neither one significant at the 0.05 level of confidence. But the correlation between pronunciability and successful recognition for these 16 words is −0.83, highly significant.

There seems no doubt, from these correlations and from positive results in two further experiments wherein trigram frequency was controlled, that pronunciability of a set of graphemes is indeed an important determinant of its perceptibility by skilled readers.[4] In contrast, there is no evidence that summed frequencies of single letters, bigrams, trigrams, or any other arbitrary letter sequence is effective.

Cornell University — ELEANOR J. GIBSON

## DISTRIBUTION OF EFFORT IN MEMORIZING

In the early 1930s, I memorized nine 50-line selections from Milton's *Paradise Lost* with intervals between successive readings ranging from 0 to 8 days.[1] As the results tabulated below show, the longer the intervals between successive readings, the fewer the number of readings required for complete mastery.

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[3] See Gibson, *et al.*, *op. cit.*, for details.

[4] E. J. Gibson, Harry Osser, and A. D. Pick, A study of the development of grapheme-phoneme correspondences, *J. verb. Learn. verb. Behav.*, 2, 1963, 142-146; E. J. Gibson, C. H. Bishop, William Schiff, and Jesse Smith, Comparison of meaningfulness and pronunciability as grouping principles in the perception and retention of verbal material, *J. exp. Psychol.*, 67, 1964, 173-182.

[1] A. P. Bumstead, Distribution of effort in memorizing prose and poetry, this JOURNAL, 53, 1940, 423-427.

| | Intervals between readings | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 0 | 1 hr. | 2 hr. | 4 hr. | 8 hr. | 1 d. | 2 d. | 4 d. | 8 d. |
| No. of readings | 49 | 43 | 34 | 28 | 24 | 19 | 16 | 15 | 14 |

When one selection was read continuously, 49 readings were required, but when rest periods of 1 hr. were inserted between successive readings, only 43 were needed. (Actually, the learning in this experiment was spread over 3 days with 14 readings on each of the first two days, and 15 on the third, and with 8 hr. out for sleep after the first and second days.) As the length of the intervals between successive readings increased, the number required for complete mastery decreased. Only 14 were required when the readings were 8 days apart.

I asked myself why the number of repetitions required for learning should decrease as the length of the intervals between successive readings increased; in other words, why should the rate of memorizing be a function of the length of the interval between successive readings?

Results of experiments by others are comparable. Erickson, Ingram, and Young had 3 groups of *Ss*, learn a list of 14 paired-associate nonsense-syllables.[2] Group I learned the list at a rate of 1.5:1.5-sec. presentation with a 3-sec. intertrial interval. Group II learned the list at a rate of a 2:2-sec. presentation with a 4-sec. intertrial interval; and Group III learned the list at a rate of 4:4-sec. presentation with an 8-sec. intertrial interval. They found that paired-associate learning was a function of the rate of presentation.

Kientzle had *Ss* practice printing the alphabet upside down. Trials were uniformly of 1-min. duration. the *Ss* accomplishing as much as they could in that time. The intervals inserted between trials ranged from 0 to 90-sec., with one group receiving one trial every 7 days. In the words of Osgood, who described and discussed the experiment: "The amount of facilitation in learning attributable to intertrial distribution is a negatively accelerated function of the length of the interval."[3]

The results of these experiments are in fundamental agreement that learning is a function of the interval between successive practice periods, but they do not tell why this is true. Neurophysiologists may be developing an hypothesis that will account for this relationship as they commonly believed that when a memory trace is being laid down or recalled, electric currents pass among the particular neurons that are activated by the event or recollection that is being experienced. The passage of such currents from neuron to neuron serves to strengthen them, thereby making it easier for the same neuronal patterns to be activated in the future. Sensory centers of the brain are to be regarded, however, not merely as the organs of consciousness of immediate sensory experience. Russell has advanced the idea that a trained central nervous system will direct new arrivals into well-established channels.[4] This will automatically strengthen the memory traces that have already been laid down. In this way, the longer the memory-trace has been in the storage, the stronger it will be. If this hypothesis is valid, it suggests that in memorizing poetry fewer repetitions would be necessary when the intervals between learning periods were longer, because the electric currents would be active longer. It also provides a plausible explanation for the superiority of distributed over massed learning.

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[2] C. C. Erickson, R. D. Ingram, and R. K. Young, Paired-associate learning as a function of rate of presentation and prior serial learning, this JOURNAL, 76, 1963, 458-463.

[3] C. E. Osgood, Method and theory in experimental psychology, 1953, 506.

If a selection were memorized by reading it once every 8 days, the electric currents activated by the first reading would be functioning for 104 days. The currents caused by the second would be active for 96 days, the third for 88 days, and so on. The currents caused by the 13th reading would be active for 8 days; and after the 14th reading the selection would be memorized. In the aggregate, the electric currents would be functioning for 728 days, or 17,472 hr. Similarly, if a selection were memorized by 43 repetitions spaced 1 hr. apart, the currents would be active for 42 hr., plus 41 hr., plus 40 hr., and so on for a total of 903 hr. If 16 hr. had been taken out for sleep, this should be included, making the total 919 hr. If it took 3 min. to read a 50-line passage, and the learning was not distributed, but was massed, that is to say it was read 49 times without interruption, the currents would be active for 144 min. plus 141 min. plus 138 min. . . . 3 min. for a total of 3,498 min. or 58 hrs.

It appears that an answer is at hand to the question I have been asking myself for 30 yr.

San Francisco, California
ARTHUR P. BUMSTEAD

## THE BOYS FROM SYRACUSE: ANOTHER AMBIGUOUS FIGURE

This note adds a new ambiguous figure to the psychologist's store of those whose alternation can depend upon factors more psychological in nature than intensity, contrast, and breadth of stroke. It was drawn by the eminent caricaturist Al Hirschfeld to advertise the musical, "The Boys from Syracuse," and appeared in the Drama Section of *The New York Times* of 29 September 1963.[1]

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[4] W. R. Russell, *Brain, Memory, Learning,* 1959, 14.

Psychologists who have studied the perception of ambiguous visual figures have most often used simple contours, such as Necker's Cube, Rubin figures, or adjacent profiles, as stimuli. When the figures are constructed on a basic Y-like shape, they usually are ambiguous with respect to the third dimension, *S* perceiving changes in perspective. In the other cases, one variant of the contour is seen as figure, the other as ground. But even with these structurally simple figures, set, intention, and like cognitive processes play a role in facilitating or inhibiting alternations in a manner at least as striking as that attributed to variations in the optic array.[2] Another kind of variation is due to more contextually dependent information, as in the Wife and Mother-in-Law figure,[3] for example, and in the present one.

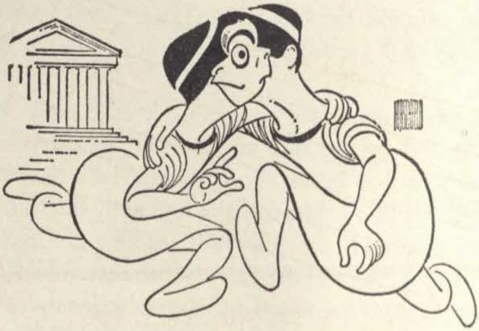

FIG. 1. THE BOYS FROM SYRACUSE

It is interesting to note the absence of a change in apparent depth in the figure presented here as first one boy and then the other becomes dominant. Alternating the two parts of the figure rapidly might produce such a depth-effect.[4]

Massachusetts Institute of Technology PAUL A. KOLERS

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[1] Copyright by *The New York Times*. Reprinted by permission of *The New York Times* and Mr. Hirschfeld.

[2] J. S. Bruner, Leo Postman, and F. Mosteller, A note on the measurement of reversals of perspective, *Psychometrika*, 15, 1950, 63-72; Leon Cohen, Rate of apparent change of a Necker Cube as a function of prior stimulation, this JOURNAL, 72, 1959, 327-344.

[3] E. G. Boring, A new ambiguous figure, this JOURNAL, 42, 1930, 444-445.

[4] P. A. Kolers, Apparent movement of a Necker Cube, this JOURNAL, 77, 1964, 220-230.

## A NEW AMBIGUOUS FIGURE: A THREE-STICK CLEVIS

The accompanying figure (Fig. 1) recently printed in the advertising section of an aviation journal,[1] is reproduced here because it is, as I believe, a new type or kind of ambiguous figure.

Unlike other ambiguous drawings and geometrical figures—Jastrow's "Duck-Rabbit,"[2] Hill's "Wife and Mother-in-Law,"[3] Botwinick's "Father and Father-in-Law,"[4] a duplication of Hill's drawing with a masculine motif, and Necker's Cube, Schroder's Stairway, and Mach's Book[5]—an actual shift in visual fixation is involved in its perception and resolution.

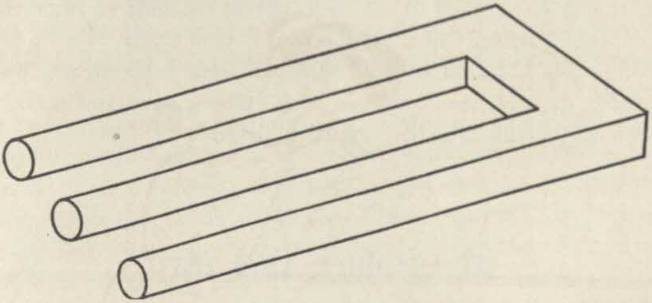

FIG. 1. A NEW AMBIGUOUS FIGURE

When an observer fixates, at reading distance, the left end of the figure, he sees three rods with the right end indistinct and foggy; when he fixates the right end, he sees a U-shaped figure or clevis.

It is only when he fixates the center or allows his fixation to sweep slowly across the figure that he realizes he is viewing an "impossible figure." It is "impossible" like the illusory figures described by Penrose and Penrose,[6] and by the authors to whom they refer.[7]

Collins Radio D. H. SCHUSTER
Cedar Rapids, Iowa

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[1] *Aviation Week and Space Technol.*, 80, 1964 (No. 12, Mar. 23), 5.

[2] Joseph Jastrow, *Fact and Fable in Psychology*, 1900, 295, Fig. 19.

[3] E. G. Boring, A new ambiguous figure, this JOURNAL, 42, 1930, 444-445.

[4] Jack Botwinick, Husband and father-in-law—a reversible figure, this JOURNAL, 74, 1961, 312-313.

[5] For illustrations of these ambiguous geometrical figures, see E. B. Titchener, *Experimental Psychology, Students' Manual, Qualitative,* 1901, p. 154.

[6] L. S. Penrose and R. Penrose, Impossible objects: A special type of illusion, *Brit. J. Psychol*, 49, 1958, 31-33.

[7] M. C. Escher, *Catalogue 118,* 1954, Stedelijk Museum, Amsterdam; and F. P. Kilpatrick, Elementary demonstrations of perceptual phenomena, in *Human Behavior from the Transactional Point of View,* 1952, Institute for Associated Research.

## AN INTERDISCIPLINARY JOURNAL OF THE HISTORY OF THE BEHAVIORAL SCIENCES

The publication of a new interdisciplinary periodical—the *Journal of the History of the Behavioral Sciences*—is announced for January 1, 1965. It is edited by Professor Robert I. Watson of the Department of Psychology, Northwestern University with the cooperation of an international board of editors from the fields of anthropology, history, linguistics, neurology, neurophysiology, psychiatry, psychoanalysis, psychology, and sociology.

The new *Journal,* which is published by the Psychological Press, 4 Conant Square, Brandon, Vermont 05733, is to appear quarterly in numbers 100 pages in length. Its price of subscription is $10.00 a year.

In addition to historical articles in the behavioral fields, to which it will be primarily devoted, it will contain book reviews, news and notes, queries, letters to the editor, and an annual bibliography.

K.M.D.

## Karl Buhler: 1879–1963

Karl Buhler died on October 24, 1963 in Los Angeles at the age of eighty-four years. He was born on May 27, 1879, in the village of Meckesheim near Heidelberg, Germany, which some say is the home of the first *Homo sapiens.* He was born Bühler but dropped the umlaut after he came to America in 1940. His parents were farmers and there is little in his early background to suggest a life of scientific inquiry which spanned two continents and practically the entire history of psychology since its birth as an independent science. A scholarship enabled him to go through the *Gymnasium* at Tauberbischofshein. After graduation he entered the medical school of the University of Freiburg in Baden and received his M.D. in 1903. Much of his time in medical school was devoted to research in physiology as assistant to Johannes von Kries, under whose sponsorship he wrote his doctoral thesis on the "Duplicity theory of color vision." Another doctoral thesis on "Studien über Henry Home," earned him a Ph.D. from the University of Strassburg in 1904.

For a short time he practiced medicine as a ship's doctor, a setting which no doubt was intended to satisfy his never-ending curiosity—this time about the world outside his own country; yet two years later, after some work at the University of Berlin, he joined Oswald Külpe in Würzburg as his assistant. For the following nine years the two men were bound by close collaboration and personal friendship. It was Buhler who made the three final important contributions to Külpe's Würzburg School of imageless thought in 1907-1908: *Tatsachen und Probleme zu einer Psychologie der Denkvoränge*. Buhler became *Professor extraordinarius* (the equivalent, more or less, of an American associate professor) in 1909 at the University of Bonn and in 1913 at the University of Munich. Here he established a research center in experimental psychology. Its laboratory was equipped with the most advanced technical apparatus of that time.

The four years of World War I, 1914-1918, stimulated Karl Buhler's first brush with applied psychology and clinical research. As a medical officer in the German army he organized a Military Psychological Test Service for transport drivers and airplane pilots. Later he collaborated with the psychiatrist Isserlin in both research and rehabilitation of brain-injured soldiers.

At the end of the war, in 1918, he was appointed to a Professor of Psychology at the Technische Hochschule in Dresden and in 1922, at the age of 43, he became Professor of Psychology and Head of the Psychological Institute at the University of Vienna. There he stayed for sixteen years, until the Nazi annexation of Austria in 1938 forced him to leave not only his position but also the country which he had by then adopted.

The years in Vienna represent the most productive and satisfying years of his career. It was here that, in close collaboration with his wife, Charlotte Buhler, he created the Psychological Institute, the closest thing to an American Psychology Department to be found anywhere at that time within the walls of a European university. The Institute offered a varied curriculum of experimentation: theoretical and development psychology, statistics and research methods in child psychology and social psychology, and attracted doctoral and post-doctoral students from many countries and all of those sciences which we now tend to call 'behavioral.' This, too, was the period in which Buhler gained international recognition. In 1927-1928 he taught at Stanford, Johns Hopkins, and Harvard and in 1929 at the University of Chicago. In 1926 the Institute received a ten-year research grant from the Rockefeller Foundation. In 1927 he was awarded an honorary Doctor of Law degree by Wittenberg College and in 1929 he was elected president of the German Psychological Association.

Buhler was then fifty years old and there seemed to be a change of pace. He had made his contribution—at least for the time being—to the psychology of thought processes, of perception, of Gestalt-theory, of the mental development of the child. He had introduced experimentation and naturalistic observation as valid methods of psychological investigation into two major European universities. He had traveled widely and drawn students from all over the world around him. The Institute was well established. He had written and published in 1927 his critical exploration of the anabolic crisis of psychology, the untranslated *Die Krise der Psychologie.* It almost appears as though the phase of preparation and of encyclopedic searching were over and he was now ready to devote himself to one major area, the one for which he is perhaps best known—the theory of language. He joined the Prague Linguistic Circle and in 1931 contributed to it a paper on "Phonetics and phonology." Two years later this was followed by *Ausdruckstheorie* and in 1934 by *Sprachtheorie.* This work was considered by many of his colleagues his most significant contribution. A number of smaller papers on phonetics and language had appeared when the political upheaval in the early spring of 1938 brought his scientific work to a standstill for a dozen years.

He was imprisoned by the Nazis for having supported the anti-Nazi Austrian government and for philosemitic behavior in his private life and in the administration of the Psychological Institute. He was freed with the help of Norwegian friends and eventually brought to the United States.

Buhler's exile was more than political and geographical. Not only was he cut off from his country, his culture, and his language, his friends and, for a while, even from his family, but he was also without his students, his collaborators and the facilities and stimulation of a great university. It is probably too soon to try to understand the failure of communication between American psychology and Karl Buhler in 1939 and all the reasons for the tragic academic isolation which continued for the rest of his life. He started his American career as a Professor of Psychology at Scholastica College in Duluth, Minnesota, and from 1940 to 1945 he taught at St. Thomas College in St. Paul, Minnesota. In 1945 he moved to Los Angeles and returned to a long abandoned early interest. He became a practicing clinician affiliated with Cedars of Lebanon Hospital and was appointed Assistant Clinical Professor of Psychiatry at the University of Southern California.

Karl Buhler was, however, no 'applied' psychologist. During the years following World War II, he gradually emerged from the obscurity of his retirement. Once more he returned to theoretical problems in the two fields which had never lost their fascination for him—biology and Gestalt-theory. Two English publications (1951 and 1954) in *Acta Psychologica* dealt with the question: How do animals orient themselves in space and time? By 1958 the results of his thinking, reading and personal communication with younger men in the field found their way into his last book, written in German, *Das Gestaltprinzip in Leben der Menschen und Tiere.*

In evaluating the contribution of a man whose scientific life started with questioning the validity of classical theory of association and ended with questioning the creative potential of machines, one is tempted to look for one consistent major interest which might account for all of his works. Yet in so doing one would be fitting Buhler into a wrong mold. He did not seek to create a monolithic theoretical system, although at the time of his death he was planning to write a "theoretical psychology" in which he aimed at a "unified concept."

Throughout his life he allowed his curiosity to take him into a diversity of professional areas. A catalogue of his concerns would include the psychologies of thinking, perception, language and child development, as well as theories and systems. He did not look for a single operating principle but in all his work he asked the question *how,* over and over again. How is it done? How does man think? How does he perceive? How does he communicate? How does the child's intellectual mastery of the world develop? How do animals orient themselves in time and space? How did Helen Keller learn foreign languages? Rarely did he ask *what.* Always it was: *How?* In his psychology of thinking Buhler criticized Freud for his preoccupation with the specific content of associations, although he agreed with him on the principle that associations are connected with one another by the meaningfulness of their content.

In his assessment of the state of psychology in 1927, his basic question, paraphrasing Kant, was: How is psychology possible? His answer here was methodological. Psychology has three aspects. It must be studied in three ways; namely, by means of observable behavior, inner experience, and mental products *(geistige Gebilde).* His theory of language is formulated in terms of function. Language is a tool by which one person communicates to another person something about things or events. The functions of language are expression, appeal, and description. In his latest work he returned to his lifelong interests in biology and Gestalt-theory and again his question was: *How?* How do animals relate to Gestalten? Referring to the works of Sauer, Lorenz, and Tinbergen, he emphasized the creative aspect of Gestalt perception in animals. Examples are the night flights of some migratory birds as they are guided by the stars and the nest-building patterns, of certain other birds. Buhler asked further: If organisms respond to Gestalten creatively, how about man-made robots? The conclusion here was that machines are less flexible, less able to relate simultaneously to multiple Gestalt-experiences and that they are therefore less creative.

Throughout his life Buhler was fascinated by the capacity of living organisms to make new connections and to create new forms. He coined one of his best-known terms, the "aha" experience, to describe the sudden realization of having made such a new connection, of having related two or more hitherto unrelated thoughts. He also introduced the concept of "functional pleasure," originally in the context of his critique of Freud's early libido theory, which postulated the need for the discharge of tension as the basic motivating factor in human behavior. Functional pleasure and creative pleasure were Buhler's major concepts in his appeal to psychoanalysis to expand its theory of human motivation to account for creative as well as for pathological human behavior. Had Buhler had an opportunity to review such current psychoanalytic thinking as is represented by Franz Alexander, Anna Freud, Rene Spitz, and Erik Erikson, he would have found much in common with his own earlier work. The concern of ego psychology with problems of normal development and with cognitive functions (Alexander's "surplus energy" in particular and its use in playful activity) is closely related to the general psychological problems in which Buhler was so vitally interested. Undoubtedly he was a pioneer in the study of human creativity, the primary concern of so many present-day psychologists.

Any account of Karl Buhler's life would be incomplete if it did not convey his impact as a teacher. His curiosity was contageous, his language was vivid and rich in new terms, his arguments were vigorous and unexpected, whereas his obvious enjoyment of the business of thinking held his audience of several hundred students spellbound through many courses in general psychology. He treated his students as partners in the most exciting game of all—the raising and solving of problems. He did not leave a school or a clique of followers but he enriched European and American psychology by the large number of distinguished scientists whose commitment to psychology had been inspired by him.

Isaiah Berlin in *The Hedgehog and the Fox* has said that thinkers can be divided into those who relate everything to a single central vision and those who pursue many unrelated ends not trying to fit a wide array of experiences into an all-embracing system. These latter are the foxes who "know many things," while the former are like the hedgehog who "knows one big thing." Was Buhler a fox or a hedgehog? Clearly he knew many things and at no time in his life, except perhaps at the very end, did he attempt to unify and systematize his work. Yet in spite of his fascination with the world's variety, one cannot escape the conviction that there was one very big thing Karl Buhler wanted to know all his life. He wanted to understand the nature of cognitive processes, to know *how* living organisms relate to their environment. He was one of the foxes, who "see the many but all their life agonize to see the one."

Cedars-Sinai Medical Center, Los Angeles HEDDA BOLGAR

# Frederic Lyman Wells: 1884–1964

Frederic Lyman Wells was born April 22, 1884 on Beacon Hill in Boston, in a family firmly rooted in the intellectual heritage of nineteenth-century New England. A great-grandfather was an intimate of Daniel Webster; a grandfather for a time conducted the University Press at Cambridge; a grandmother was related to the Alcotts. His scholarly father, Benjamin W. Wells, seems to have had an important influence on his formative years. The elder Wells, after receiving his Ph.D. degree from Harvard in 1880, took his family with him when he devoted four years to postdoctoral study in Europe from 1887 to 1891. He was Professor of Modern Languages at the University of the South, Sewanee, Tennessee, from 1891 to 1899, and subsequently was an editor and author, residing in New York City.

Young Lyman, as his family always called him, first attended school in Germany, and was able to read German fluently and to converse in it throughout his life. He entered college at Sewanee at the precocious age of fifteen, but after a year transferred to Columbia where he earned his A.B. in 1903. It was after obtaining his M.A. in 1904 that he changed his specialization from Germanic philology to psychology. In his Ph.D. dissertation of 1906, Wells acknowledged his obligations to his teachers —Cattell, Woodworth, and Thorndike. The subject of the dissertation, "Linguistic Lapses," manifested his background and also to a degree forecast his future. It was an experimental study of the misperception of linguistic sounds, using the then new-fangled phonograph and as much phonetics as psychology. But in a sense it was also dynamic psychology ("we hear actually what we expect to hear"), although it cited no reference to Freud's almost contemporary writings. Like many of Wells's later studies, the dissertation gave a minutely detailed analysis of the performance of a small number of subjects which, while drawing general conclusions, did not hesitate also to note the relationships of individual differences to the subjects' personal characteristics.

Immediately after receiving his Ph.D. degree, Wells assumed the then uncommon role of a clinical psychologist which he maintained throughout his career. From 1907 to 1921 he was Assistant in Pathological Psychology at McLean Hospital in Waverley, Massachusetts, with two short spells of other duty as an experimenter and lecturer at the Psychiatric Institute and Columbia in 1910–1911, and as an Army captain supervising selection tests in 1917–1918 for the embryonic Air Service of World War I. From 1921 to 1938, he served as Chief of the Psychological Laboratory of Boston Psychopathic Hospital with a concurrent appointment in the Harvard Medical School with the rank of Assistant Professor from 1932 onward. In 1938 he became Psychologist of the Department of Hygiene of Harvard University, continuing in the post until his retirement in 1950. World War II also drew on his knowledge when, as Consultant to the War Department in 1941–1946, he had much to do with the development of the Army General Classification Test.

The national affairs of the American Psychological Association and of other professional organizations soon made use of F. L. Wells's energy and judgment. When only 26 years old he was, with R. S. Woodworth, a working member of a committee on the standardization of testing procedures, appointed by the APA, an enterprise which resulted in the publication of the influential *Association-Tests* under their joint authorship in 1911 Wells became Chairman of the Clinical Section of the APA in 1922, and was a member of the Association's Council of Directors in 1922–1924. He was a member of the National Research Council from 1925 to 1928. The American Association for Applied Psychology made him its Vice President, Consulting Section, in 1939–1940, and in the same year he was elected President of the Eastern Psychological Association. He was twice President of the American Psychopathological Association, first in 1942–1944 and then in 1946–1947. He was a member of the first American Board of Examiners in Professional Psychology from 1947 to 1949.

From 1905 to 1956, Wells was the author of some 150 articles and monographs and of four books in psychology. His productivity in research maintained an almost constant level for forty years, with from 31 to 35 publications in each decade from 1910 to 1950. His initial concern for phonetics soon evolved into an interest in word-associations, the topic of many of his early articles. Papers on the allied subjects of reaction-time and the galvanic skin response soon emerged. With these came numerous papers whose titles included the then unfamiliar term, "experimental psychopathology." He became one of the first to take the psychological laboratory into the hospital.

It was inevitable that Freud's early work should soon receive Wells's attention. As an experimentalist, given to deliberative judgment rather than to impulse, he saw some values in the new "dynamic" psychology without becoming its adherent. In 1912 he wrote of psychoanalysis, "The subject seems unfortunately apt in turning either a man's stomach or his head." Wells sought a middle ground between emotional rejection and uncritical faith. He evolved it in a series of articles on dynamic psychology, adaptation, and regression which led to his book, *Mental Adjustments,* in 1917. Here in general American psychology began a movement, relating the concept of "adjustment" both to psychopathology and to the everyday life of normal persons, a movement which has become so pervasive that its origins are often clouded. Wells was not alone; his mentor Woodworth published his influential *Dynamic Psychology* in 1918. But Wells helped to establish a rapprochement between general psychology and certain concepts adapted from psychoanalytic mechanisms. Their widespread acceptance throughout the subsequent years is perhaps a measure of his contribution.

His earlier studies of association-tests inevitably led Wells to a broader concern about tests of value for clinical practice, a concern that evoked two books and many journal articles spanning the years from 1916 into the 1950s. Although he often published on norms and other technical aspects of tests, he was always the clinician at heart. He performed no factor analyses and advocated no formalized actuarial methods. The test for him was a tool for understanding persons. He deeply believed in tests as superior to casual and uncontrolled observations but he was always the master of his tools, not their servant. His administration of tests was often flexible to a degree that a strict psychometrician would find distressing, and he was an early advocate of the position that "the examiner must make as much as he can" out of tests. His test-interpretation was analytical in the old sense of drawing detailed inferences from trends which ran through the patient's performances on a variety of tasks. His *Mental Tests in Clinical Practice* (1927), with its insightful practical hints and its illuminating case studies, educated many of the teachers of today's crop of clinicians. When the Rorschach arrived, he was one of the first to use it but did not become a devotee. In a tone consistent with his anti-authoritarian bent, he wrote in 1935 that "one must not contemplate the work of Rorschach, any more than that of Binet, as a faith once delivered to the saints." Although his central interest was clinical, he also contributed to the use of tests in the educational and industrial fields. The widely used Revised Army Alpha (1932) was his product. His last long series of papers, 1947 to 1951, presented detailed psychometric case studies of the Harvard students of the Grant Study. They are still fruitful reading for anyone concerned with the intellectual, personal, social, and adjustment problems of superior youths.

Mainly in the 1920s, but with extensions into the 1940s, Wells wrote extensively on the roles of clinical psychologists, on their training, and on their interprofessional relations. As a representative of the APA, he reported on a conference of psychologists with psychiatrists in the Spring of 1921—perhaps the first such meeting—"to discuss the difficulties of the situation and to promote a better understanding of the respective points of view." There evidently was not too complete a meeting of minds, but Wells concluded that the desirable course for his profession lies in "measures that further the service of psychology to society," and not in those created "principally for the interest of a professional group." Wells's conclusions of 1921 can be appreciated with a wistful smile; committees of the 1940s and the 1960s arrived at the same recommendations and expressed them no better.

After his retirement in 1950, F. L. Wells continued for a time to write in psychology. He also carried on two life-long avocations, the study of spiders, in which he had a scholar's status, and outdoorsmanship in the Appalachian and Adirondack Mountain Clubs. Responding to an appeal for senior volunteer workers in 1956, he went to Metropolitan State Hospital, and a little later returned to McLean Hospital, not as a psychologist but as a volunteer doing any kind of useful chore "so long as it's something that can be done in dungarees." He ended his career where he had begun it, with perceptiveness, social sensitivity, and a great deal of good humor. Frederic Lyman Wells died at Belmont, Massachusetts, on June 2, 1964.

Teachers College
Columbia University

LAURANCE F. SHAFFER

# BOOK REVIEWS

Edited by T. A. RYAN, Cornell University

*Perception and Motion: An Analysis of Space-Structured Behavior.* By KARL U. SMITH and WILLIAM M. SMITH, with the assistance of MARGARET F. SMITH. Philadelphia, W. B. Saunders Company, 1962. Pp. viii, 341. $8.00

The aim of this book is to explore the proposition that spatio-temporally organized behavior sequences correspond to spatio-temporally structured stimulus patterns. To stress their criticism of conventional theories of learning and behavior, still based on the nonspatial concept of the reflex, the authors label their theory "neurogeometric."

The first six chapters are devoted to a repetitious introduction, summary, and re-summary of past work on perception and behavior in displaced visual fields. Most readers would do well to begin with Chapter Six, a "Critical Summary of Displaced Vision Studies," otherwise they will probably never get to the more interesting and original material beginning with Chapter Seven, "Experimental Foundations of Neurogeometric Theory." There the Smith brothers argue convincingly that "the temporally organized reflex model does not fit the spatially organized motions that we observe" (p. 126).

The starting point of neurogeometric theory is the assertion that the "spatial organization of motion depends on the ability of the living system to react to differences in stimulation between specific points" (p. 126). These points may lie on the same receptor surface, or on different ones, or one point may involve an effector. It is postulated that internuncial neurons subserve the function of difference-detection, with specific dendrite endings detecting specific point-differences. The cardinal point of the system, therefore, does not assert that patterns of stimulation produce organized percepts which guide patterns of behavior, but rather that patterns of stimulation produce point-differences which serve as stimuli for motor responses. In thus substituting specific difference-detecting neurons for specific stimulus-detecting neurons, neurogeometric theory represents an attempt to provide an atomistic rather than wholistic solution to the problem of organization. One important issue the theory does not deal with is the manner in which the detector neurons, in the face of the myriad excitations constantly impinging on all sensory surfaces, choose to detect just those differences relevant to the ongoing performance of the individual. Another serious omission is the lack of any clear definition of a "point" although this would be central to the theory and is a problem that has long vexed psychologists.

'Neurogeometric theory' is a brave new name for the old but important set of problems previously brought to our attention under the aegis of *Gestalttheorie.* The Smiths have, however, extended the range of these problems in an important way by their insistence on the consequences of organized stimulation for organized *behavior,* rather than for perception as such. In this sense, the title of the book, *Perception and Motion,* is something of a misnomer, since the authors systematically eschew non-behavioral aspects of perception, and stress instead the directness of the linkage between *stimulation and behavior.* Misnamed or not, the book raises an important question: do we need to assume a distinctively perceptual phase in all psychological acts, intervening between stimulation and behavior? Such a phase, if it exists, would be richer in its possibilities than overt behavior can be in actuality. In suggesting that human behavior is quite directly linked to stimulation, the Smiths may have enriched our conception of that linkage by exploring the 'neurogeometric' problem, but they may also have impoverished our conception of human behavior as founded in human experience.

The second half of the book is mainly devoted to a presentation of the Smiths' ingenious use of television as a versatile tool in psychological research. In a wide variety of manual tasks involving visual-motor coordination, the subject is given a televised view of his performing hand, rather than a direct view. This device permits numerous controlled experiments comparing different kinds of visual feedback: spatial disarrangements studied include right-left reversal, up-down inversion, angular displacement, and size-distortion.

Very full descriptions are given of the experimenters' apparatus and of the subjects' motor performance, but the reader is left very much to his own resources in trying to understand how the stimulus scene looked to the subjects, this omission being in line with the authors' theoretical bias. I found this neglect particularly trying when attempting to visualize the difference between angular displacements caused by wearing prisms and those caused by shifting the television camera angle.

The authors suggest that each motor response to visual displacements involves a fairly specific visual-motor habit. But by and large they obtained considerable positive transfer from practice with direct writing (visual input displaced, motor output normal) to compensated writing (visual input displaced, motor output similarly displaced) and *vice versa;* this result suggests at least some non-specific learning within each condition of visual displacement. In this regard, then, the picture remains unclear.

The Smiths' experiments deal with relatively brief practice periods extending over a few days, and are well-suited to estimating severity of initial disturbance and success of learning early in the course of perceptual-motor readjustment. The Smiths, however, have not produced a body of work from which can be estimated the *maximal* capacity of the human organism for readjustment to displaced visual fields; indeed, their major experimental techniques preclude protracted, uninterrupted exposure to such fields. The authors contend that their experiments indicating no difference between massed and distributed practice permit the conclusion that there is no need for studies in which *O* wears distorting spectacles for many days. But, since the total duration of their experiments does not approach the longer studies of displaced vision, and since they offer little or no data on the phenomenal appearance of distorted visual fields, there is no clearcut way of comparing their work with previous studies. These limitations vitiate the Smiths' claim to have measured the "final level of adaptation" and their conclusion that "adaptation is never complete" in displaced visual fields (p. 306).

The authors employ a useful trichotomy among types of movements: postural adjustments, transport and travel movements, and fine manipulative or contact movements. In several of their experiments, performance is analyzed into two of these components, travel and contact, and it is shown that the disturbing effects of different kinds of spatial disarrangements depend on the kind of motion involved. The authors' predictions do not always seem to follow rigorously from their theory; moreover, experiments in which predictions are not borne out are not pursued in such a way as to clarify the theory. Nevertheless, the Smiths have made a valuable contribution in insisting on the need for a subtle and analytical approach to the complex problems of visual-motor coordination, and in pioneering new experimental methods for work in this field.

New School for Social Research **Howard E. Gruber**

*Current Problems in Animal Behavior.* Edited by W. H. Thorpe and O. L. Zangwill. New York, Cambridge University Press, 1961. Pp. xiv, 424, $8.50.

This is a welcome collection of papers from a behavior discussion-group which met at Cambridge University during the years 1953-1958. The papers are divided into four sections: one on neural mechanisms with contributions by D. M. Vowels, L. Weiskrantz, and O. L. Zangwill; one on experimental studies of animal behavior with contributions by A. D. Blest, J. H. Crook and P. Marler; one on common problems in human and animal psychology with contributions by R. A. Hinde, W. H. Thorpe, M. A. Vince, D. E. Broadbent, and A. J. Watson; and one on theoretical approaches to behavior with contributions by R. L. Gregory, H. B. Barlow, and V. Cane. The individual papers range in topic from Encephalization and the Scotoma (Weiskrantz) through such items as Developmental Changes in Learning Capacity (Vince) to the Coding of Sensory Messages (Barlow). Any collection on as wide a variety of topics and from such a variety of points of view runs the danger of being a disjointed set of research reports peculiar to each contributor's very specialized interests. The danger doesn't materialize in this case. The integration of the collection can be attributed to the relatively broad outlook of the contributors themselves toward problems of behavior, to the integrative rather than specific nature of the papers, and to the unifying and synthesizing nature of the introductory comments to each major section.

Even in so restricted a topic title as "Neural Mechanisms in Insect Behavior," Vowels is concerned with relating his specific analysis to the more general idea that the invertebrate CNS is not just a simpler version of the vertebrate type and to the important general point that apparent behavioral complexity doesn't necessarily imply neural complexity. O. L. Zangwill in his introduction to the neurological section relates all three essays in the section by pointing out that a central common theme is the concern with the relation between neural mechanism and complexity of behavior.

Again the essay in the theoretical section of the book on the Brain as an Engineering Problem (Gregory) is not concerned with specific brain models, but rather with the more abstract issues of the implications of various strategies which take an engineering approach to biological systems. To illustrate some of these implications Gregory uses the problem of localization of function. Among other points his engineering approach raises the intriguing question as to whether brain stimulation and ablation studies are, in fact, a very fruitful strategy with which to approach the problems of brain function. This paper dovetails nicely with an earlier essay in the collection on "Lashley's Concept of Cerebral Mass Action" (Zangwill) in which much of the classic data on mass action is reëxamined and the suggestion made that even that classic data give little support to the principle of mass action.

There are many such provocative ideas contained in these pages. This perhaps is the best kind of result of a discussion group in which animal behaviorists, on the one hand, will critically consider ideas about the relation between Human Perception and Animal Learning—reactive inhibition and the continuity-discontinuity problem (Broadbent) and experimental psychologists, on the other hand, will participate in discussion of "Flock Organization in Birds" (Crook).

University of Minnesota HERBERT PICK

*Birth to Maturity: A Study in Psychological Development.* By JEROME KAGAN and HOWARD A. MOSS. New York, John Wiley and Sons, Inc. Pp. IX, 381.

Attempting to "review" a 381 page, complex, 30 year longitudinal study is about as difficult as trying to tell someone, in one sentence, what you learned in college. In addition, it is unfair to evaluate a longitudinal study unless the evaluator is fully conversant with the scientific necessity of accepting the inevitable "longitudinal discount" of diminishing and unrepresentative samples, obsolescent assessment devices, post hoc theoretical divinations, and fluctuating observer recordings over the years.

Faced with the unavoidable erosions of time, there is a great temptation to succumb to "hem-and-haw" disease and to try to rationalize the limitations of all longitudinal studies and to make them disappear with a wave of the theoretical wand. Kagan and Moss have faced this continuing issue squarely and have tried to make as full an account as possible of the cautions about extrapolation of the findings of this study to other populations and to development in general.

The study they have accomplished is an important one, one marked by an unusual degree of theoretical sophistication, and one rich with hypotheses and directions for new research. This, I think, is the single most vital product of any longitudinal research.

*Birth to Maturity* is a study of 89 children from 63 families aimed primarily at an assessment of ". . . the stability of selected motive-related behaviors, sources of anxiety, defensive responses, and modes of interpersonal interaction from earliest childhood through young adulthood" (p. 9). In addition, this long-range study considers the effect of maternal practices on the child's behavioral development and assesses the validity of some popular techniques in personality assessment. The areas explored range through observations of child and parent behavior, personality testing, and mental development of parent and child.

The methods employed in the study included observations of the mother and child in a variety of settings, personality testing (TAT, Rorschach, MMPI, Kuder Preference Record), assessment of mental development (Gesell Development Schedule, Merrill-Palmer Infant Test, Stanford Binet), plus mental testing of the mother and father (Otis I.Q. Test). In addition, adult assessment included a modified ink blot task, selected TAT stimuli, a self-rating inventory, conceptual sorting tasks, a tachistoscopic recognition task, the Weschsler-Bellevue Intelligence Scale, and selected measures of autonomic reactivity.

Spread throughout this extended exploration are verbatim excerpts of the raw material from which the tables were constructed and the conclusions formulated.

These are particularly valuable since they add the spice and flavor that so often are missing from such studies. For psychologists concerned with personality, development, and insight into the relation of early life to later status this is an informative study. It should be viewed, however, as the authors do— ". . . as a source of new hypotheses and not as an almanac of facts" (p. 19).

The University of Michigan — ELTON B. MCNEIL

*Inkblot Perception and Personality: The Holtzman Inkblot Technique.* By WAYNE HOLTZMAN, JOSEPH S. THORPE, JON D. SWARTZ, and E. WAYNE HERRON. Austin, Texas, University of Texas Press, 1961. Pp. vii, 417. $8.00.

This is a serious and, on the whole, successful attempt to put the interpretation of standardized inkblots upon a sound psychometric foundation. After 40 years the Rorschach is still a vehicle for the intuitive operation of clinical impression and not an instrument of testable (let alone significant) reliability or validity. Things, however, are looking up.

The senior author, Wayne Holtzman of the University of Texas and the Hogg Foundation, exhibited no false modesty in naming the technique HIT, after himself, as he deserves credit for having fathered a clinical research program of considerable scope and for having, with the help of his collaborators, carried it through to an impressive conclusion. The crucial variable in inkblot interpretations, number of responses, has been controlled by using a greater number (two alternate forms of 45 blots each) of stimuli and permitting only one response per card. Thereupon, using only slight modifications of traditional scoring categories such as Location, Form Definiteness, Form Appropriateness, Color, the HIT has demonstrated satisfactory levels of inter-judge and odd-even reliability, and alternate form reliabilities which, though modest, are far better than anything hitherto achieved with the Rorschach.

The HIT also seeks to capture formally certain aspects of verbal interpretation of inkblots that are not explicitly scored, though regularly noted by clinicians, in the Rorschach. Most important among these is the behavior which the authors designate "pathognomic verbalization" which includes fabulation, incoherence, contamination, and self-reference. Regarding this category the authors note that "more effort was expended in developing an explicit rationale and scoring system for Pathognomic Verbalization than for any other variable in the Holtzman Inkblot Technique." It is unfortunate that this effort did not include consulting a good dictionary, for there the authors would have found that what they are interested in is correctly designated *pathognomonic verbalization,* and that the word "pathognomic," though perfectly legitimate, has a meaning quite different from that which they give to it. The impression of semi-literateness that this kind of mistake, repeated dozens and dozens of times throughout the volume, leaves is not dissipated by a style that is singularly lacking in elegance or precision and that at times achieves an almost wooden incoherence. Consider: "Credit for Movement is given only when the subject voluntarily ascribes movement or potential for movement to the percept. . . . Since movement is designed to reflect the degree of movement, tension, or dynamic energy projected into the percept by the subject, it is important that concepts clearly involving tension and dynamic energy not be excluded merely because they may not be perceived in actual motion." Does a subject ascribe movement *voluntarily* (as opposed to in-voluntarily), or do the authors perhaps mean *spontaneously?* And is such movement ascribed to the *percept,* or to the object *perceived?* Or is it ascribed to neither, since the authors speak of *concepts* that "may not be perceived in actual motion?" It is hard to say which is more confused here, the epistemology or the mode of expression.

On the psychometric side, fortunately, the authors do far better that in the philosophical or stylistic. Fifteen different samples were employed in the standardization, embracing a wide developmental and diagnostic range, including pre-school and school children from the 5-year to the 11th grade level, middle class housewives and city firemen, college students, schizophrenics, depressives, and mentally retarded adults. A total of 1239 cases was employed in establishing percentile norms for the various scoring categories, providing a statistically reliable foundation for evaluating different kinds of inkblot interpretations. At long last it is good to be able to say such things as that "only one in a hundred normal adults gets a raw score of 29 or less, while 31 per cent of the chronic schizophrenics and mental retardates get a score that low on Form Appropriateness" (pp. 51-52); that "a significantly higher percentage of mental retardates than elementary school children fail to use color as a determinant. Depressed patients have significantly lower mean scores than average adults" (p. 197); that with regard to shading "a rather striking developmental trend is apparent, with college students showing the highest scores and young children the lowest," and the latter is "matched by smilarly low scores for mental retardates" (pp. 197, 200); that "college students give far more movement responses than any other group, while the five-year olds, chronic schizophrenics, and mental retardates have the lowest mean scores on this variable" (p. 201). As might be expected, college students also have the highest mean number of H responses and the highest Integration score (similar to Beck's Z), while mental retardates have the lowest H and the highest A score.

Using a centroid factor analysis on the matrix of intercorrelated scores, the authors are able to extract three clear and two less clear factors. Factor I has significant loadings on the HIT equivalents of Z, M, H, P and FC/FY. Factor II is identified by the equivalents of CF and YF, and factor III chiefly by verbalizations that are "pathognomic," anxious, or hostile. These results point to an underlying similarity of the Holtzman and Rorschach techniques that should facilitate the gradual replacement of the latter by the metrically sounder HIT. The successful use of Fisher's discriminant function with HIT scores as a technique for differentiating between normals and paranoid schizophrenics also suggests fascinating possibilities for future exploration.

After a lengthy and rather belaboredly obvious case analysis, the volume concludes with appendices on the scoring of location, popularity, and form appropriateness for the two versions of the HIT.

Marquette University

Raymond J. McCall

*Teaching Machines and Programmed Instruction.* By Edward Fry. New York, McGraw-Hill Book Company, 1963. Pp. viii, 244. $5.95.

*The Learning Process and Programmed Instruction.* By Edward J. Green. New York, Holt, Rinehart and Winston, 1962. Pp. x, 228. $5.35.

These two books have some similarities in describing the objectives, methods, and results of programmed learning and are similar in size but they differ significantly in scope. Fry gives much more description of the hardware or details of teaching machines and more about how to program. Green describes the theory of the learning process in much greater depth. Consequently, their titles are descriptive in both instances. They both bring out that the essentials of programmed learning are the analysis of content into small bits put into frames, the arrangement of these frames into a careful sequence, forcing the learner to respond to each frame, giving the learner a prompt feedback of the correctness of his response which serves as an adequate reward for learning. Usually this reward is present in 90 per cent of the frames since they have been carefully graded to allow the learner to be correct that much of the time.

Both agree that the key issue is the quality of the program, rather than the detail of the hardware. They point out that the term "teaching machine" is confusing in that the central concept of programmed instruction can be included as well or in some instances better in a book than transmitted through a machine. They both review some of the history of programmed instruction showing the early contributions of Pressey and Skinner. Both give the contrasting branching program of Crowder, but Green in particular shows a preference for the Skinner or completion type.

Both books have bibliographies. Fry's consists of 178 items with the most frequent listings being six each for Fry, Holland, and Lumsdaine. Green lists 76 items which includes ten by Skinner, seven by Green, five by Estes, and four each by Holland and Hull. This census is revealing of the emphasis in Green upon the theory of Skinner and Hull and the animal research of Estes.

Both show something of the time and effort of developing an instruction program. The teacher is presented by Fry as being in charge of the learning process presumably in an effort to allay the fear of teachers that learning programs will make them obsolete. This reassurance is not completely convincing to the reviewer. Granted at the present the abundance of students and the relative scarcity of successful programs make this a teachers' market. But when there are plenty of motivated students and plenty of good instruction programs, and when the culture changes to accept programmed instruction, the teacher's role can be reduced. There are lists of programs showing the variety of topics that have been programmed. While mathematics and more factual subjects have been the mode, the programming of instruction in psychology by Holland and Skinner shows that the less quantitative subjects can be successfully programmed as well as the impressive teaching of pigeons to play ping pong by Skinner.

Fry asserts that programmed instruction may lessen individual differences. Possibly so, but this reviewer was struck with the contrast to the apparently opposite claim made generally that a learning program allows students to proceed at their own pace which presumably would heighten individual differences. Maybe there is some of both, lessening for the slow learners and heightening individual differences for the fast with a net effect of lessening.

Each of these books is interesting and each makes a contribution beyond the relatively disconnected and incomplete anthologies which up to now have been the main book literature in this field, although there are adverse criticisms of each. They are both optimistic about the future of programmed instruction and give many examples of its use and review the literature of some accounts of its successes. This enthusiasm seems well justified by the evidence presented. It has been said that in the rapidly booming field of programmed instruction, the few experts who really know how to program are so busy programming that they are too busy to write about how to do it. The combination of these books has something to offer as they complement each other. Fry is long—too long—on the descriptions of programs and machines, but is short on learning theory. Fry's organization of his material is not completely clear. Green is long on the learning process in a way that may appeal to graduate students, but is probably too difficult to be readily understood by undergraduates. Green does not attempt to give a thorough account of sample programs and machines. Taken together both books are worth reading by serious students of programmed learning. Fry tells a person who is interested in developing a program how to do it. He advocates a team approach of a subject matter specialist with a programmer which this reviewer has found can work.

Granduate School of Business
Stanford University

THOMAS W. HARRELL

*The Human Dimension in International Relations.* By OTTO KLINEBERG. New York, Holt, Rinehart and Winston, 1964. Pp. xii, 173. $2.75 (paper).

This gem of a paperback soundly and clearly tells student and layman about the operation of human factors in international affairs. Klineberg first explains in detail how the "minds of men" function—how their motivations lead them into war, how they think in terms of ethnic and national stereotypes, how their perceptions and attitudes condition their reactions as they handle their political, economic, and social affairs. And then this international social scientist—an American now occupying a chair in the University of Paris—indicates how these basic facts and principles are being applied in a variety of ways, as in understanding public opinion about foreign affairs and in the selection of personnel for assignment overseas.

As in his other work, Otto Klineberg is foremost the disciplined psychologist. He writes with the measured judgment of the mature scholar, as when he handles the controversy as to whether public attitude precedes or follows elite decision, arguing "that the relationship is a circular one, with public opinion and the political situation mutually influencing each other" (p. 101). Klineberg is devastating when he shames the anthropologists in their unruly assertions about national characteristics by simply exhibiting the inadequacy of their standards of evidence, as "they still present us primarily with one man's judgment, one man's opinion" (p. 138). This dean of all psychologists interested in international relations shares with the experimentalist his respect for basic evidence, in allowing Jean Stoetzel's facts to refute Ruth Benedict's inaccurate characterizations of aspects of Japanese life—and he shares a high regard for methodological rigor, in making an elegant arrangement of the studies of ethnic hostility, from Katz and Braly's work in 1932 to Pettigrew's in 1958.

Our theories and methodologies allow us to research the impact of studying abroad. The psychologist interested in the utilization of psychological knowledge in international political affairs will be struck, however, by the inadequacy of our bridges from the laboratory to the phenomena of life. Professor Klineberg keeps pointing these out, as he proceeds from chapter to chapter: he fails to find evidence on the linkage as to how the "stereotypes held by Germans before World War II contributed in substantial degree to their warlike attitude and their preparations for war" (p. 41); he notes "One can only guess at how much misunderstanding between nations is due to such group-determined differences in how we perceive" (p. 95); and after describing one of Morton Deutsch's two-person games, he realizes the analogy with the international situation is by no means perfect, but "then hopes that once again the principle is clearly applicable" (p. 151). At times Klineberg laments the failure of policy-makers and international executives to make use of scientific psychology, as when he recalls that former Prime Minister Macmillan "has been quoted as saying after a conference with Khrushchev, 'We see the world differently.' This is at least a first step, but no political leader in the knowledge of this writer has ever taken the indispensable next step of trying to understand why this is so, and progressing to practical conclusions" (p. 96). But why does as eminent a scholar as Klineberg find himself guessing and hoping, when he attempts to extrapolate psychological knowledge from our experiments to the world of diplomats and statesmen? Klineberg knows the literature in depth, even though he apologizes for his *sentiment d'incompletude*. He lacks not articulation or demonstration of the basic principles. It is to the gap between fundamental knowledge and our application to which our best talents in psychology should be addressing themselves with vigor, it would seem. Perhaps then when the statesman becomes ready to utilize our work in his political decision making, the gap will not be abysmally huge, as Klineberg's book indicates is now the case.

It is interesting to detect the shift in Klineberg's emphasis, from his opening chapter on "The Minds of Men" to his closing chapter on "The Meeting of Minds." Throughout the book the author focuses on the functioning of the individual personality in a social environment. But then finally at the end of the book, Klineberg recognizes that personalities must meet—and devotes the last chapter to a discussion of how individual-oriented psychological processes must be understood within a context of interaction, that of negotiation. Perhaps in another decade, should the work in the group psychology of decision-making processes continue at its present accelerated pace, Klineberg will be able to write as elegently the companion volume he intimates is now needed, *The Group Dimensions of International Relations.*

Northwestern University HAROLD GUETZKOW

*The Psychology of Communication.* By JON EISENSON, J. JEFFERY AUER, and JOHN V. IRWIN. New York, Appleton-Century-Crofts, 1963. Pp. ix, 394. $6.50.

This volume, bearing a somewhat ambitious title, is an uneven and disjointed contribution to this rapidly growing area. Eleven chapters are by the senior author, five by Auer, three by Irwin. In addition there are two signed chapters, one by William Etkin on "Communication Among Animals," and one by Milton Valentine on "Information Theory and the Psychology of Speech." It would have been more appropriate to call the book *The Psychology of Oral Communication*, since there is no treatment of reading or writing as systems of communication, although there is considerable stress on gestures. Important contributors to the field who receive no mention, or only passing footnotes or bibliographical reference, are M. M. Lewis (error in initials M. W.), Piaget, Vigotsky, Luria, Osgood, Froeschels, N. Wood, K. de Hirsch and G. Wyatt.

One of the strongest chapters in this reviewer's opinion is Chapter 4 on "The Nervous Mechanism and Speech," which is a readable, updated account of the pertihow to program are so busy programming that they are too busy to write about how to do it. The combination of these books has something to offer as they complement each other. Fry is long—too long—on the descriptions of programs and machines, but is short on learning theory. Fry's organization of his material is not completely clear. Green is long on the learning process in a way that may appeal to graduate students, but is probably too difficult to be readily understood by undergraduates. Green does not attempt to give a thorough account of sample programs and machines. Taken together both books are worth reading by serious students of programmed learning. Fry tells a person who is interested in developing a program how to do it. He advocates a team approach of a subject matter specialist with a programmer which this reviewer has found can work.

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One of the strongest chapters in this reviewer's opinion is Chapter 4 on "The Nervous Mechanism and Speech," which is a readable, updated account of the pertinent research of Hebb and of Penfield, stressing especially the role of the reticular formation. Part VI contains three challenging, though less well documented chapters on personality and speech, which will be of special interest to clinical psychologists.

Chapters 12 and 13 comprise the treatment of developmental aspects of communication and depend largely on the chapter on "Language Development" in Carmichael's *Manual of Child Psychology*. It is regrettable that only the 1946 edition was used, as that particular chapter was greatly expanded and enriched in the 1954 edition.

Auer's chapters on "Group Communication," "Public Address," "Radio and Television," and "Psychology of Stage Fright," bear little relation to the rest of the volume. Their appeal is more to social psychologists and those in public speaking.

Noticeably lacking are any extensive treatments of stuttering, articulatory defects, foreign accent or aphasia. These are areas which most psychologists would expect to have treated systematically in a volume on *The Psychology of Communication* by people trained in the field of speech.

More serious than the sins of omission, for no 400 page book can cover everything in this broad field, are the numerous inaccuracies and distortions of fact. For example, O. Hobart Mowrer is twice indexed incorrectly. On page 22 the report of Seidl's unpublished dissertation refers only to "scores," but does not indicate what instruments were used. On p. 202 the title of the article by McCarthy should read *sex* differences, not *set* differences. Table 4 on page 215 is from McCarthy 1930, although no reference is given to the original source.

Data from McCarthy and Templin on proportions of parts of speech in 50-sentence samples of conversation are erroneously interpreted as parts of speech in vocabularies. Eisenson, failing to take age differences into account (18 vs. 36 months) implies discrepancies in regard to these tables between the two studies and concludes "Templin (1957, p. 101) finds a smaller proportion of interjections"; whereas, Templin (1957, p. 102) interpreting the same two sets of figures states, "In the current study the percentage of each of the different parts of speech is in close agreement with that reported by McCarthy."

Such inaccuracies are not what is expected in professional writing. When cursory reading of material familiar to the reviewer reveals such careless documentation, editing and interpretation, one questions the dependability of scholarship throughout the volume.

Fordham University

DOROTHEA MCCARTHY

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(The books listed here have not as yet been noted in our pages. Listing here does not, however, preclude their later review.)

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